
discussion article

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On the nature of theoretical archaeology and archaeological theory *Matthew H. Johnson*

Abstract

In this paper I want to make some general comments on the state of archaeological theory today. I argue that a full answer to the question ‘does archaeological theory exist?’ must be simultaneously ‘yes’ and ‘no’. Yes, there is, demonstrably, a discourse called archaeological theory, with concrete structures such as individuals and schools of thought more or less substantively engaged with it; no, in that the claims for a distinctive way of thinking about the world in theoretical terms specific to archaeology, to which most or even the largest group of archaeologists would willingly or knowingly subscribe, are over-stated. In particular there is a lack of correspondence between theoretical backgrounds and affiliations that are overtly cited by archaeologists, on the one hand, and, on the other, the deeper underlying assumptions and traditions that structure their work and condition its acceptance. These underlying traditions stretch from field habits to underlying paradigms or discourses. I will explore this latter point with reference to the manner in which agency theory and phenomenology have been developed in archaeology. My conclusion suggests some elements of a way forward for archaeological theory; it is striking that many of these elements have been addressed in recent issues of *Archaeological dialogues*.

Keywords

archaeological theory; theoretical archaeology; agency; phenomenology

Introduction

In this paper, I want to advance the proposition that archaeological theory both does and does not exist. Clearly there is, concretely, a practice of theory, with concrete structures such as individuals and schools of thought more or less substantively engaged with it. Articles with the word ‘theory’ in the title regularly appear in the major academic journals; substantial portions of introductory textbooks, particularly in the Anglo-American world, are devoted to discussions of theory (Renfrew and Bahn 2004; 2005; Thomas 1998). There is widespread disagreement on the subject of how, precisely, to define theory, and the extent to which ‘the data’, however conceived, are or are not theory-laden. As a practice, however, theory’s place in archaeology is

secure to the extent that its necessity can now be assumed; it no longer has to be argued through.

I start by defining theory very simply: theory is the order we choose to put facts in. Attempts to be more precise or detailed than this tend to preclude a range of viewpoints – the more precise the definition, the less it is inclusive of the range of theoretical views on offer in archaeology today, and the more it tends to prescribe or lead to a particular theoretical viewpoint. The problem is that the question ‘what is theory?’ is in part dependent on the question ‘what is archaeology?’. The interdependence of the definitions of theory and of subject terrain is apparent in other disciplines. When, for example, Terry Eagleton (1983, 1–17) attempts to define literary theory, he rapidly gets bogged down in the question of what is ‘literature’ in the first place. Debates over disciplinary definition and configuration (is archaeology a humanity or a science?) tend to be rather sterile. Further, they tend to be complicit in an explicitly or implicitly positivist definition of subject areas in which subject domains can be discretely identified and separated according to their subject matter, rather than according to a contingent historical account of how disciplines are created. Even the mildest version of social constructivism or of Foucault’s thinking (for example 1989) undercuts such a position; rather than exist in an a-priori manner, superficially independent subject domains are created at least partly through a discursive process contingent on intellectual and cultural history.

At a deeper level, I want to argue that a characterization of ‘theory’ as a discrete field which conditions and directs the questions we ask as archaeologists and the methods by which we go about answering them is at least partly illusory. It stems in part from a demand that we ‘put theory into practice’ – an expectation from the archaeological community to demonstrate that theoretical endeavour has a tangible (and demonstrably positive) impact on archaeological interpretation. This expectation is not unreasonable but it is potentially misleading. First, it tends to conspire in a division between theoretical and other forms of archaeological discourse in which ‘practice’ is deemed ‘theory-free’; second, as we shall see below, theory can rarely be lined up in such a way to demonstrate a simple cause-and-effect relationship of this kind.

There is, to put it very simply, a disjuncture between what we say we do as ‘archaeological theorists’ and what we actually do as archaeologists. This disjuncture or lack of correspondence leads very quickly into areas of great sensitivity and even anger among archaeologists, as Mark Leone has pointed out in relation to Walter Taylor and his criticisms of culture history in North America (Leone n.d.). To what extent do the musings of those who call themselves archaeological theorists present an accurate reflection of what archaeologists actually do?

I argue here for a lack of correspondence. This lack of correspondence takes several different forms. One form is the severely abbreviated nature of archaeological theory. Historically, explicit discussion of theory is a relatively recent phenomenon in the discipline. All archaeology, of course, is theoretical in some sense, and was so in the past; historical re-evaluation and revision of Renfrew’s characterization of the century before 1960 as the ‘long sleep’ of

archaeological theory is now well established (for example Schlanger 2002). Nevertheless, it remains the case that before 1960 explicit discussions of theory were few and far between in the major journals. It is easy for the English-speaking university academic to forget that this historical situation persists in many areas of archaeology, particularly in the professional/cultural resource management, and museum worlds, and in many other areas of the globe.

In any case ‘theory’ has, historically, tended to confine itself to a delimited set of topics. Where ‘new archaeology’ did openly articulate theory, it did so with reference to two areas: epistemology and questions of social reconstruction and process (see, for example, the structure of the collected papers of the new archaeology gathered together in Leone 1972). Marxist, postprocessual and related strands of archaeology added a third area, or arguably drew out into the open a third area that was always more or less latent and implicit: the reflexive and political nature of archaeology (Trigger 1980 and 1984 being landmark articles in this respect).

Expansion outside these three areas, in particular into the themes of the cultural and emotive context of archaeology, has been relatively recent. As Sarah Tarlow (2000) has pointed out, not even the ‘power view’ of early postprocessualism embraced such themes in its writings. Michael Shanks’s *Experiencing the past* (1992) drew a mixed reception; Shanks and Pearson’s *Theatre/archaeology* (2002), or Colin Renfrew’s *Figuring it out* (2003), are both recent and exceptional. One can observe of Jennifer Wallace’s *Digging the dirt. Archaeology and the Romantic imagination* (2004) that it took someone who is not an archaeologist to write such a book – in other words, it took a literary critic and historian to write such a broadly reflective book on the nature of archaeology and its meaning in the modern world.

A second issue with the nature of theory is the lack of correspondence between the formal tenets of theory and the underlying discursive assumptions at work in concrete archaeological practice. To put it more simply, very often, archaeological theorists, the author of this article included, can appear as hypocrites. The case studies offered in support of a particular theoretical position frequently do not match up to the claims made about them in the preceding theoretical excursus. This phenomenon is characteristic of the early periods of new thinking. This tension can be a productive and positive one, characteristic of the dangers of innovative thought; early polemical statements of the new archaeology were often accompanied by accounts of recycled culture history. Similarly, many early postprocessual case studies, when examined carefully, have the formal properties of processual analyses. Shanks and Tilley’s famous beer cans and Neolithic tombs (1987a) are analysed in terms of the different structural properties of the societies that produced them, and their analysis of key variables and demonstration of relationships between those variables has heavily processual undertones (see Johnson 1989). Conversely, Lewis Binford’s assertions that ‘mentalist’ explanations are unverifiable and outside the domain of science are in turn contradicted by his case studies, which implicitly have assumptions about mentalities and world views built into them (Binford 1983).

A third issue is the constructing of theoretical positions around a perceived middle or common ground. Because a middle ground is rhetorically appealing

to archaeologists as much as it is to politicians, such a strategy involves the danger of substituting rhetorical for theoretical discourse. Bruno Latour (1987) developed a useful analogy for the way scientists work, that of a military campaign. Scientists seek to win victories over their opponents, and to do so they seek to establish, maintain and extend alliances of certain kinds. In the world of archaeological theory, such alliances can take various diverse and even curious and intellectually challenging forms: VanPool and VanPool's characterization of 'the scientific nature of postprocessualism' (1999), for example. Clearly this issue of the right balance between alliance and consensus on the one hand, and the creation of a space for debate and dialogue on the other, is one of current concern; Madonna Moss's worries over Michelle Hegmon's characterization of a 'processual-plus' orthodoxy in Americanist archaeology are a case in point (Hegmon 2003; Moss 2005).

I will spend the bulk of this paper discussing a fourth issue. My suggestion is this: the relationship between explicit archaeological theory and other elements of archaeological thinking and practice has itself been under-theorized. Specifically, there exists a complex, shifting and historically particular relationship between different elements of the craft of archaeology. These elements include the very different field habits of different regional, national and thematic traditions of archaeology; the underlying discursive and/or paradigmatic assumptions made in archaeology as a whole and within different sub-fields; the social, intellectual and cultural context of archaeology, both within and outside the academy (Bourdieu 1984; 1996); the changing configuration of professional archaeology and workplace issues within the discipline; and, of course, the construction of the project of 'archaeological theory' itself.

This complex and shifting kaleidoscope of interests, contexts and discourses presents a more difficult and complex terrain of theory than simple diagrams of theory versus data often imply, or indeed the configurations suggested by more subtle diagrams such as Trigger (1989, 20) or Kristiansen (2004, 81; although, to be fair, neither author is attempting a complete map of theoretical terrain with these diagrams). It is a kaleidoscope perhaps best explored historically. I will do so through an account of two recent concerns in archaeological theory: agency theory and phenomenology. I will look historically at how these concerns came to be so pressing in current thinking (I would write 'fashionable', but this is not to trivialize their nature – Raymond Williams points out (1976, 27–28) that implicit in the aesthetic idea of fashion is an assumption of its secondary or unimportant nature). I will then look at how the tensions and issues that each raises can be mapped on to this very complex and shifting terrain.

The consequences of postprocessualism

Agency theory and phenomenology have diverse and various historical origins, and have been applied to the archaeological record in a great variety of ways. Nevertheless the concerns of both have, in part, arisen out of elements of the postprocessual critique, and specifically from problems and issues that arose from the very success of that critique.

In this respect – that is, of moving from successful critique to more problematic issues – postprocessualism as an intellectual movement in archaeology was following a well-worn path. It is a commonplace observation that new archaeology was accurate in many of the questions it raised, but that diversity and confusion ensued in the attempts to answer them (Johnson 1999, 32–33). Most obviously, new archaeology raised the question of science and the need for a scientific method. This appeal to science was well founded. Preceding archaeological methods did seem to have left archaeologists in the unenviable position of running harder and harder to stay in the same place in terms of our knowledge about the past (Clarke 1972, 3). However, new archaeology faltered in its turn to what philosophies of science might therefore be appropriate as models, in its attempt to rigorously define what such a scientific method entailed, and in providing convincing case studies of archaeological data.

One can make a parallel observation about postprocessualism, in two senses. First, postprocessual archaeologists rightly raised the question of the individual, and of understanding human intentionality and agency. Second, postprocessualists insisted on the necessity of understanding culture as meaningfully constituted, and therefore the necessity of coming to terms with the question of cultural meaning. It is striking that Michelle Hegmon's characterization of current thinking in Americanist archaeology as 'processual-plus', following on from traditions represented in statements like those of Earle, Brumfiel, Cowgill and others, takes both these concerns fully on board, but sees them as compatible and indeed integrated with a broader processual approach (Hegmon 2003; see also Brumfiel 1992; Cowgill 1993).

Many scholars, most obviously Colin Renfrew and many of the contributors to Yoffee and Sherratt's *Archaeological theory. Who sets the agenda?* (Renfrew 1982; Yoffee and Sherratt 1993), might sketch a slightly different genealogy for these two ideas. In particular, the claim can be made that an interest in the 'archaeology of mind', and more broadly an interest in cognition leading into cognitive-processual archaeology, cannot be understood solely in terms of a reaction to postprocessualism; indeed, the claim can be made that it would be arrogant for postprocessualists to make such a claim. Rather, in this view, an interest in cognition and symbolic meaning evolved independently from the 'social archaeology' of the 1970s and 1980s (Renfrew 1982). One can also point to the impact of political changes in the practice of archaeology in the United States, in particular the political challenge of the encounter with indigenous perspectives (see, for example, Varien and Wilshusen 2002). In this respect, an interest in cultural meaning and how it can be studied archaeologically might be argued to stem from a process of independent evolution in different spheres or domains of archaeological thinking. Similarly, many of the current contributors to agency theory come from either a Darwinian or another evolutionary perspective; such perspectives have been claimed to share common ground with postprocessualism, for example by VanPool and VanPool (1999).

However their history might be characterized, both these observations, forceful, convincing and straightforward enough in themselves, led on to difficult and intractable theoretical questions that rest in part on the

relationship between theory, practice and discourse. The need for a theory of agency, and to understand cultural meanings, was clear: the question was, how to do it?

Agency: theory versus practice

Agency theory, however complex in its implications, is in its essence very simple (as John Robb comments: 2004, 105; see also Johnson 2004, 241). With apologies to Jane Austen, it is a truth universally acknowledged that pots are made by people. In other words, the archaeological record is created by human beings, through a series of individual behaviours and actions. One has only to think of the limits of this observation (in dealing, for example, with hominids before *Homo sapiens sapiens*, or in contexts where ‘natural’ factors may be responsible rather than human action, in other words very early contexts and phases of the archaeological record) to realize just how wide these limits are. However, human beings do not make those pots in conditions of their own choosing. Society is never a complete free-for-all; if it were, it would not be society. People are constrained by a whole series of different structures and structural relations, some technological, some physical, some social and cultural.

One possible form of understanding agency, arguably the dominant form in current archaeological thought, is through ‘structuration theory’, a term first coined by the sociologist Anthony Giddens (1984), drawing on the work of Pierre Bourdieu (1977) and others. Giddens’s structuration theory proposes that agency and structure are dialectically related. Social agents have their own goals and select strategies to achieve those goals, but they do so within a social structure. That structure, however, is not an external given but is rather constantly coming into being. It is constantly being reproduced and renegotiated through the many small actions of individuals and groups. For both Giddens and Bourdieu, ideas of habit, everyday action and so on are important in showing how the structure is over-determined; in other words, there is a tendency for the agents to replicate the structure rather than transform it.

What is, then, a very simple observation for the archaeological theorist leads on to conceptual issues of great complexity. First, there is a question of how one conceptualizes the relative strength and configuration of agency and structure. This depends on historically specific questions and cultural factors that vary from society to society, such as the strength of the idea of the individual, or the nature and structure of power in that society. The Giddens–Bourdieu model, it has been noted, has a decidedly cross-cultural feel to it. Giddens’s highly reflexive social actors, choosing their goals, reflecting on the success or otherwise of their chosen strategy, may be characteristic of modernity. Adam Smith (2004) notes further that the ‘essential archaeological subject’, far from being historically particular and culturally variable, as postprocessual accounts might claim in polemical statements, tends rather to be inscribed along axes of class, race and gender regardless of particular context. I have argued elsewhere for a ‘historicity of agency’ – that is, for the possibility that agents are constructed differently in different cultures and historical situations, and that the archaeological record can be used to

explore these constructions and the way they are materialized in, for example, architecture and landscape (Johnson 2000). Second, feminists have pointed out that the construction of a gender-neutral model of agency is problematic (Gero 2000; Moore 2000, 259). Third, the conception of structure in any model of agency bears extended analysis and comment; far from physical, social and cultural structures being separate spheres, they merge and combine and actively constitute each other, for example in the social constitution of technology (Bruno Latour's (2000) examination of the social relations implicit in the artefact of the Berlin key is an obvious and elegant example). If structures need more extended analysis, so do agents. It is possible that artefacts themselves can be viewed as agents in some sense (Gell 1998), hence recent discussion of artefact biographies (Jones 2003). In ethnoarchaeological study, even the long-dead ancestors themselves have been seen as active agents in their own right (Sillar 2004).

All these points are both important and deserving of further theoretical and empirical investigation, and they lead one away from a simple model of agency to a complex and difficult one. However, they are only part of the issue. What makes agency theory particularly difficult to grasp, in my view, is not its intrinsic theoretical properties, not the conceptual refinement of concepts like habitus, structuration or action. Rather, it is that a serious consideration of agency conflicts directly with virtually everything else that we do as archaeologists. The whole organizational, practical and conceptual apparatus of 'bread-and-butter' archaeology bears down upon the construction of agency and grinds it into dust under its weight.

The overwhelming majority of archaeologists continue to divide the past and its material remains into cultures, phases and types. All three of these words appear commonsensical, to the extent that they appear to be simple and jargon-free. However, all three are anything but commonsensical. In particular, all three militate against the visibility of agency in the archaeological record, in particular through their construction of that 'record' in terms of entities characterized by similarity rather than variability – in other words, in terms of entities where individual agency is less rather than more immediately apparent.

An archaeological 'culture' is still characteristically defined in terms of similarity, whether that similarity is in terms of single artefacts or artefact classes, or is polythetic (Childe's traits, recurring together again and again on sites within a bounded area). Similarly, archaeologists construct their perception and language of chronological phases around similarity rather than variability. This construction runs from the most basic phasing and sequencing of sites, based on principles of stratigraphy, in which major moments of change are characterized as 'horizons' (Lucas 2001, 47–51, discusses the relationship between stratigraphy and Americanist culture history). The language of types, whether constructed in formal typologies or used in a much looser sense, is the language of similarity. One can see this similarity as being the manifestation of a common mental template, or of an adaptive response – it matters not. One can observe of one of the classic case studies in archaeological interpretation – the Mousterian debate – that underneath the overt disagreements over the appropriate theoretical

apparatus to use in its interpretation is a language and classification based on types. The interpretation of Bordes's cumulative diagram might be disputed – adaptation? culture? dating? – and even the nature of the cumulative diagram itself might be questioned, but these debates overlie an unquestioned acceptance of the definition of 'type' and of the classification of tool types themselves.

This conceptual language and apparatus exists at a deeper level than mere explicit theorizing. As any ethnographer will observe of a culture, that which is foundational at the deepest level of that culture's mental world will be that which is rarely if ever overtly articulated or stated. So it is with the discursive construction of cultures, phases and types. It is very striking that despite the constant to-and-fro of theoretical critique over these three concepts, they continue to structure and be foundational to much of the everyday observational language of archaeologists. Their strength, and the way they survive decades of cogent critique, battered and bloodied yet unbowed, should tell us something important about just how central they are to archaeology's identity as a discipline. To set up a recent sociological theory against the strength of such an inert mass is truly to set up an unequal contest.

It is not only agency theory which finds itself grating against this solid mass of disciplinary assumptions. Darwinist archaeologists of the Dunnell tradition have noted in parallel that adoption of Darwinist selection as a guiding principle for archaeological interpretation will necessitate the abandonment of such a conceptual language and of the mode of thinking in norms, types and aggregates that goes with it (Maschner 1996; Shennan 2002); one might add that this difficulty, as much as the alleged conceptual limitations of Darwinist archaeology itself, has limited its appeal (Schiffer 1996).

At another basic level, agency theory is no match for one of the existing structures of archaeological discourse: the site report, and the division of labour and other relations that lie behind the report. The structure and organization of the basic site report obscures agency by its very organization. This point has been made by Janet Spector (1993) in her feminist analysis of a single artefact from a Dakota site, and in a different way by John Barrett (2005). Barrett points out that although postprocessual archaeology has presented new theoretical understandings, the older categories of field practice and classification of archaeological material still survive. For example, he argues, recent arguments over the Neolithic of Britain and north-west Europe have revolved around the competition of 'economic' and 'social' explanations. Yet these terms, and the assumptions of economic and social as discrete categories, are in his view constructions of existing practices such as the organization of material within the conventional site report. Thus, in favouring 'social' over 'economic' explanations, scholars run the danger of reinscribing positivist understandings of the Neolithic even as they seek to question them.

Again, an exception proves the rule. North American historical archaeologists have recently analysed and published finds by house lot, rather than by artefact type, in contrast to their British and European counterparts; the classic example is the Five Points excavations in New York, where, as a result of this strategy and its combination with identification through the

documents of the names and ethnic affiliations of particular inhabitants, a meaningful understanding of particular households could be made (Milne and Crabtree 2001; and others in the same volume). But however remarkable such findings are, they must be viewed as exceptional, in the nature of the attendant documentary history and the richness of the urban archaeology involved.

All these factors, then, conspire against the ‘visibility’ of human agency in the past; they do not simply make women invisible as feminist scholars have noted, but all (engendered) human beings. This was put most eloquently by Ruth Tringham in her account of how the crushing weight of her whole disciplinary training lay behind her reluctance to envisage the very simple activities of ‘people leading cows to pasture and gossiping around the household chores’ and reconstructions of prehistory ‘as really human entities with a social, political, ideological, and economic life’ (Tringham 1991, 93 and 94; see also Johnson 2007, chapter five).

A particularly revealing aspect to the agency debate is the question of whether material things have agency. A series of powerful and convincing studies have explored the notion that objects and landscapes have identities, and in particular ‘biographies’, of their own, stemming from Gell’s work on object biographies (Gell 1998; see, for example, Jones 2003). Even landscapes have been said to have their own biographies (Pollard and Reynolds 2002). This work is interesting and fruitful, not least because it leads archaeologists to question apparently commonsensical notions of personhood. Clearly, though, objects and landscapes do not have agency in the sense outlined by Giddens and Bourdieu (Meskell 2004). What is interesting in this context is the strength of the temptation to assert that they do. It leads the archaeologist into interesting and productive territory, to engage with issues of materiality. It is difficult to avoid the suspicion, however, that its strengths are in its rhetorical appeal to the centrality of material culture to shaping identity, rather than in its coherence as an intellectual position.

To summarize, then: what drives and characterizes debates over agency is only partly a matter of ‘theory’. The reasons archaeologists think the way they do about human agency have as much to do with a much wider set of factors. The relationship, then, between overt theorizing and what archaeologists actually do has been poorly understood.

Phenomenology and the discontents of Romanticism

The second theme isolated by the postprocessual critique was the importance of meaning, and of material culture as meaningfully constituted. Even if it was always set up in opposition to a straw person, the critique idea of culture as a purely or even dominantly adaptive construct, nothing more than an adaptation to an environment, was largely accepted across much of archaeological discourse (though not all; as Kristiansen notes, following Schiffer, different communities continue to ‘redline’ the understandings of other communities: Kristiansen 2004, 77; Schiffer 2000).

The question thus arose: how do archaeologists get at meaning? Or, to put it in a longer fashion, what methods might be proposed for the understanding of cultural meanings in the past that are rigorous, and can be evaluated

against the evidence? The construction of a neutral and value-free middle-range theory might be open to critique, even a doomed project, and it was easy enough to demonstrate that the formal ‘testing’ of data against theory proclaimed by new archaeology almost never actually happened. However, this recognition of failure did not absolve archaeologists from the insistence that evaluation of theories about the past, the proposal that this understanding is more coherent and convincing than that one, was a necessary and central element of our discipline.

The absence of an immediate or obvious answer to this question of method laid postprocessualists open to the charge of an implicit relativism in the view of their critics, a charge that persists to this day (Kohl 2004, 21). However much the reply was made that the accusation of relativism was a vulgarization of the issues, or even straightforward misrepresentation (see Lampeter Archaeology Workshop 1997; and replies in that volume of *Archaeological dialogues*), the 1980s and 1990s might be characterized historically as an exploration of different possible methods for the empirical study of cultural meaning. Archaeologists rummaged through the toolbox of the social sciences for appropriate methods, much as they had earlier rummaged through the toolbox of the natural sciences for guidance on epistemology and testing.

One of the first moves made was to structuralism and post-structuralism. Perhaps material culture was like a text, and perhaps therefore the language of material culture could be ‘read’ in an analogous way using the methods developed by students of language. Such a possibility opened up a series of fertile avenues (Tilley 1990; Moore 1985). I suggest that much of the power of the idea of material-culture-as-text lay in its pedagogical and heuristic value. Theoretical points about multivocality and multiple meanings were not easy to grasp in the abstract, but any short reflection on the use of language gave the student a ready model to understand and apprehend how different meanings can be generated from a simple set of rules, or how meanings can change through time with different readings and rereadings. It also gave the student an idea of how individuals learnt about the world around them – for example, I discussed the complex arrangements of space in medieval castles in terms of a common code or language that could be ‘read’ by a medieval person, but which in each case was variable and could be read differently, like a text (Johnson 2002, 69–71). In other words, material-culture-as-text worked and continues to work as an easily understood metaphor, a convenient way into understanding an issue or piece of evidence. Problems and issues attendant upon its use as anything more than a productive metaphor – that is, as a systematic application to material culture – are, however, now well known (Hodder and Hutson 2003, 59–68).

It is no coincidence that as intellectual problems arose with structural models and interest in them waned, so interest in phenomenological approaches grew. Phenomenology can be defined as the study of lived human experience, and the conditions that make experience possible (Tilley 1994; 2004a; Thomas 1996; 2004). Its origins go back to Continental philosophy and aesthetics between the wars, in particular the work of the German philosophers Heidegger, Husserl, Gadamer (1989), Benjamin and

others. There is a certain symmetry between the concerns of structural and phenomenological models: the one largely (though far from exclusively) Gallic, the other Germanic (with the obvious exception of Merleau-Ponty); the one taking delight in structure and play, the other rooted in everyday activity and the soil.

I am arguing, then, that the turn to phenomenology can be seen historically as the next stage in the struggle to understand past cultural meanings. This turn can be seen perhaps most explicitly in Gosden, who published *Social being and time* (1994) at precisely this moment of a turn away from the early postprocessual preference for linguistic models. However, this is not a full explanation of why phenomenology is now so firmly in fashion, and why in particular it has become so characteristic of a particular area of archaeological discourse, namely British prehistory. Currently the application of phenomenology to archaeology, and in particular the study of landscape, has become a highly distinctive and popular activity, to the extent that articles refer to the ‘British phenomenological tradition’ (Robin 2006). It is associated in particular with the work of prehistorians, especially of the British Neolithic and Bronze Age; I know of only one attempt to apply phenomenology formally to a historic landscape, and that hedged around with qualifications (Corcos 2001). I suggest that, to understand fully the reasons for this fashion in this particular area, one has to understand not just the terrain covered by an abbreviated notion of ‘theory’, but also the nature of the British archaeological record, and of the history of field practices and habits in this area.

In particular I suggest that whatever the formal affiliations and derivations of its theory, the underlying discourse of British phenomenology is structured and preconditioned by a prevailing and underlying discourse of Romanticism. This discourse has been so successful in embedding its assumptions into the taken-for-granted of field practice and interpretation that its presence is scarcely noted or acknowledged. (Andrew Fleming hints at this observation when he asks, ‘in what sense does this kind of “phenomenology” represent an advance on the advance of William Stukeley?’ – ironically, the connections and intellectual cross-fertilization between the 18th-century antiquarian Stukeley and the Romantic poet Wordsworth were very close (Fleming 2005, 931; Wallace 2004, 30–44)).

To summarize arguments discussed at greater length in Johnson (2007), Romanticism succeeded in embedding a series of very deep assumptions and judgements into British intellectual and cultural life. These included:

- a stress on bodily activity and in particular walking (Wordsworth tramping across the Lakeland fells);
- a discourse of ancestry;
- a stress on the gaze and the distant view, particularly from above;
- a certain gender-blindness, in which the gaze of the male poet took everything in within his all-encompassing and roving eye, and in which sensation was elevated above bodily experience;
- most fundamentally, a latent but all-pervading empiricism. The means of translating observations into literature was to stand on an elevated point

on the fells and to gather up what was in one's heart – and it became a poem.

Conceived of as a field method, this could as equally well be a description of much of the field method characteristic of many examples of British phenomenology (with the possible exception of a discourse of ancestry; see below). I am not arguing here that writers such as Julian Thomas and Chris Tilley consciously draw their inspiration from Wordsworthian Romanticism; their discussions are based on close and critical readings of the phenomenological literature and their citation of that literature as their intellectual inspiration is a sincere one. However, this does not mean that the end point of such discussions, when conceived of as a field practice and particularly when popularly applied by a much wider 'second generation' of writers, does not conform to the assumptions and judgements set out above.

How is it that this constellation of values that came into being at the end of the 18th century could be translated so uncritically and with such a lack of self-awareness into archaeological discourse two centuries later? One element in this translation is that Romanticism and phenomenology are themselves intellectually related. A critical Romanticism emerged in the 19th century, partly in response to some of the difficulties of the Romantic project as conceived of above, and it was this critical Romanticism that was foundational in the project of phenomenology itself. So Romanticism and phenomenology are not unrelated; they are, to oversimplify, intellectual cousins. However, other reasons, again, are to be found in the attenuated conception of 'theory' and its relations with discourse and field practice, and indeed with the nature of the archaeological record itself.

First, there is the nature of the archaeology to be found in the British landscape. Both early antiquarians and modern archaeologists can walk across and between prehistoric monuments, most obviously those of the Wessex chalk plains and downlands. Where Flannery's fictional Real Mesoamerican Archaeologist only stopped to record ancient mounds when he had to put his Jeep into low gear to get over them (Flannery 1976) the scale of the British landscape is such that it can (should?) be appreciated on foot, by walking across it. A particular subsequent history of the British landscape (in particular an early history of enclosure and consequently the preservation of humps and bumps under pasture, where they can be appreciated by the roving eye) accentuated this nature. So there is a powerful combination: the force of Romantic discourse is multiplied by the particular nature of the archaeological record in much of the British Isles (for example, the rolling downlands around Stonehenge and Avebury, or the Welsh hills).

Second, the Romantic vision was deeply inscribed into the early foundational history of British field archaeology through its characteristic tools: the map, the aerial photograph and the hachured plan. Though their origins were as old as antiquarianism itself, all these techniques were developed in the course of 20th-century field archaeology, in particular in the work of O.G.S. Crawford (Crawford 1928; 1953). The map, aerial photo and plan, however, also left important lacunae in field study. As Mark Edmonds has pointed out, one gets little sense from them of how the landscape is viewed at a human scale (Edmonds 1999, 162). There are, then, some very

simple and very fruitful observations to be made about what one sees from various points, about the bodily experience of moving from one place to the other, about intervisibility, and so on. All these aspects are interesting and fruitful observations, given a pre-existing discourse that tended to push such observations into the background. Further, they intersect with the concerns of phenomenology in the search for an understanding of lived experience. Phenomenology, then, did not just dovetail neatly with the nature of the archaeological record, it offered an obvious complement to some of the existing methods of apprehending and understanding it.

I am arguing, then, that when first proposed, the ideas of phenomenology fell on very fertile ground. First, they resonated with existing and very deep-seated intellectual predispositions. Second, they ‘made sense’ in terms of being obviously applicable to the British prehistoric landscape. Third, they fitted in with existing patterns of field practice – the Sunday field trip to local sites, the tramp across the landscape, the empirically informed but informal and inductive discussion of what had been observed over a pint in the local pub. To repeat, Tilley, Thomas and others cite Heidegger, Husserl, Merleau-Ponty and so on, and consciously base their work on a critical appreciation of those authors. In particular, they insist that phenomenology is about problematizing experience, not affirming its unity in a Wordsworthian sense or in a way that assumes or argues for a unity of human subjectivity (see, for example, Thomas’s reply to Brück (2005) in *Archaeological dialogues*). However, the academic work of individuals is never produced in an intellectual and cultural vacuum and cannot be evaluated as if one were hermetically sealed off from its wider adoption and intellectual currency. The observations of phenomenologists on the prehistoric landscape ‘make sense’ – gain intellectual currency and acceptance – within a structure of discourse and archaeological field practice that is characteristic of English Romanticism and indeed of English empiricism, the empiricism here being a belief that the data speak for themselves without the benefit of intervening theory – just walk across the fields with the wind in your hair and you just *know*.

All these reasons and more suggest why it is that phenomenology has been so popular. However, this congruence of certain field and theoretical concerns does not mean that attempts to understand the landscape on a human scale are formally dependent on a deep understanding of the theory of phenomenology. For example, Bradley (2000) (termed ‘postprocessual-lite’ by Kristiansen (2004, 86)) discusses experience, intervisibility, and a string of associated concepts without needing to resort to a more formal or deeper reading of phenomenological theory. These cases might be cited as examples where it is difficult to specify what, concretely, ‘theory’ has added to a given interpretation, but where there is nevertheless a close and extremely valuable and productive relationship – the work of Bradley and others unfolds within an intellectual environment that is made more receptive to such ideas by more formal and ‘high-powered’ discussions of theory in other texts.

Criticisms of phenomenology as an approach have been various, and have been treated before in *Archaeological dialogues* (Brück 2005). The suggestion I am making here is that a series of well-worn criticisms of Romanticism, in particular from feminist and Marxist scholars (for example

Todd 1988; McGann 1984; see discussion in Johnson 2007), can equally well be recycled here into a critique of phenomenology. Critics, most notably Raymond Williams, showed how Romanticism constructed ideas of ‘the rural’ and of ‘Nature’ as something timeless and unchanging, whereas in fact they were shifting and contingent reflections of ideas of urban and metropolitan development; English Romanticism is in this sense a reaction to and artefact of the Industrial Revolution (Williams 1973), through Continental Romanticism.

It is important to be even-handed here and point out that if those using phenomenology have not always been as reflexive as they might be, this can also be said of phenomenology’s critics. Thus Fleming (2005), in response to phenomenology’s critique of Cartesian space, points out that traditional landscape archaeology is not simply a downward application of the theories of the gaze developed in the Renaissance, but has also been hammered out through the detailed craft of field observation and interpretation – sharp eyes and muddy boots. Fleming is accurate in this criticism, but is not justified in the implication that this makes the theoretical apparatus of ‘fieldcraft’ any less theoretical. Traditional landscape archaeologists may come back from their day in the field with their cheeks rosy with windburn and their coats spattered with mud, but this does not mean they have been doing archaeology devoid of theory.

Conclusion

Does archaeological theory exist? Yes. Can it be characterized as a distinctive body of knowledge, to the extent that changes in theory can be described autonomously, or simply through theory’s interaction with data? No. A whole series of other factors intervenes and melds with theoretical discourse as narrowly conceived. Further, the way in which these other factors do so is dependent on the particular context of interpretation. What is a natural and commonsensical, indeed ‘pragmatic’ or ‘atheoretical’, way of proceeding is very different for the respective students of, say, Palaeolithic stone assemblages, Roman fibula typologies, Maya art and architecture, and medieval churches. When Alison Wylie wrote that ‘the question of what epistemic stance is appropriate . . . should be settled locally, in the light of what we have come to know about specific subject matters and about the resources we have for their investigation’ (1992, 35), she could have broadened her observation from the question of epistemic stance to that of theory in general.

I have given two case studies where I have tried to trace a very complex and shifting relationship between the development of archaeological theory, dependence on certain underlying models and assumptions that one might characterize as discourse, and the particularities of their application to our understanding of the archaeological record in certain contexts. Our understanding of the playing-out of these relationships is quite practised in the history of archaeology, but that does not mean that we have not always been as reflexive as we might have been in thinking about theory in the recent past and the present.

This lack of reflexivity does not simply result in a lack of correspondence between what archaeologists say and what archaeologists actually do, as

noted in the introduction to this paper. It also results in an impoverishment of theoretical appreciation and evaluation of other people's arguments, particularly when those arguments stem from questions arising in an area and period other than one's own. It was R.G. Collingwood (1946) who famously asserted that philosophical positions had to be understood in terms of the questions that were uppermost in people's minds, but the implications of this very simple point have not always been followed through.

In concentrating on agency theory and phenomenology, I have selected fairly delimited examples to pursue the wider theoretical point. However, similar comments might be made on a broader canvas. For example, the opening commentary for the SAA session made the point that however much theorists make an honest and sustained attempt to move debates on, much discussion reverts to the processual/postprocessual divide, however sincerely this attempt is made. The debate between Kristiansen and Robb is a case in point – neither party is seeking out factional division, but division ensues nevertheless (Kristiansen 2004; Robb 2004). Is this because either or both are bad archaeologists, or being perverse or blinkered? Or is it rather that the theoretical fault-lines here are conditioned by much deeper and more powerful factors? I suggest that part of the reason this divide is so enduring, keeps coming back however much all parties make the attempt to move on, is that its underlying parameters refer to two of the most powerful cultural demons of the modern West: an unbridled science on the one hand, and a disabling relativism on the other. Western intellectuals in the critical social sciences feel embattled and outflanked on these two sides. In particular, it is not difficult to see behind the alarm at postpositivist thought the appalled reaction of intellectuals particularly (though far from exclusively) in universities in the United States to the sustained assault on scientific thinking of the Republican right and creationist and other groups. Nuanced and carefully qualified position statements are no match for such powerful fears, on both sides.

Clearly much of the language I have used in attempting to understand these relationships has been borrowed from Michel Foucault, and his understanding of the properties of discourse and characterization of discursive formations. Foucault's work is tremendously useful here, as the metaphors he uses of 'geology' and indeed 'archaeology' offer a way into characterizing levels of understanding of the world that are at a deep discursive level, underlying but nevertheless conditioning theoretical structures that lie closer to the surface. However, I would hesitate beyond characterizing the use of such terms as more than an extended metaphor; the language of paradigms might be held to be as applicable (Fuller 2000). Theory may, then, be difficult to define, to be a slippery creature, and to be frustrating in its inability to provide a one-to-one match between this theory and that better interpretation of the archaeological record. It may have a complex relationship with underlying discourses that is difficult to delineate and define. None of these points detracts from the absolutely central role of theory in elevating archaeology from the mindless collection of old junk to something a little more intellectually worthwhile. 'Theory versus data' is not a zero-sum game; theory's importance and relevance, in this sense, is not something that is somehow diminished by

an affirmation of the importance of the data in evaluating archaeological arguments. Rather, it is increased.

Archaeology stakes its claim to be a responsible and intellectually rigorous discipline on its ability to produce convincing accounts of the past, and in particular on its ability to evaluate and give a meaningful and empirically informed response to some of the big questions of human existence in that past. In this respect, I have learnt from Adam Smith's (2004) discussion of 'The end of the essential archaeological subject' in *Archaeological dialogues* and find myself challenged to reassert the ability of archaeology to do something more than simply produce particular narratives and legitimate sectional interests. Most archaeologists would, moreover, go further, to varying degrees, and cite the active role of those accounts in providing a foundational history of humanity in the present. The role of 'theory' in assembling and coordinating archaeologists' findings in a manner that addresses these questions and foundational histories is beyond debate. The project of understanding the precise parameters of the role of theory has barely begun.

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Is archaeology a science, an art or a collection of individual experiments ...? Anick Coudart

'Has theoretical archaeology ever developed into archaeological theory?' That was the question posed by Michael Dietler and Liv Nilsson Stutz, which the British Matthew Johnson has courageously agreed to answer. Before commenting on his answer and trying, in turn, to answer this question, I strongly recommend the non-British reader to look at the brief synthesis on British phenomenological archaeology written by Joanna Brück (2005). Coming from the innermost postprocessual circle of the Department of

Archaeology of Cambridge University, Brück clarifies the issues and places them in perspective.

Her work is also an opportunity to recount the development of the postprocessual archaeology since its initiator, Ian Hodder (who had to break with the scientific tradition of his father), opened the debate. We have every reason to be grateful to Brück for saving us from having to read a large number of perfectly forgettable papers and books (even though several of the references she gives are far from forgettable).

Theoretical *archaeology* and archaeological *theory*

In accordance with the respect a speaker of the British language has for his or her interlocutor ('It is, isn't it?'), Johnson respects the freedom of choice of the readers he is talking to. He does not force his opinion upon his readers, but gives them two answers: 'yes' and 'no'. Bravo! However, there are also two distinct subjects here: theoretical archaeology and archaeological theory.

The existence of theoretical archaeology is beyond doubt. It exists, even if it uses concepts, theories and ideas borrowed from other disciplines. It is a field of enquiry that is practised by numerous individuals, grouped in different schools of thought (as Johnson mentions), and generating numerous books for publishers' catalogues and so on. If we look at the list of archaeology books published by Cambridge University Press during the last three years, a third of them are described as theoretical or with a theoretical dimension (at Routledge it is 20%). Whether practical archaeology (including fieldwork, description and analysis) or theoretical archaeology is more prestigious than the other is a non-issue as archaeology is a single *chaîne opératoire* that goes from data acquisition to data interpretation according to a specific procedure linking both.

However, the fact that theoretical archaeology exists is used by Johnson to also answer 'yes' to another question: 'is there any archaeological theory?' This shift is a little puzzling and disturbing to me, as it seems that two epistemologically different (and independent) things are – notwithstanding their dissimilarity – conflated: theoretical archaeology and archaeological theory.

Is archaeology a science, a set of principals or an art?

'Does a body of theory, that is distinctively archaeological, actually exist?' The question raised by Dietler and Nilsson Stutz posits the problem of the status of archaeology.

Is archaeology a science (like physics, biology, geography, sociology, social anthropology and so on)? Epistemologically speaking, scientific disciplines produce theories (plural) and not a single disciplinary theory. There are mathematical theories, biological theories, anthropological theories and so on (for example set theory, systems theory, theory of relativity, quantum theory, theory of evolution, theories about the incest taboo and about kinship systems, structuralist theories, Marxist theories, etc.).

In this context, a scientific theory is a representation of the 'essence' of the reality, rather than the reality itself, a representation that was produced by applying a formal procedure to data (observation – construction of a

conceptual model – interpretation – verification/prediction based on new observations) in order to generate general knowledge – that is, knowledge that extends beyond the particular instances from which the theory has been derived. In this procedure, the theory is thus directly tied to the observations.

But using the expression ‘archaeological theory’ in the singular means explicitly that the question here is to know neither whether archaeology is a science nor whether archaeologists are able to derive theories, rules and laws like mathematicians, physicists, biologists, social anthropologists and historians (and ‘new archaeologists’ if they had had their way).

Archaeological theory in the singular can have two meanings:

- 1 either such a theory is a set of principles leading to archaeological practices or ethics, much like Bolshevism (the revolutionary theory of Lenin) also designates the praxis of the Bolshevik Party;
- 2 or theory is a discourse that describes the specificities of a particular domain (like art theory or aesthetics, literary theory that describes the stylistics and the morphology of literary works and/or analyses those works).

The definition given by Johnson – ‘theory is the order we choose to put facts in’ (p. 118) – explicitly refers to the second kind of theory; archaeology is thus in his opinion not a science, but one of the (liberal) arts. In that case, archaeological theory would have the same form and status as literary theory.

But archaeological theory in the first sense also exists. Recently, some books have been published or republished on archaeological ethics, which come close to this kind of approach (see, for instance, Vitelli and Colwell-Chanthaphonh (2006) or Scarre and Scarre (2006)).

May I also add that the discourse of archaeologists and the expectations of society, as relayed by the demands of politics, are often found closely associated (to the great discomfort of the archaeologists involved). Our discipline has provided us with a wealth of data about different ‘others’. Throughout history, such building blocks have been interpreted in different ways by particular individuals or groups of people, in the light of their conceptions of their present and/or their future. They have been used in the negotiation of identity in ways that are often far from clear to us. In these negotiations we observe complex interactions between those tendencies that accentuate similarities between people and those that accentuate differences. For some time, archaeologists have thus faced a heavy responsibility, whenever their contributions were solicited in support of various national identities or in order to justify violent political confrontations with very questionable motives but unquestionable risks of excess. How archaeology has been employed is not clear; that it has been used is crystal clear. There is no reason to assume that it will not be used again.

Archaeology as a personal exploration of the past . . .

As presented by Johnson, ‘archaeological theory’ in Great Britain seems to be more an exploration (often in a very personal way) of the past, using concepts, notions and ideas borrowed from other disciplines, or using philosophical

views more or less (or sometimes not at all) related to archaeology, or even based on personal, emotional experience.

Although I can see a link between the phenomenology of Merleau-Ponty and landscapes ('as a lived experience'), I must confess that I see no connection, but rather a contradiction, between phenomenology (particularly Heidegger's) and the study of the past – a past as an object to be explored in a time that no longer exists (even if the present and the future are always the heirs of the past).

Johnson suggests very intelligently that the success of phenomenology in British archaeology is due to 'a prevailing and underlying discourse of Romanticism' (p. 127). I would add that phenomenology, reduced to a minimum as it is in Tilley (1994; 2004a; 2004b) – e.g. a crucial bodily movement through space that provides human beings with a specific way of viewing the world (a position that concerns phenomenology as well as the New Age philosophy) – could only be successful among those young British fellows who have grown up in a repressive corporal school system and were educated in accordance with the very English puritan morality (not to say inhibited sexuality). But when Christopher Tilley (2004b, 201) writes 'we and people of the past share *carnal* bodies' (my italics), he seems to forget or to ignore that a body is not only a physical, but also a social and cultural, construction set in a specific historical time.

As Joanna Brück (2005) writes, considering that the 'Cartesian "gaze" inherent in the production of archaeological maps and plans ... has not been fully exorcized', British phenomenological archaeologists went to the point of renouncing the use of virtual reality modelling and geographic information systems for their continuum with the objectivist and Cartesian model of space. As for Julian Thomas (1993a; 1993b; 1996; 2004), he draws on phenomenology to develop a critique of Cartesian positivism, denouncing a series of conceptual dualisms such as 'mind–body', 'culture–nature', 'self–other', 'individual–social', 'subject–object' and so on. In doing so, he rediscovers that 'subject' and 'object' or 'self' and 'other' cannot be opposed, as each is part of the other. He also rediscovers that the world is revealed to us historically. But, amazingly, he forgets or ignores that, whatever their culture or their history, human beings can only make sense of 'things' if they have recourse to a dualist way of thinking. It is because conceptually they built their world according to a system of binary relations that human beings can articulate phenomena and give meaning to things (Héritier 1996); duality is a kind of universal syntax, of which the elements are found in the symbolic repertoire of every culture. However, meaning does not reside in the content of the conceptual categories (indeed, what is 'high?') but in the dynamic articulation of their conceptual opposition – 'high' does not make sense without 'low'. Trying to define absolute 'hot' or 'cold' is a vain pursuit. The most immediate, the oldest and the most widely shared of these relationships is probably that between the sexes, which leads to the relation 'identical–different' as well as 'feminine–masculine'. But does that mean that conceptual representations and lived realities may be confounded? Does it mean that when such categories are conceptually opposed, they are irreconcilable in real life and cannot be related?

To debate such dual categories as if they were autonomous and causal entities has never produced anything. A famous Western categorical duality is the ‘nature/culture’ opposition. According to Philippe Descola (1996a; 1996b; 2003; 2005), Western naturalist cosmologies since Plato and Aristotle lead us to believe that nature exists or, in other words, that certain entities owe their existence and their development to a principle which is alien to human will. But what are ‘the objective justifications for affirming that human beings form a community of beings which is entirely distinct from the other biological and abiotic components of the environment’ (Descola 1996b, 65)?

I am not sure that phenomenology is the best way to answer the question. As Johnson says, the congruence of certain field and theoretical concerns does not mean that attempts to understand the landscape on a human scale are dependent on an understanding of phenomenology. Phenomenology is a little more complex than ‘the study of lived human experience’ (p. 126; as in Thomas (1996; 2004)). Above all, phenomenological approaches are numerous and various in form. Apart from Husserl (phenomenology as a science able to reconcile the self with the world, the subject with the object), Heidegger (whose fundamental ontology radically distinguishes the being from the phenomena: *Sein/Dasein*), Sartre (awareness is nothing because every reality, including the self, is external to it, but this ‘nothing’ is everything because it is awareness of every object), Levinas (who considered ethics towards others as first philosophy) and Merleau-Ponty (who highlights the dialectic between expressed meaning and meaning that, through our behaviour, is in ourselves and emerges in the things of the world), there are also a number of philosophers who appeared in the 1980s (particularly in France), including Michel Henry, Paul Ricoeur, Marc Richir, Jean-Luc Marion and so on, not forgetting Jacques Derrida, who recommended the famous ‘deconstruction’ (in shattering the very logic of the sign) of both ‘expression’ (*Ausdruck*) and ‘indication’ (*Anzeichen*). In this respect I must confess that I am full of admiration for my American and English colleagues who have been able to understand Derrida’s writings without being part of his own academic ‘court’. I personally do not understand Derrida’s writings, in spite of my training in philosophy.

Archaeology as a global market

Johnson declares, ‘Attempts to be more precise . . . tend to preclude a range of viewpoints – the more precise the definition, the less it is inclusive of the range of theoretical views *on offer* in archaeology today’ (p. 118; my italics).

Are we here practising the venerable British open-mindedness or are we in a global market where archaeological theoretical views are just goods and archaeology is a stock market? It is a little as if Johnson were the manager of a supermarket forced to not exclude any products in order to not infringe the free play of competition. But not all conceptions are worth buying. Some of Heidegger’s Nazi commitments show that there are things to avoid; in spite of numerous requests, he never formulated the definitive condemnation of Hitler’s ideas. And we must not forget that Heidegger used the concept of *Dasein* (being-there, being-in-the-world) to uncover the primal nature of ‘being’ (*Sein*). The question of being itself (e.g. on truth) is built on *Dasein*,

just as Heidegger's philosophy is built on his being-in-the-world (including his adherence to Nazism).

Philosophical chickenpox

If we now turn for a moment to Johnson's second option (archaeological theory does not exist), he explains it by invoking the disjuncture between what 'we say we do as "archaeological theorists" and what we actually do as archaeologists' (p. 118) (I would have preferred the more modest expression 'theoreticians'). Johnson rightly regrets that the relationship between overt theorizing and what archaeologists actually do has been very poorly understood. Why might this be the case?

A few years ago, while we were talking about the confusion that postprocessual archaeology sparked off among the French archaeologists, my friend Bruno Latour gave me a possible answer: 'you know, Anick, philosophy is like chickenpox: you never recover if you catch it too late in life' (philosophy is a part of the French secondary-school education; some children get it early).

Another possible answer was given only a few months ago by one of my other friends, Maurice Godelier:

There has been a lot of talk in the last twenty years about deconstructing the social sciences; post-modernism even made a specialty of it. . . . We are well acquainted with post-modernism in France, after all we were the ones who exported it in the works of Derrida, Foucault, Deleuze, Lyotard and others. But although we French are exporters, we are not necessarily consumers, or at least we consume only those parts of their work that can be of use in analysing the complexity of the facts (2005).

What is scientific?

Stephen J. Gould (1989) has remind us that it is neither the method of observation nor the type of analysis, nor even the adequacy of the link between the interpretation and the facts or of the reasoning, nor the kind of law used (never mind its nature: social, physical or biological), that allows us to establish that an interpretation is scientifically correct. What is scientific is defined by whether we are able to decide if the hypothesis and the interpretations are wrong or probably correct. What is scientific is decided by whether one is able to verify the interpretations and to test them. As far as the validation of archaeological interpretations is concerned, it is based on the quality and quantity of the data. If there are sufficient data, that allows us to notice similar procedures and tendencies in different types of data. The abundance of data is a precondition for our testing and validation methodologies. Validation is possible as soon as interpretation is based on regularities, and on such an abundance and such a variety of data that no other coordinated interpretation can be possible, even if any of these interpretations taken separately cannot provide convincing proof.

Certainly, the 'new archaeologists' failed. They failed more by practising extremism than by choosing any particular scientific option. They simply forgot that there are two types of science: the stereotyped sciences (physics, chemistry and so on) and the flexible sciences (history, archaeology,

anthropology, sociology and so on). New archaeologists wrongly took archaeology for a stereotyped science, trying to adopt a ‘hard’ mode of reasoning (in particular the hypothetico-deductive one that prevails in physics). As a result they were thinking in a closed world, forgetting that a society is an open system. They neglected the fact that the general principles that sustain any social constructs take locally different cultural forms, and the fact that these forms may be full of contradictions and adaptations. Actually, flexibility, contradiction and adaptation are the fundamental conditions of human success, both biological and historical.

Was the failure of the ‘new archaeologists’ a reason to throw away the baby with the bath water? Was it a reason to consider the notion of system incorrect to the point of banning the word itself? Was it a reason to consider that archaeological theories were impossible to derive? Was it a reason to consider archaeology an ascientific field of enquiry?

Let’s try now to be constructive and not ‘post-’, or ‘post-post-’

A much more efficient tool than phenomenology to deal with the unreal (but conceptual) nature of the Cartesian dichotomies is probably the ‘symmetric anthropology’ project, initiated by Bruno Latour (1993; 1994; 1996; 2002; 2004; 2005; see also Descola 1996a; 1996b; 2003; 2005) twenty years ago. Latour explores the possible philosophical origin of the ‘nature/culture’ opposition, gives another definition of modernity and offers an alternative in the form of the notion of ‘collectives’. The word is used to underline that neither ‘cultures’ nor ‘natures’ are brought into play, but what he calls ‘actor[human–non-human]-network-theory’. He shows that the two extreme positions “humans” and “non-humans” are devoid of meaning and should be replaced, instead, by a focus on the exchange of properties, competences and performances. ‘There is no sense in which a human or a technique can be said to exist’ (1993, NN). Furthermore, he promotes two slogans: ‘we have never been modern’ and ‘post-modernity was a useful transition’ (2002).

Even if there do not seem to have been any archaeological data that have ever served to produce a general (non-Mickey Mouse) theory which could be used to interpret other (archaeological or non-archaeological) observations, I do know that archaeology and material culture can produce scientific laws and theories and deal with variations and differences without rejecting similarities dear to our necessary and fundamental archaeological typologies. I have simply and modestly done that with the study of the articulation between uniformity, variability and differentiation that underlies any domestic dwelling (architectural model, options, contingencies and individual traits). Once this articulation was decoded, it was easy to bring the process of transformation of an architectural tradition into light and, one step further, to predict the potential persistence (durability) of a culture (Coudart 1994a; 1994b).

Of course this must not be confused with reality, as we are in the field of theories. But for once the theory has been produced from archaeological data and material culture, and not borrowed from another discipline to be applied to archaeological data.

Foundational histories and power Mark P. Leone

Matthew Johnson's essay is somewhat more conservative than I had anticipated and I am happy about that. He promotes a scientific archaeology in the sense that he promotes the kind of knowledge that can be established through verifiability. Then he takes what I see to be an increasingly frequent approach to the use of the phenomenological and hermeneutic position in archaeology. His positions are relatively gentle on these matters and are an attempt to seek out a way to utilize archaeology as a superior political tool when it is needed, as it most assuredly is, from place to place in the world. He remains attuned to the established fact that the definitions of many facts are themselves a function of political contexts. I am not sure yet that Matthew Johnson's brief essay succeeds with all the links he intends to make, but I think that a number of scholars are headed in this direction.

I would like to explore elements of Matthew Johnson's positions by making reference to his invocation of foundational histories in archaeology in his last paragraph. I am going to insert into Johnson's term my assertion that introductory archaeological textbooks in English for undergraduates around the world are what he means by foundational histories. To round out this definition by citation I include Brian Fagan's *Ancient North America. The archaeology of a continent* (1995), *In the beginning. An introduction to archaeology* (2001) and *People of the Earth. An introduction to world prehistory* (2004); Kenneth Feder's *Linking to the past. A brief introduction to archaeology* (2004) and *The past in perspective. An introduction to human prehistory* (2007); Colin Renfrew and Paul Bahn's *Archaeology. Theories, methods, and practice* (2004); Robert Sharer and Wendy Ashmore's *Archaeology. Discovering our past* (2003); Mark Sutton and Robert Yohe's *Archaeology. The science of the human past* (2006); and David Hurst Thomas and Robert Kelly's *Archaeology* (2006) and *Archaeology. Down to earth* (2007). These books are foundational because they contain the questions that archaeologists have asked for over a century, the data that archaeologists define as answering these questions, the methods used to link the questions and the data, and then a worldwide presentation of answers to these questions, as well as a worldwide summary of what Graham Clark (1977) so felicitously called *World prehistory*.

In my reflections on Matthew Johnson's invocation of foundational texts and the existence of archaeological theory, I would like to make three points:

- 1 Foundational histories require a commitment to theory, or at the very least an acknowledgement that it exists. Introductory texts treat theory as facts; reduce it to innocent paragraphs; and make incompatible theories equivalent to each other by giving them the same space, weight and respect. Introductory texts commodify theories and, in that sense, they make theories look like every other compartment in the book.

As an archaeologist who uses Marxist ideas, I would argue that theories are always political. Theories are not neutral because they always have a political setting, and that setting is usually unavoidable. While this sounds like an accusation, I mean it to be a statement of the virtually inevitable. However, my position invites us to see that there is no such thing as theory without politics, just as there turned out to be no such thing as the theory of relativity without the atom and hydrogen bombs; by this I mean their use on Japan in the Second World War and in the Pacific Islands in testing situations during the Cold War.

- 2 From here on I would like to limit my comments to historical archaeology and the inclusion of its material in foundational texts, including texts by many of the same authors. I limit myself here to historical archaeology because, increasingly, it is what I know. However, because I was trained as a prehistorian, I know that what I say is true of anthropological archaeology as a whole.

A central justification for doing historical archaeology is the history of European expansion, including the advent and spread of colonialism. Within this domain, and increasingly separate from it, is an effort to understand communities created by colonialism. Understanding these communities is frequently expressed as an attempt to recover peoples without histories, without voices and without credit for their place in national life.

Without adequate use of a powerful theory, most archaeologists in foundational texts have failed to ask why some people are rich and some poor. Most archaeologists do not deal with the justifications for being rich and the objections to being poor. Nor do archaeologists of any kind deal with the issue of a purpose in social life. We do deal with functionalism and in that sense we describe rich and poor and their roles in social life (Wogaman 2006). However, we in historical archaeology have left evolutionism behind, for the most part. While this may not be a mistake in some ways, it is a mistake in one way because it allows us to avoid that painful moment when evolutionary theory insists that there is no purpose behind evolution except proliferation and the propagation of life forms. It is important to face this theoretical statement in front of undergraduates because it is the power of this statement, when translated by us into social anthropology, that contains the basis for a discussion of intelligence in social life. Do we think there is any? It is at this point that we can confront with great seriousness the actual roots of the matter of creation, creationism and intelligent design. If we do not think there is anything other than functionalism in social life, and that it is fundamentally not intelligent in the sense of being directed to an ultimate end, then archaeologists as anthropologists and social philosophers must say why. It is not that there are no answers. There are. It is that we have been quite cowardly in our efforts to address the point of creating new knowledge of social life through our field. This is completely missing in foundational texts and would not be if we were fluent in any kind of theory.

3 In order to attempt to deal with the matter of purpose in social life, as well as the place of human intelligence in it, I think it is crucial that we return to science and see that training in it is fundamentally democratizing (Holt 2006). We teach people to think scientifically, to use empiricism and to reason in such a way that ideas about the world are connected to evidence drawn from the world. The democratizing quality of science does not come from the ease of learning science. Its democratizing capacity comes because it is open to anyone and it contains the invitation to entertain ideas which can change the world. A combination of a new idea and empiricism allows challenges to established thinking. The use of science sets a tone (Toll 2006) and that tone has an established history of frequently being in opposition to and contesting with established hierarchy. In other words, one of the purposes behind social life is to analyse it and to provide individuals with methods by which we can offer the means of resistance to unfair and exploitative sources of power. These forms of resistance can be achieved through archaeological genealogies which create new elements in foundational histories that are in opposition to self-proclaimed, self-evident truths which then begin to look questionable.

Because science is a source of innovation in all of the disciplines we know of, and because it has the capacity to democratize, it is a potential tool within social life that provides a form of intelligence which improves the purpose of social life, and thus comments positively on the purpose of that life itself.

I strongly recommend that archaeologists of all kinds acknowledge that we flourish in and are dependent upon democratic societies. We have long understood this. There are three theories which support democratic societies. There is the kind of Enlightenment theory we associate with the American Declaration of Independence, and the Bill of Rights as attached to the United States Constitution. There are parallel French documents of virtually equal age. Then there is Marxist theory, as opposed to communist practice, which contains socialism that is also a democratizing theory.

The third set of theories is much harder to evoke, but is virtually universal in the lives of archaeologists around the world. In order to evoke the actual source of theory in historical archaeology, I want to ask whether or not such archaeologists are really Christian Democrats in the European sense and not just democrats. In order to discover the theory behind European Christian Democrats, I want to first pose the possibility that we are afraid to deal with this. We would rather deal with Marxist theory and contextualizing approaches which privilege everyday life in communities around the world. Because foundational histories exist within theoretical contexts, although unacknowledged, and because such contexts are inevitably political when so widely read and offered, it is crucial to understand what motivates our questions, and the lack of answers. I conclude by asking, what is it that really motivates our preference for the disadvantaged in our archaeological genealogies? What are we afraid to discuss? Could it be that our theoretical rationale for

historical archaeology actually stems from the Christian upbringing of most of us?

I would like to combine the notion of foundational histories with the third point that science is fundamentally democratizing in so far as it may, and often does, confront and confound hierarchy and autocracy. I would like to provide an example of an archaeological genealogy which is an outline of how to write a few paragraphs in a foundational history which explores hypotheses about our origins and which, simultaneously, confronts an active autocratic act. I do this all by way of illustrating the connection between theory and power.

In January 2006 I co-taught a course to 22 University of Maryland undergraduates who were enrolled in a study-abroad course in Italy. The course is jointly assigned to economics and anthropology. There is a good deal of ancient history, art history, classical myth and anthropological archaeology in the course. I was temporarily understudy to the senior economist who created the course and taught it for the previous five years. The course began in modern Ascea, a town outside ancient Velia, a very large Greek ruin occupied from the 6th century B.C. to the end of Roman times. The course then spent five days in and around Pompeii, five days in Rome and four days in Florence. Because Professor Emeritus Clopper Almon, who established the course, and I both enjoy geology, we visited Pozzuoli where the principal of rapid uplift was first identified by Charles Lyell in the 19th century. When we got to Florence something quite different happened concerning geology.

Clopper Almon had assigned a term paper on Nicholas Steno to one of our undergraduates, Anar Mikailov (2006), who was assigned a recent book called *Seashells on the mountain top* (Cutler 2003). When we got to Florence, Professor Almon pointed out that Nicholas Steno was buried in the Church of San Lorenzo, the Medici parish church near the centre of the city which contains Michelangelo's famous Medici tombs. Nicholas Steno was formally buried in the crypt of this church, to which he had been removed from Germany, where he was first buried when he died. This removal had been requested by one of the Medici in the 17th century.

It is clear that Steno was known to the Medici because he was a brilliant and famous anatomist, who had then become an even more brilliant geologist. While he had done his anatomy in northern Europe, where he was born and raised as a Lutheran, he had enunciated the principles of stratigraphy by working in central and southern Italy, particularly around Naples, which was and remains a geologically active region with remarkably sudden moments of geological uplift that were well recorded. During his time in Italy he had become a Catholic, and later a priest. He was buried in Florence because of his scientific fame.

Foundational texts used to mention Steno's work as pivotal to the work of Scandinavian and British archaeologists, as well as the early geologists like Lyell, who enunciated the more current uses of stratigraphy and stratigraphic excavation, including the use of 'index fossils'. However, Steno's work is still cited (Thomas and Kelly 2006, 153; 2007, 100). Even though foundational texts mention Steno less, virtually all scholars familiar with the history of

geology and archaeology know that Lyell, and then the Scandinavians who invented the three-ages system, depended on him.

When our University of Maryland class got to Florence, Professor Almon pointed out that in 1988 the previous pope, John Paul II, had declared Nicholas Steno to be a saint. When this was done, Steno's body was moved from the San Lorenzo crypt up to a chapel in the main church, sitting roughly above Michelangelo's Medici tombs. When one visits this chapel, there is a mosaic picture of Steno, an early Christian sarcophagus done in the Roman style that holds his bones, and then a large igneous rock embedded in the wall opposite the sarcophagus. I visited. While I was standing in the chapel, I noticed a couple of dozen small pieces of paper sitting on the sarcophagus lid and, without thinking, I picked one up. I realized that they were supplications to the saint. I quickly put down what I had just picked up. So here is Nicholas Steno, founder of modern geology, and without whose work modern archaeology would be impossible. And we have him in a chapel with people praying to him.

I raise this issue here because I propose that canonizing Nicholas Steno is a substantial attempt at appropriating his scientific genius and domesticating it for non-scientific reasons. This conclusion is easy to come to because a six- or seven-minute walk across Florence from San Lorenzo, in the Church of Santa Croce, is the tomb of Galileo, with whom those who create saints have never made their peace. Based on my hypothesis that Steno belongs to science and that the appropriation is really aimed at making Steno ideological, I think an analysis that shows young archaeologists how to think about hypothesis-testing in the history of their field is appropriate and belongs in a foundational text.

To test the hypothesis, I found a history of Steno's life and subsequent treatment of him on the website (2006) of the Danish Embassy in Washington, DC (<http://www.ambwashington.um.dk/en/menu/informationabout-denmarke/ducation&training/famousdanishscientists/nicolaussteno.htm>). It is clear there is an enormous amount of material on Steno himself but the reasoning behind his canonization is anything but explicit. In his later years he attempted to lead an exemplary life in Germany and kept a record of his own attempts at a pious life of good works. He put aside his anatomy and his geology. No reason is given for this, or for his vocational change to a religious life.

As we rewrite a foundational text to show our own ability to contest the appropriation of one of our founding geniuses, it is important that we throw into doubt the reasons for Steno's canonization by asking why him, and not Galileo. It is important to distinguish between scientific accomplishments and an effort to turn a scientist into a saint through a process which confuses achievements, power and epistemologies. Through this we maintain our ability to understand human processes and to keep them rational, as opposed to magical and subpoenaed by hierarchy. With this we can use the democratizing capabilities of science to contest the actions of hierarchy, especially when those actions impinge upon our own accomplishments, and thus their empirical foundations. In other words, the ideology we confront in Florence is that we can be saints as well as scientists.

Acknowledgements

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Archaeology, hermeneutics of suspicion and phenomenological trivialization *Bjørnar Olsen*

The point of departure for this debate (and the SAA forum) was the question 'does archaeological theory exist?' Matthew Johnson's answer is wisely mixed, but mostly negative. Archaeological theory does not exist, he tells us, because there is hardly any distinctive archaeological way of theoretical thinking 'to which most or even the largest group of archaeologists would willingly or knowingly subscribe' (p. 117). I shall not spend much time on Johnson's denial, which for several reasons may well be justified, just note that if we apply his rather rigorous consensual criterion to other disciplines, we would probably be searching in vain also for any sociological, economic or even philosophical theory – which Johnson still seems quite convinced do exist.

In most of his rich paper, however, Johnson is not really dealing explicitly with this somewhat ambiguous question. Rather, he is concerned with some theoretical issues closer to his heart, such as the well-being of 'agency theory' in archaeology and the fallacies of what he terms British phenomenology. Taking his paper as my starting point, I will structure my comments in four sections. The first deals with the supposed theory–practice split, the second with the representation of archaeological doings and the third with the question of agency, while the final section addresses Johnson's criticism of 'British phenomenology'.

Theory and practice

In his paper Matthew Johnson uses much energy in arguing that archaeology is a discipline heavily affected by thick layers of black-boxed knowledge that govern most of our conduct. To Johnson these 'underlying assumptions and traditions' (p. 117) do not qualify as 'theory'; actually they constitute a counter-regime of tacit disciplinary power that blocks almost all attempts to apply new theories to the archaeological material. This leaves us with a

Janus-faced discipline where there is no correspondence between the loudly uttered ‘will to theory’ on the one hand and archaeological practice on the other.

Johnson rightly claims that the relationship between theory and practice has been under-theorized and his paper constitutes a good starting point to embark on the discussion of what this relationship is all about. Having said this, it is not very easy to grasp how Johnson himself conceives this relationship in more general terms. Are theory and practice ontologically distinct realms? And if so, how do these (eventual) realms blend or merge? It seems as if Johnson’s ideal conception of this relationship complies with a conventional modernist hierarchy, where theory is the head, and practice is the obedient acting body (an order unfortunately totally disturbed in archaeology).

However, and also taking into account the phenomenological legacy, one may ask if there are other ways of coming to knowledge than through theory. Is fieldwork, for instance, only an arena for the proper ‘practical’ application of theories – in order to provide data sensible to, and readable by, these very theories? Or may our engagements with landscapes, local people and the persistent past, the walking and working, the cohabitation and collaboration (including associated sentiments, discourses and politics), in short, all encounters experienced in the field, actually positively affect and challenge the way we think, write and mediate the past (cf. Renfrew 2003, 39–40; Bradley 2003; Witmore 2004)?

Johnson seems convinced that theory always intervenes – that things cannot speak ‘without the benefit of intervening theory’ (p. 129; cf. his criticism of the empiricism of ‘British phenomenology’). This conviction is of course commonplace in theoretical discourses but has no self-evident justification. It is a faith related to a particular modernist ontology that, since Kant (at least), has denied any direct access to things (as non-transcendental entities). Within this regime (to which ‘empiricism’ also belongs), things appear to us only as the refined products of our thinking; it is only in this abstracted condition that they are allowed access to the knowable world and become objects of science, in other words data. Data probably need theory to become meaningful, but things do not. Otherwise how could people find their mundane life worlds meaningfully constituted? This is not to take the absurd position that our archaeological interpretations and explanations will be better off without theory – just a gentle reminder that there are other passages to knowledge than through theory.

Moreover, as science studies have shown, to do science itself – despite its edifying and pure self-image – is never a question of just theory and bright minds. The key to success has probably far more often than not included clever rhetoric, enabling infrastructures, political networks, alliances and muddy practices (cf. Hughes 1983; Latour and Woolgar 1986; Latour 1987; 1999). Science has, to borrow a phrase from Latour (1999, 288), always been ‘hairy, networky, rhizome-like’. More important for this debate, however, is that this social and practical entanglement is far from conceived of only as a repugnant source of contamination, constraint and distortion. It constitutes an indispensable and productive force in ‘science’s blood flow’ (Latour 1999), in science as labour and in knowledge as realistically fabricated. Does

Johnson see any positive implication for our knowledge ambitions in what archaeologists actually do?

Bringing in Latour, it is tempting to reflect on the fact that what Johnson refers to as the unspoken traditions of today are themselves the embodied, ‘black-boxed’ outcomes of often fierce debates and controversies in archaeology’s past. This triggers a question: what happens if some of our current theoretical debates are settled, some of our own theories become successfully applied and approaches stabilized, and if consensus and the desired correspondence are arrived at? Mark Leone’s slightly hasty claim from 1971 (!), that the revolution had ended and that the new archeology had become everyone’s archaeology (Leone 1971, 220), is probably not the desired scenario for most of us, regardless of what paradigm becomes normalized. In my opinion, what we see today (also on a global scale) may be a ‘clash’ not primarily between theory and practice, but rather between different archaeologies (conceived of as different ‘blends’), where some are more stabilized – better networked – than others.

Sameness and diversity

Matthew Johnson’s depiction of archaeology leaves little room for merriment and optimism. The picture painted is that of a prison-house of tradition in which ‘the overwhelming majority of archaeologists’ just uncritically divide the material remains of the past into cultures, phases and types (p. 123). It is somewhat of an irony that Johnson, who is so concerned with the archaeologists’ blindness towards the variability in the archaeological record, is so unable to spot anything but sameness in his own dealing with archaeology and archaeologists. Or, to be more correct, one exception is actually spotted (which, however, ‘proves the rule’): ‘North American historical archaeologists have recently analysed and published finds by house lot, rather than by artefact type, in contrast to their British and [*sic*] European counterparts’ (p. 124). For this particular claim I can do nothing but suggest another survey to find out what is actually taking place around the world. Moreover, to replace artefact type with house lot as the analytical unit (although artefacts still have to be named, I presume?), however pleasing and right it may seem, is also to activate other names and conceptual schemes that carry their own effective histories of black-boxed conceptions, hidden meanings and taken-for-granted disciplinary and cultural assumptions. Maybe archaeology in its ‘everydayness’ is no worse off than anthropology, sociology and other social sciences? How much more reflexive and critical are the common and taken-for-granted uses of concepts such as society, social structures, institutions, intentions and action, including the new mantras of agency and embodiment, in these disciplines (or, for that matter, in archaeology)?

With no intention of replacing black with white, I think Johnson far too conveniently glosses over the changes that have taken place in the so-called ‘practical domains’, such as archaeological teaching, fieldwork, data-handling, the curation of museum exhibits and so on. Taking fieldwork as an example, the new archaeology of course had a tremendous impact upon the way we survey, excavate and treat the finds. The postprocessual call for reflexivity (whatever importance we assign to that) is not just a buzzword

in the university corridors (Chadwick 2003; Berggren and Hodder 2003). Hodder's work at Çatalhöyük (Hodder 1997; 1999; 2000) may be overstated, but there are numerous other (and maybe more pertinent) examples in, for instance, Scandinavia, where so-called reflexive approaches are applied frequently also in contract archaeology (Larsen *et al.* 1993; Berggren and Burström 2002; Jensen 2005; Borake and Beck 2006).

Depressed by the field, Johnson finds the site report, which 'obscures agency by its very organization' (p. 124), especially constraining. Leaving the question of agency aside for the moment, I certainly agree that the conventional monological site report has not been very receptive of the dynamic and multiple processes of encounters and interpretation taking place during fieldwork. Moreover, by its simplistic narrative it has also seriously misrepresented the palimpsests of structures and things excavated. Since the 1990s, however, new means of representing – or mediating – this experience, utilizing the possibilities offered by visual media and electronic information technology (e.g. to create hypermedia documents), have made their way into archaeology (e.g. Banning 1993; Larsen *et al.* 1993; Huggett 1995; Hodder 2000; Tilley, Hamilton and Bender 2000; Tringham 2000; Holtorf 2000–1; Witmore 2004). In some areas more than in others, of course, this has added at least some nuances to the story told.

Agency and the hermeneutics of suspicion

This brings me to the question of agency. Johnson uses the sad fate of 'agency theory' in archaeology to exemplify the incompatibility of theory and practice. The whole organizational, practical and conceptual apparatus of 'bread-and-butter' archaeology grinds any serious consideration of agency into dust. This poor sociological theory has no chance against the inert mass of archaeological assumption and constraining disciplinary structures; it is 'to set up an unequal contest' (p. 124). But if this is the case, then what is the role of agency in the disciplinary community we call archaeology? Is this theory without relevance for the study of the archaeological society itself? If agency theory does not work in a petty community of rational scientists, where could it ever work? Or is it the case that we have been given a rather biased picture of archaeology that 'militate[s] against the visibility of agency', to borrow a phrase from Johnson himself (p. 123). I have encountered this before. On the one hand, archaeologists talk in almost fundamentalist terms about agency, the active individual and variability in the past, and on the other hand they talk about archaeologists and their own being-in-the-world as if they were dupes, totally lacking the capacity to act and change. Again, there is a striking and rather ironic resemblance between the way Johnson approaches archaeology and the way he claims archaeologists deal with the past and the material record.

There is much more to be said about this. To Johnson, agency is a property of human actors. When dealing with those who are more liberal and want to give a share of it to things and non-humans, Johnson becomes suspicious. The reason why there is this talk in archaeology today about biographies of things, and things being actors, he tells us, is 'its rhetorical appeal to the centrality of material culture in shaping identity, rather than . . . its coherence

as an intellectual position' (p. 125). I must admit that I find this hermeneutics of suspicion strangely misplaced, and once again without much attempt at justification. To put this conspiracy theory to a simple test: if it were true, why is it that these ideas are being claimed mainly outside archaeology – in philosophy, anthropology, sociology and science studies? Is it the rhetorical appeal to the centrality of material culture in shaping identity that has led Alfred Gell, Bruno Latour, John Law, Michel Callon, Michel Serres and Donna Haraway – just to mention a few – to their conclusions? Probably not, and if so, why should archaeologists not be granted the same capacity of intellectual coherence? And no, I do not accept the catch-22 rhetoric where the accusation of having hidden agendas can always be justified as an inescapable consequence of effective history ('the academic work of individuals is never produced in an intellectual and cultural vacuum and cannot be evaluated as if one were hermetically sealed off from its wider adoption and intellectual currency'; p. 129).

Talking about agendas and suspicions, I wonder why this peculiar hermeneutics is not applied more generously also to 'agency theory' and to Johnson's own long-held concern with the active individual? Why, for instance, do we see this tendency in certain archaeological settlements to isolate an 'agency theory' from the wider orbit of structuration theory, *pace* Giddens and Bourdieu (also given the possible confusion that may arise from the fact that agency theory developed as a concept of its own in managerial theories of organizations and business enterprises during the 1970s)? It is tempting to think it is because the agency component is felt to be more attractive and humanistically pleasing than other and more dismal aspects of structuration theory (although, ironically, the idea of structural 'overdetermination' may have suited Johnson's analysis of archaeology quite nicely). And, to complete this suspicious hermeneutics, was it just accidental that the focus on agency, individualism and human intentionality grew so strong precisely in Britain and the US during the 1980s and 1990s? If not, maybe one should be more careful when conceiving them as almost a-priori attributes of humanity, something already known, that can be used as a baseline for all social analysis (Thomas 2000, 144, 149), and which, provided the right attitude, always should be expected to shine through in the archaeological record? As John Barrett has argued, within this ontology, agency is reduced 'to an isolated being whose actions are represented by the archaeological record. That is to say, archaeology seeks the individual whose actions have resulted in a material trace' (Barrett 2000, 61). I must admit that I am a bit amazed – and amused – that Johnson refers to Foucault to sustain his position, a scholar deservedly famous for his persistent decentering of the subject.

Phenomenology, Romanticism and trivialization

I shall finally turn to so-called British phenomenology, which according to Johnson is little more than a kind of neo-Romanticism. The problem with Johnson's presentation is not to assert that there is a link to Romantic poets such as Wordsworth (see below). The problem – or at least one of them – is that this argumentation is very imprecise, providing hardly any

references to what works and what particular reasoning he is actually talking about. It all boils down to a rather ridiculous straw-man version of this so-called phenomenology (as well as of the presumed Romantic legacy) where knowledge is arrived at purely by rambling around in the fields with wind in your hair. It may be funny, but I think it misrepresents the works of those scholars who actually have toiled to deal with these issues at considerable theoretical depth.

And the word depth is crucial in this respect. Phenomenology has become one of those buzzwords that keep popping up almost everywhere. But what proliferates is a trivialized version of phenomenology, which often consists of little more than name-dropping and common-sense statements. The trivialization of phenomenology illustrates a worrying tendency of ‘instantness’ in parts of archaeology today. Theory may be firmly in place, but getting at it should not be too difficult and time-consuming, involving any scrutiny or deep readings. Why bother to read Heidegger or Merleau-Ponty, not to mention Benjamin, when you can get far by reading excerpts of Christopher Tilley’s book on the phenomenology of landscape (Tilley 1994). Or, if that is too exhausting, you can suffice with the two-sentence condensation of phenomenological philosophy in theoretical textbooks, where students learn that phenomenology is ‘the study of conscious human experience in everyday life’ (Johnson 1999, 114). Alternatively, you can turn to Hodder, who conveniently summarizes Heidegger’s *Sein und Zeit* in the dictum ‘all human understanding, including the natural sciences, is interpretive’ (Hodder 1999, 32). Also in the current paper, Johnson turns phenomenology into interpretation, an epistemology: ‘how do archaeologists get at meaning?’

These statements, however self-evident and convenient they may seem, are unfortunately not very helpful when it comes to telling students (and others) what phenomenology as a philosophical position is about. What Heidegger claimed was that our ordinary understanding of the world is mostly not interpretative in the sense suggested above. It is not something consciously experienced or grounded in theory or reflexivity. Actually these are ‘ontic’ modes that move us away from things, from our circumspective, ready-to-hand dealing with the world, and which introduce an abstracted and disentangled gaze characteristic of science. Neither is phenomenology just about how we experience the world, it is also about the way the world makes itself present to us. Phenomenology is, in Heidegger’s words, to let ‘that which shows itself be seen from itself in the very way in which it shows itself from itself ... expressing nothing else than the maxim ...: “To the things themselves”’ (Heidegger 1962, 58). Or, as Merleau-Ponty remarked, ‘It is the things themselves, from the depth of their silence, that it wishes to bring to expression’ (Merleau-Ponty 1968, 4).

Johnson has wisely revised some of his opinions regarding the proposed link between English Romanticism and the phenomenological approaches of archaeologists such as Tilley and Thomas. It still remains unclear, however, actually what ‘constellation of values’ have these authors, ‘so uncritically and with such a lack of self-awareness’ (p. 128), translated into archaeological discourse two centuries later. If it is those listed (i.e. the stress on ancestry, a

distant gaze from above, being gender-blind and embedding an all-pervading empiricism) I am anticipating some verification.

Johnson rightly notes that there is a link between Romanticism and phenomenology (as a philosophical project), and thus it should not be a big surprise that it is possible to identify a connection also in current phenomenological approaches in archaeology. However, given the negative and ironic portrayal of poets such as William Wordsworth it is hard to understand what this link consists of, and even less why it exists. May it be that the Romantic legacy is more complex, nuanced and positive than the one Johnson narrates?

Heidegger was fascinated by the poetry of Friedrich Hölderlin (as was Benjamin), and several of the themes he explored (especially in his later writings) show a clear affinity also with the ideas expressed by William Wordsworth. Central to these poets was their concern with the loss of our 'mimetic' (Benjamin's term) relationship with things and thus a 'full' and embodied involvement with the world. To Wordsworth, this relationship could only be recaptured or reactivated if we sealed off the urge of our dominant rational consciousness to keep things and humans always separated (a move close to Heidegger's 'step back') (Andersson 2001). By stressing poetry and experience over science and theory (a hermeneutics of experience rather than of interpretation) (Bruns 1995), these 'Romantic' poets anticipated much of the 'thing-friendly' writings of Rilke, Benjamin and Heidegger. And apropos distant gaze, maybe Wordsworth, as Bate (2000) has argued, was actually trying to replace the 'tyranny of the eye' (as exemplified by Romantic picturesque art) with a new (or lost) poetic wholeness, enabling us 'to see into the life of things'? Taking into account the large body of research and literature on the complexity, variation and depth of this tradition, as well as on the relationship between these late 18th-century poets and Heidegger (cf. Cavell 1988; Bruns 1989; 1995; Bate 2000; Andersson 2001), we may end this discourse by concluding that yes, there is a relationship between Wordsworth and Tilley–Thomas, but it may be more subtle, nuanced and less suspicious than Johnson's alleged 'wind-in-your-hair' approach to knowledge seems to suggest.

According to Johnson, 'theory is the order we choose to put facts in' (p. 118). Such a narrow definition may work for some epistemologies, and it may work for applied social theory. Ontology, however, is ruled out – and phenomenology is more than anything about being-in-the-world. To what degree we should ground our understanding of materiality and the past in our everyday experience is of course a matter of debate. It probably relates to what knowledge we are searching for. There are many problems with phenomenology, which anyone who bothers with it should discuss. However, my main message here is that we achieve very little by trivializing and misrepresenting it. To use Johnson's words, perhaps it 'results in an impoverishment of theoretical appreciation and evaluation of other people's arguments, particularly when those arguments stem from questions arising in an area and period other than one's own' (p. 131).

Who's afraid of Martin Heidegger? Some friendly comments on the paper by Professor Matthew Johnson *Christopher S. Peebles*

I am in general agreement with Professor Johnson's main points. To his and the editor's question, 'is there archaeological theory?', His answer is 'yes and no'. This ambiguous but accurate answer can be divided variously. The first category would be archaeological theory per se, which would encompass the work and thought from that of Nicholas Steno in the 17th century to that of Michael Schiffer (2002) today. It would be about the natural transformations of 'deposits'. Next there would be theories about the transformation of natural materials into the more or less durable remains that comprise archaeological 'data' – the stuff of everyday lives. Again, work of Michael Schiffer (2001) and Daniel Miller (1998) comes to mind. They and a long list of contemporary archaeologists deploy theory on behalf of the understanding of the creation of things that build the human habitat. Broader use of theory comes with the deployment of frameworks from demography to demonology and ecology to ethics that are used to structure historical and anthropological questions that might be answered with archaeological remains. Each selection of a theoretical position entails one or another metaphysical commitment about what comprises 'data' and 'evidence' and what constitutes a proper explanation. In this regard Professor Johnson asks, does archaeology indiscriminately adopt and apply various philosophical positions without proper regard for their extent, implications and conditions for application? His answer is yes, certainly, and to our disadvantage. As Christopher Chippindale and I have noted on several occasions, archaeology treats philosophical traditions a bit like a Chinese menu or perhaps the contents of a supermarket: the archaeologist takes one from column A and another from column B, and a whole bunch of stuff from the dairy aisle and more from the fruits and vegetables section. Not only is this not kosher – one does not mix dairy and Derrida – but the selections are radically contradictory and sometimes even incoherent.

Take, for example, the much-maligned adoption of logical empiricism – logical positivism if you will. The goal was worthy: analyse the form of arguments, including the place of theories therein, add their empirical referents in the process of explanation, and finally verify the match between premises and conclusion. It became a manifesto complete with social groups and an implicit mantra that proclaimed, 'if you reason correctly, you are always right'. Not true. There are sufficient logical and empirical contradictions in the dogma of positivism to bring it crashing down: there are metaphysical inconsistencies – the positivist position that excludes metaphysics is itself a metaphysical proposition – and then there is the problem of 'systems with histories' (biological and cultural) that shift their initial conditions through time, which means that one term of the explanans is not stable and that the symmetry of explanation and prediction, a tenet of

positivism, does not hold. As Joseph Schwartz emphasizes in his superb book *The creative moment*,

Historicity is the central feature of living matter. Every biological object is an historical object carrying within its DNA the shaping of billions of years of history. In the often quoted words of Max Delbrück: ‘You cannot hope to explain so wise an old bird in a few simple words.’

Although it is threatening to established frameworks, Delbrück’s paradox is also exciting. The recognition of historical processes, the emancipation of biology [and culture] from the timeless relational schemes of physics invites a critical examination of a fundamental concept of Western culture. The world is not a machine. It is a complex, historically evolved network of physical, biological, and cultural structures. And the sooner we liberate ourselves from the then liberating, and now oppressive mechanical framework of the seventeenth century the better off we will be (Schwartz 1992, 131).

Delbrück’s paradox does not eliminate the requirement for the construction of well-formed deductive arguments in the construction of causal explanations in the biological and human sciences. Certainly the normative programmes of philosophers as diverse as Popper (e.g. 1972) and Toulmin (e.g. 1972) are apposite. Moreover, as several of the chapters in the recent book *The politics of method in the human sciences. Positivism and its epistemological others* (Steinmetz 2005) point out, positivism in this more temperate form is alive and well in several of the human sciences. However, positivism as practised in mid-century and adopted wholesale by some archaeologists is dead. What is worth saving is the value of an explicit deductive argument with empirical referents. It is not the only mode of explanation, as I will sketch below, but it is an important framework for the creation of knowledge and as a vehicle for understanding.

Jerome Bruner, the great psychologist and cognitive scientist, argues in his book *Actual minds, possible worlds* that there are ‘two modes of cognitive functioning, two modes of thought, each providing distinctive ways of ordering experience, of constructing reality. The two, though complementary, are irreducible to one another’ (Bruner 1986, 11). Each has its own standard of judgement. The one, the logico-scientific, which he calls paradigmatic, is assessed in terms of how well it is formed, and the standard of judgement is the match of its conclusions with the empirical world. In a word, it relies on Tarski’s kind of truth with a little ‘t’. The second, which he designates the narrative, is judged in terms of its ‘verisimilitude’, its lifelikeness:

The imaginative application of the narrative mode leads instead to good stories, gripping drama, believable (though not necessarily ‘true’) historical accounts. It deals in human or human-like intention and action and the vicissitudes and consequences that mark their course. It strives to put timeless miracles into the particulars of experience and to locate the experience in time and space (Bruner 1986, 13).

Bruner does separate historical narrative from fiction. The former depends on evidence beyond the imagination of a historian or reader, on the

traces of the past. In a word, it depends on evidence. In Collingwood's felicitous phrase, these traces are the 'past encapsulated in the present' (1939, 96–100). To put it in the words of Paul Ricoeur (1984), in his Aquinas Lecture for 1984, history must be true to these traces of the past. Furthermore, Ricoeur, who has been included among the phenomenologists (more on this below), expands on the nature and use of historical 'facts':

I have tried to distinguish various manners in which history can be considered a science: first, documentary history, where one can answer *true* or *false* to the presentation of facts: this is the level where we solve questions such as 'How many prisoners were in the Bastille on July 14, 1789?' Next, explanatory history, which includes a discussion on the respective roles of social and economic forces, an evaluation of the place of politics in relation to these, and the narrative element tied to the domain of events. Then, a final level, which I had not encountered at the time I was writing *Time and Narrative*: the level on which are forged the grand categories such as the Renaissance, the French Revolution, which depend more on interpretation and writing, the notion of *historiography*, the writing of history, understood in the strongest sense. Three levels, then, from documentary history, which possesses the criteria of verification, explanatory history, open to controversy, to the history that can be called poetic, since it is one of the great plot constructions (*affabulations*) forming the self-understanding of a nation through its founding narratives (Ricoeur 1998, 85; original emphasis; see also Ricoeur 2000).

This position is certainly consistent with that of both Bruner, noted above, and of Collingwood.

If I may be permitted a few words about our late philosophical and archaeological colleague Robin G. Collingwood: he has been systematically either disparaged or ignored by most archaeologists over the last 50 years. I also note with pleasure that he is definitely not disparaged by Professor Johnson, whether in his book on archaeological theory or in the position paper he presented here. Collingwood's point, and that of Professor Johnson, is that we should focus on the questions that are being asked and the problems that are up for solution. Theory is mobilized in these specific contexts and to these ends. Moreover, as Collingwood stresses, the task of the historian is to turn 'testimony' – the traces of the past that are in the present – into 'evidence' that will serve as answers to the questions posed and offers the empirical part of the solutions to the problem.

The arguments constructed that lead from historical 'testimony' to historical 'evidence' can be either paradigmatic or narrative in Bruner's terms. In each case, A.C. Spaulding's (1960) admonitions about the 'dimensions of archaeology' and the concepts that govern 'samples' and their use come into play. The regulatory notion at this point is that arguments and the construction of their elements must be explicit at each point. It is this message that has been delivered most forcefully by Jean-Claude Gardin over the last 40 years. I regret that he has not received a sympathetic hearing in the English-speaking world – although Gardin's *Archaeological constructs* (Gardin 1980) was written and published in English.

In Gardin's 'logistic' scheme, information languages produce access to materials and texts that serve as the foundations for 'archaeological constructs'; in his terms, these constructs comprise compilations and explanations that lead to an understanding of past human populations and their social and cultural works. There is an explicit, three-part structure to Gardin's programme. First, there is the base of data and its description in an information language. Second, there are the chains of propositions that connect compilations to explanations, evidence to theory, and vice versa. The appropriate language at this point is some form of scientific language – well-formed classifications coupled with valid, logical arguments; premises that entail conclusions and conclusions that match the 'real' world. Third is the study of the scientific process itself: the construction of a meta-language that makes explicit the reasoning processes – the transformations, if you will – that link the various stages of propositional reasoning. To the extent that these constructs can be specified formally, they can become a part of a knowledge base; to the extent that they can affect the structure of the way the data are conceived, they eventually can effect changes in the structure and content of the information language itself. It is the whole of this reflexive process among data, description, classification, models, theories and the human career that Gardin's logistic programme takes as its subject. Moreover, it can be followed in either the paradigmatic or the narrative mode. The crucial factor is the explicit nature of each transformation from premises to conclusions, from explanans to explanandum, or vice versa.

Instead, as Professor Johnson points out, two recent trends in the profession of theory in archaeology lead to methodological individualism and agency theory on the one hand and phenomenology on the other. Neither the one nor the other lends itself to transparent deductive or narrative presentation. In fact, they are at opposite ends of the continuum from hyper-rational to completely sceptical. The former, which has its fullest expression in various parts of economics, posits a completely rational agent who is in possession of complete information, and has been shown to be theoretically wanting (Simon 2001) and empirically false (Henrich *et al.* 2004). The latter, which has its roots in Pyrrhonian scepticism, leads to a 'passive acceptance of the world' in the face of the belief either that knowledge itself is impossible or that there is always insufficient evidence to decide if knowledge is possible (Benson and Stangroom 2006, 24–26).

There are several problems with the use of the notion of agency in archaeology. Professor Johnson points to the most immediate: there is a profound contradiction between archaeological practice and the implications of agency theory. The best efforts by James Bell, a philosopher with interest and expertise in archaeology, in his book *Reconstructing prehistory* (1994) sets methodological individualism within the wider contexts of cognitive and holistic (cultural) approaches in archaeology – in a few words, it cannot stand on its own as a part of archaeological theorizing except as a factor in the falsification of a hypothesis. Finally, methodological individualism and agency theory lead to profoundly problematic lines of investigation. On the

one hand, they are the foundations for 'rational-choice theory', with all of its unwarranted and unrealistic assumptions and, as experimental cross-cultural economics has shown, empirical falsity. On the other hand, sociobiology, with its keystone concept of 'inclusive genetic fitness', explicitly focuses on the individual and its reproductive success, but this position can slip into the worst kinds of conservatism and nihilism. The implications are disturbing at best and destructive at worst.

Phenomenology is a search for 'essences' that are prior to all the distorting 'culture stuff' that conditions experience of the world. The definition adopted by Professor Johnson in his paper is 'the study of lived human experience' (p. 126), but there is much more to it that precludes its use in archaeology. Phenomenology in most of its forms deploys a common method – the eidetic method. As detailed in the *Encyclopedia of phenomenology*,

the attempt to discover the eidē requires, as its first step, a deliberate attempt to redirect attention from the usual focus on the domain of particular facts to an explicit articulation of essences and essential structures. . . . the eidetic epochē and reduction involves a suspension of belief with regard to the actual existence of any objects to be considered from the eidetic perspective (Embree 1997, 168).

It is no accident that Lester Embree, a phenomenological philosopher with an abiding interest in archaeology, analysed the thought-in-action of Gordon Willey as he conducted his Viru Valley research rather than the resulting prehistory of the Viru Valley per se. Moreover, he argues that archaeology is the most basic of the positive sciences, because from his perspective it can be divorced from the distorting effects of 'naturalism' (Embree 1992, 37). Ricoeur's focus on hermeneutics places his thought far from the core of phenomenology, certainly without the deployment of epochē, and his work places him squarely in the centre of contemporary historiography and epistemology (Ricoeur 2004).

It is time to return to explicit arguments in which the standards of judgement are respected and narrative is not confused with paradigmatic science. It is also time to cease using the one as a club to beat the other. Moreover, as Gardin has pointed out in print on numerous occasions (e.g. 1996; 2000), there is no third way. Attempts to reach some compromise, some middle ground, lead to incoherence. Similar conclusions are forcefully argued by Wolf Lepenies in his book *Between literature and science. The rise of sociology* (1988). Finally, although paradigmatic arguments may be embedded in narratives, the standard of judgement for the one is not to be visited on the other, and vice versa.

Perhaps Jerome Bruner says it best:

In the end, then, the narrative and the paradigmatic come to live side by side. All the more reason for us to move toward an understanding of what is involved in telling and understanding great stories, and how it is that stories create a reality of their own – in life as in art (1986, 43).

Questions, theory and concepts *Stephen Plog*

I agree with the thrust of Matthew Johnson's paper; in particular, I concur with his conclusion that there is archaeological theory, but not a 'distinctive way of thinking about the world in theoretical terms specific to archaeology' (p. 117). As Johnson argues, I believe we currently have multiple theories of archaeology and all of these overlap with other disciplines. During a six-year period of administrative service at the University of Virginia in which it was necessary to venture outside the literature that I typically read and review the publications of faculty from a variety of departments as part of promotion and tenure decisions, I was initially often surprised by, and later reminded of, the degree of theoretical commonalities between archaeology and other disciplines that Johnson has highlighted.

I am not an advocate of either phenomenological or agency approaches and will therefore limit my comments to a few other aspects of Johnson's paper that I find particularly important. If there is any aspect of theory that is or should be somewhat unique to archaeology, it concerns the relationship between general theory and, to use Johnson's (p. 120) phrase, 'other elements of archaeological thinking and practice'. Yet, as Johnson argues, it is precisely this aspect of archaeology that is often under-theorized. We have a multitude of theoretical approaches, but remarkably few theoretical programmes that outline how these approaches can best be implemented and tested. The result, as Johnson (p. 118) cogently states, is that there is often 'a disjuncture between what we say we do as "archaeological theorists" and we actually do as archaeologists'. In part as a result of this problem, archaeological case studies too often do not measure up to the 'preceding theoretical excursus' (p. 119) and the resulting conclusions too often seem more like self-fulfilling prophecies than well-reasoned, rigorously tested arguments.

My own research focuses on the Pueblo region of the American south-west – south-western Colorado, south-eastern Utah, north-eastern New Mexico and north-western Arizona – an area with great continuity between Native American history and prehistory and a region where early practitioners played a key role in the development of both method and theory during the first decades of the development of American archaeology (Sabloff and Willey 1980). In addition to the continuity between the past and the present, archaeological studies also are facilitated by several other factors: the arid climate and thus relatively good preservation of materials, the long history of dendrochronology that has greatly enhanced our understanding of cultural chronology and patterns of culture change, and the extraordinary number of both ethnographical and archaeological studies conducted for more than 125 years.

And yet, despite all these assets, contemporary archaeological studies too often illustrate many of the fundamental problems that Johnson identifies as central to the disjuncture between, most notably, the following positions: (1) 'The overwhelming majority of archaeologists continue to divide [and, I

would add, condense] the past and its material remains into cultures, phases and types' (Johnson, p. 123) and (2) such descriptions of the archaeological record emphasize similarity rather than variability. (Although my discussion focuses on the American south-west, I would suggest that these same characterizations apply equally well to most archaeological research in North America and probably to the vast majority of studies in the New World.)

Some of my own studies focusing on the south-west have shown that the type concept is particularly problematical for some eras and that phase-based chronology may significantly distort our understanding of human behaviour and culture change (Plog 1986; 1990; Plog and Hantman 1991). The most common explanation, for example, of significant periods of depopulation in the northern south-west during the mid-12th century and again at the end of the 13th century is that they were caused by climate change. In the most recent such study, Benson, Peterson and Stein (2006, 18) conclude that 'Great House construction and renovation at Chaco and at Aztec [north-western New Mexico] which had accelerated during wet periods terminated near the beginning of the mid-12th century drought' (i.e. at A.D. 1130 (2006, 9)). If one accepts their data on climate change, the alleged correlation seems plausible as stated based on a phase-based reconstruction of demographic patterns. If, however, we use continuous rather than phase-based chronologies to reconstruct demographic change, it is clear that the demographic decline began well before the mid-12th-century drought and continued unabated through both favourable and less favourable climatic conditions. Construction activity in Chaco Canyon, for example, began to decline in the early 12th century and had already dropped quite drastically (Lekson 1984, 263) by the beginning of the drought that Benson, Peterson and Stein allege to have been the cause of decreased construction and depopulation.

Unfortunately, archaeological studies in the 1990s and the beginning of the 21st century have in many ways been characterized by a de-emphasis of measurement and method and, more generally, of the alternative ways that we might characterize the archaeological record in order to provide better evaluations of particular theoretical approaches. Too few archaeologists seem to ask whether the analytical concepts we have typically used in the past serve us all that well in the present, to ask whether the emphasis on similarity that cultures, phases and types create might obscure much of the variability that we claim we want to observe. In particular, as Kristiansen (2004, 80) has recently observed, quantitative methods currently are 'disastrously out of fashion'. And, most ironically, we often find that complex and sophisticated methods of describing and analysing information on climate fluctuations or chemical trace-element frequencies in pottery are juxtaposed with rather crude and imprecise measures characterizing various dimensions of cultural variation, including the fundamental dimensions of spatial and temporal variation. Graphs in Benson, Peterson and Stein (2006, figure 7), for example, illustrate temporal variation in dendroclimatically derived estimates of annual precipitation in considerable detail, but demographic patterns are shown only by very simple and imprecise time blocks representing the duration of construction, or by simple points which designate the date at which

construction ceased. Continuous measures of one variable are thus compared with nominal measures of other variables, yet somehow strong correlations and cause–effect relations are alleged.

As I believe these problems are central to current theoretical debate in archaeology, I also suggest there are additional issues at a much more general level. One of these is the all too common tendency of many of us to either ignore alternative theoretical perspectives – a practice that Schiffer (2000) has referred to as ‘redlining’ – or to reject them virtually in their entirety. Advocates of particular theoretical perspectives too often present them as mutually exclusive alternatives to other perspectives. Kristiansen (2004; see also Hegmon 2003), for a comparable, though not necessarily equivalent, position, has recently highlighted such problems of theoretical closure and has advanced the proposition that no single theoretical perspective can explain all aspects of the archaeological and thus productive exploration of overlapping, hybrid approaches should be encouraged. I strongly concur with this sentiment.

But even more fundamental is the lack of explicit discussion of exactly what questions we are or should be asking. We tend to assume that archaeologists share common goals and are asking similar questions, but I suggest that this has been less and less true over the last two decades. When I read applications of different theoretical perspectives – particularly when we refer to feminist, evolutionary, agency or similar approaches, but also in some cases in discussions of processual, processual-plus, or postprocessual theory – it often strikes me that scholars exploring different perspectives are often asking fundamentally different questions. To put it another way, they may be asking different questions and as a result they find different theoretical approaches to be more informative. However, Kristiansen’s (2004, 95) suggestion that ‘the real strength of archaeology [is] the explanation of historical process and an understanding of how people act and interact through material culture’ is an uncommon example of an explicit reference to the overarching issues that should be guiding our research. Moreover, it is far from clear to me that the majority of contemporary archaeologists share this idea.

We could profit as a discipline from more explicit discussion of the overarching questions we are attempting to answer, from exploring the ways that theoretical perspectives might be integrated to allow more complete answers to these questions, and from focused efforts to remove the disjuncture between questions and some of our most basic analytical concepts.

Undisciplined theory *Adam T. Smith*

Matthew Johnson’s engaging paper raises a number of critical issues for contemporary archaeological reflection. The paper takes as a given the existence of archaeological theory as a disciplinary tradition of scholarly

engagement, as a social fact of the *vita archaeologica*. But Johnson resists, rightly I think, the temptation to define ‘the archaeological’ intellectually, in reference to a discrete analytical terrain over which archaeology holds sovereignty. As a result, a considerable weight is put upon what we might think of as the sociology of theoretical work – concrete practices of theoretical production and reproduction located within institutions such as the university and the department. While Matthew’s paper largely focuses on the gaps within the current constellatory field between the ethos of archaeological training and the pathos that drives our theoretical agenda, in these brief remarks I want to suggest a more undisciplined sense of archaeological theory, one rooted less in the field itself and more in a historically and socially shifting understanding of the pastness of the object world.

One real advantage of resting an affirmation of ‘the archaeological’ on the sociology of the discipline, as Johnson does, is that it focuses attention quite resolutely upon the relationship between various domains of archaeological practice, between how archaeologists think as theorists and what they do as disciplined researchers. Johnson explores the potential conflicts that this disparity can produce to good effect in noting how a mode of thought staked for so long on defining general patterns in material culture can often have terrible difficulties in accounting for the efficacy of human agency in forging these patterns.

However, it is important to note that this problem is in no way unique to archaeology. The determinative power of agents vis-à-vis encompassing historical and social forces has been a constant problem for the social sciences and the humanities, including our close peers in history and art history. The art historian has long grappled with the relative determinative authority to be accorded to the singular creativity of a painter, for example the Berlin painter in Beasley’s famous system, and the cultural genius of a collective subject, such as the Greeks. This conflict played out most visibly in mid-20th-century arguments over the possibility of an artistic left-wing avant-garde. On the one hand, the debate went, artists represent the true needs, desires and historical potency of the revolutionary class. On the other hand, the autonomy of the artist’s vision must be jealously guarded, thus resisting reduction to culture, sociology or biography. Similarly, history was, until the 19th century, primarily an account of the actions of kings, statesmen, generals and other luminaries. Yet the French Revolution in particular forced historians to consider how the intentions of an episode’s main actors might have so little impact upon the course of events.

The struggle within various disciplines, archaeology included, to establish an irreducible subject – the actor, the nation, the people, the culture – is part of a larger struggle across the humanities and social sciences to define, and harness, the complex forces of causation. Debate in historiography over the primacy of these forces has led to a separation in genres of historical writing that distinguish, for example, narrative histories from cultural histories. Hence, while Johnson is correct that how archaeologists work can complicate efforts properly to consider agents, this is not an essential quality of archaeological theory, but rather the product of a long-standing effort across disciplines to identify causation simultaneously in generic forces

and idiosyncratic agents. Given the breadth of this problem across the social sciences, it is worth expanding the question ‘does archaeological theory exist?’ to query whether disciplines like archaeology have ever been able to claim a privileged relationship with a discrete body of theory.

It seems that very few disciplines in the humanities and social sciences can boast of an entirely autonomous apparatus of interpretation. The new archaeology arose out of close links to sociocultural anthropology simultaneous with the emergence of processual theory in disparate disciplines ranging from geography (the new geography), history (e.g. world systems) and sociology. It is thus not surprising that the reaction against processualism was similarly broad, drawing postmodernism out of architecture and literary criticism to reorient the constitution of knowledge in most of the human sciences. This fertile interdisciplinarity is entirely a good thing which allows for the very sort of cross-pollination that has repeatedly in archaeology’s disciplinary history shaken the field out of either an orthodoxy of practice or an intellectual torpor.

Thus there is little satisfaction in resting ‘the archaeological’ solely upon the structures of the profession. Not only does such a move run against the field’s deeply rooted interdisciplinarity, it also tends to dehistoricize our practice, leaving us with little sense of how such an odd form of research as archaeology ever came about. Nor does this formulation do justice to a more general, informal – what we might call ‘vernacular’ – sense of archaeological theory lodged far beyond the strictly policed fora of the journal and the department. How might we accommodate Johan Huizinga’s observation, penned in the margins of a 1928 lecture, that ‘history is the form in which a culture becomes conscious of its past’ (quoted in Ankersmit 2001, 1). Here the historical is defined in reference to neither a unity of theory and data nor a profession, but rather to a specific imagination of the past rooted in a unique time and place.

Huizinga’s remark implies something quite important about the historical imagination, a reflection on the past that can have, at certain moments and places, a unique disciplined formulation, but which roams more broadly in vernacular forms as well. We avoid reducing ‘the archaeological’ to the profession if we break away from the limitations of the field to understand archaeological theory as *the form in which a culture expresses its consciousness of the pastness of its things*. What follows if we examine archaeological theory not as a potentially autonomous field but rather as constituted relationally, as an ongoing reflection upon the materiality of the past? In this sense, the question ‘does archaeological theory exist?’ might be rephrased as ‘what intellectual forces and sociological positions make theories archaeological at a given moment and in a given place?’ Not only does this formulation force us to take seriously potential vernacular expressions of archaeological theory, it also allows for a broader sense of archaeology’s disciplinary tradition than we currently enjoy. Let me provide two very brief examples.

Mikhail Lermontov’s 1840 novel *A hero of our times* (2004), a tale of imperialism and disenchantment set in the Russian north Caucasus, contains an intriguing passing anecdote. As a baggage train passes through a typically rugged windswept pass in the central Caucasus mountains, the narrator looks upon a stone cross set atop a mountain and reflects,

There exists a strange legend about this cross, that it was set there by Emperor Peter I when he was traveling through the Caucasus. In the first place, though, Peter was only in Daghestan [far to the east], and secondly, written on the cross in large letters is an explanation that it was placed there at the order of General Ermolov, and specifically in 1824. But the legend, despite the inscription, has taken such firm root that indeed you don't know which to believe, especially since we are not used to trusting inscriptions (Lermontov 2004, 31).

What makes this episode resonate as a vernacular form of archaeological thinking buried deep in the literary imagination of the mid-19th century? The suspicion of texts today is quite self-consciously central to archaeology's *raison d'être*, whether as a critical reflection upon the written word's limited temporal reach or upon its inherent limitations as a representational genre. These justifications often appear to be simply efforts to make the best out of archaeology's difficult epistemic situation vis-à-vis history and sociocultural anthropology, but the accordance of an analytical privilege to things over texts has shaped not just the genealogy of the field but a far broader understanding of the materiality of the past – what we might refer to as an 'archaeological imagination'.

Let me provide a second example. Giambattista Vico opened his *New science* (1744 edition) with an explication of the remarkable frontispiece shown in figure 1. Metaphysics, standing atop a sphere, reflects a ray of light descending from an eye set in a floating pyramid onto a sculpture of Homer. The ground around the statue is littered with artefacts and the ruins of monuments, each of which symbolizes an institution of human civil life. Vico explains,

the foreground is the brightest lit plane of all for it displays the symbols of the best-known human institutions. . . . The first of these symbols is the Roman fasces because the first civil authorities grew from the union of the fathers' paternal powers . . . the sword rests on the fasces because heroic law, while a law of force, was tempered by religion . . . Duels were fought with appeals to the certainty of divine judgments . . . the symbol of the purse . . . represents the origin of coined money . . . After the purse, we see a balance, which indicates that after the heroic governments, which were aristocratic, came what I call human governments, which were initially democratic (Vico 2001, 15–20).

What is it in Vico's frontispiece that calls to mind an archaeological sense of reflection upon the past? The materiality of the past – its endurance within the object world – provides a recognizably archaeological disposition in so far as things are read as representative of a host of social institutions and cultural practices. Is not Vico's sense of the past also an imagination of the pastness of things and thus another form in which the archaeological imagination is contained? If both Vico and Lermontov are caught within an imagination of the pastness of things, then they are, in effect, forwarding forms of archaeological theory which are not subsumable to the strict confines of the discipline.

Genealogies of archaeological thought and practice which wander beyond the limited confines of the discipline do not in themselves destabilize claims



Figure 1 Frontispiece of the 1744 edition of Giambattista Vico's *New science*.

to an archaeological body of theory in the present so much as they highlight the emergent nature of archaeological imaginations. Hence the link which Johnson establishes in his paper between contemporary phenomenological approaches and 19th-century British Romanticism does not diminish the integrity of modern archaeological theory so much as it locates a powerful vernacular archaeological imagination beyond the traditional history of the discipline. Indeed, studies by Schnapp (1997), Barkan (1999) and Marchand (1996), among others, have shown not only the diverse sources of archaeological investigations but also the power of genealogies that stray beyond the traditional disciplinary paths to locate other imaginations of the pastness of things.

I am not arguing here that everyone qualifies as an archaeological theoretician, nor that the vaguest of musings upon the material world might stand as naive forms of theoretical production. While I admire the democratic impulse behind such moves, such a position ultimately obscures deeply ingrained power yielded to specific sites of cultural production. That is, within a social regime staked on inequality, it is rather perverse to suggest that all hold equal powers of reflection to generate theories of the pastness of our objects. If efforts to ground archaeological theory solely within the institutional structures of the discipline are too confining, then suggestions that archaeological theory is, in effect, everywhere, is to forget about the very real privilege accorded some locations of theoretical reflection over others. Although I have suggested the existence of vernacular forms of archaeological theory in both Vico and Lermontov, both writers occupy rather restricted social positions defined by both class and access to the means of cultural production. And yet it is equally clear, I think, that both provide examples of how recognizably archaeological theories are not always subsumable within the traditional boundaries of the discipline.

Archaeologists have long been quite used to having our arguably obscure field all to ourselves. That is, once the antiquarians were thoroughly discredited as authoritative interpreters of the materiality of the past, archaeology effectively stole the show, disciplined its knowledge and retreated into the world of culture history and determinative systems. And yet the vernacular archaeological imagination endures, shifts, ebbs and flows in ways that are critical to both what we do and how we reflect upon our objects. Does archaeological theory exist? It does, but not solely within archaeology.

On being heard. Theory as an archaeological practice

Silvia Tomášková

Suggesting that archaeological theory both exists and does not at the same time is a wise move, so long as it opens the possibility for a range of answers and dialogue. In that respect I find myself in an agreement with Johnson,

given that he takes up a piece of the theoretical minefield, and leaves plenty for others to engage with. Rather than debate his argument *per se*, therefore, I will endeavour to enlarge his essay in ways that will both contrast my position with his and further a greater conversation.

First, there is the matter of historical depth. Theory in archaeology did not spring up in the 1960s with the new archaeology, as Johnson seems to imply, even if he qualifies the statement by stating that explicit discussion of theory is recent. Theoretical archaeology may have become a favourite battleground in Britain in the 1970s, later exported to other places, but concern with theory has been with us for a long time on both sides of the Atlantic. Whether one reads Putnam and McGee's discussion of the Moundbuilders and American Palaeolithic at the end of the 19th century (Meltzer 1985), Nelson, Kidder or Kroeber in the early 20th century, or V. Gordon Childe's Marxist views on the emergence of the Neolithic, they were as theoretical as anything written on the topic today, even if these writings do not resonate with our current sensibilities. The struggle over evolutionary theory and materialism in archaeology in France at the end of the 19th century and turn of the 20th deserves a far more prominent place in the history of the discipline as a fascinating example of a fusion between science and politics. Finally, C.J. Thomsen's central contribution to archaeological theory from the margins of Denmark still awaits serious treatment from science studies. The invention of prehistory might serve as a perfect example of the way that theory travels, and the crucial importance of networks, connections and communications between scientists in the development of any knowledge claims.

In evoking this varied history, it is also important to explore greater geography than that circumscribed by the English language, dominant as it may currently be. First of all we need to recognize significant variation within English-language contexts. An initial step would be to finally disentangle 'Anglo-American archaeological theory', recognizing significant differences between theoretical landscapes in North America and Britain. That archaeology emerged quite differently in North America than in 19th-century Europe may be common knowledge in our summary histories. Yet the point seems lost in debates about theory, separating the context of origin from current issues as if they were not connected. For example evolutionary archaeology, a prominent fixture of North American archaeological debates, resonates far less in European contexts (Shennan's contributions notwithstanding). There are a host of potential reasons for this, including differences in the institutional settings of many practitioners (e.g. departments of anthropology with a strong physical and biological component) as well as differences in sociocultural and political trends in Europe and the United States. The same case can be made for the degree of inclusion or exclusion of feminist theories amid archaeological debates in different cultural and political contexts. 'Anglo-American theoretical archaeology' may be convenient shorthand, but it simultaneously masks crucial differences and prevents us from more deeply understanding how theoretical seeds have flourished or not in different landscapes.

We also need to distinguish between different theoretical approaches written in English as a second, scholarly tongue, and originating in a variety of

political and geographical contexts, such as Greece, Scandinavia, Holland or Latin America. Precisely because English has become the dominant language of theoretical discussions in archaeology we should give greater thought to the native-/non-native-speaker distinction when addressing diverse voices in debates. What is the effect of thinking in translation, of expressing one's thoughts between languages, thereby between cultures, when English landscapes, be they Romantic or phenomenological, are truly foreign to some of us? Theory is a kind of language that allows us to communicate about topics of common interest. The tools we use to access it, however, are local only to some, and we have to acquire additional skills to be able to participate in this communication. English dominance also results in attenuated connection to theories in different languages, and the possibility of structured ignorance, whereby work elsewhere is simply ignored. Do theories have to be expressed in English to be noticed or discussed, or to become central? Meticulous and innovative French research in the history of human sciences includes histories of theories in archaeology (e.g. Blanckaert, Duclos and Hublin 1990; Cohen and Hublin 1989; Richard 1993), but remains untranslated. As a consequence, aside from Schnapp's work (1997), it has received little notice in the English-speaking world. I am certain that the same could be said for publications in many other languages. Veit, for example, draws our attention to 'a wholly new approach to the past which is at the same time historic and materialistic', while 'adaptations of those ideas to the field of archaeology are still rare' (Veit 2004, 102). I intend to consult the literature that he cites, although only a few libraries in the entire United States own one of the books (Ebeling and Altekamp 2004). My students, however, will not, due to the simple fact that it is in German. Thus when Johnson speaks of the dearth of theory in other areas of the globe, he might want to explore the topic further than such a dismissive generalization allows him. To provide an example with which I am familiar, Eastern Europe is certainly not in the theoretical forefront. But, as anthropologists, should we not wonder why that may be the case? The well-trodden concept of centre and periphery might be a start, but surely we should ask additional questions such as how a periphery becomes peripheral, and what keeps it there. After the fall of communism in the 1990s newly optimistic students of archaeology in Prague read Hodder's *Reading the past* (1991) with great enthusiasm. By 'read' I mean the sort of painstaking work that requires dictionary in hand, or entails deciphering of photocopies of poorly translated versions. For this was required simply to 'catch up' with theory, let alone enter into a possible dialogue. Language, historical and political barriers are some of the central components of networks that influence when and how theories do or do not travel. Thus, if we wish to speak of a diversity of voices in archaeological theory, we need to address the literal meaning of speaking and voices and ask, what language are they speaking, and are we communicating or just spreading the message? To really examine the existence of archaeological theory, we would need to cross a range of boundaries, and lacking omniscience could only do so collectively. The answer to the question 'does archaeological theory exist' should then start with a set of counter-questions that asks about the place and history of the theory in question, so as to avoid the trap of universalizing (and dismissive) statements.

Finally, I wish to stress that theory is itself a form of practice. Viewed this way, knowledge is never a set of disembodied ideas floating in a vacuum. Rather, as several decades of research in science studies suggest, knowledge emerges within histories, political contexts and networks of participants. Thus we need to ask not only where archaeological theories come from (as Johnson does in his discussion of phenomenology), but also why they might have emerged at a particular time and who actively engaged in their discussion. Most importantly we should consider who does and who does not participate, and what it takes to count as a practitioner and produce appropriate work. I would find Johnson's dissection of agency theory or fondness for phenomenology far more convincing had he given us some sense of the practice of these theoretical approaches. As it stands, his piece provides little social or technical context for thinking beyond personal preference, or considering how these may have developed.

Theory, after all, is a form of intellectual work, and may be viewed in terms of a more general division of labour, analogous to that between fieldwork and laboratory practices (Gero 1985; 1994; 1996). The admission of race, class, gender and sexuality as categories of analysis encourages the recognition of a situated subject, one quite unlike the disembodied mind performing, in Donna Haraway's words, 'the god trick' of omniscience (Haraway 1988). Feminists have particularly been concerned with the relation of gender to theory, perceiving the latter domain as a preserve of masculinist assumption beneath its cloak of neutral objectivity. For example, theoretical archaeologists in Britain have engaged in a reflexive critique of archaeology, revealing a long tradition of participation in colonial projects and justification of Western domination through the images of savages lacking in civilization. Most of the critique has been carried out through versions of Marxist analyses of imperialism and domination of the Third World, implicating archaeology and arguing for the need for multiple voices and views of the past (e.g. Bapty and Yates 1990; Hamilakis 1999; Kristiansen and Rowlands 1998; Shanks and Tilley 1987b; 1992). Yet as Ericka Engelstad has pointed out (1991; see also a poignant discussion of the writings in Gilchrist 1991), this critique has not included feminist views. Despite all the calls for multivocality, the sex, class and race of the speakers remained uniform. Gender, for the most part, was not incorporated as a crucial contextual strand in this rebellion against intellectual fathers. While this critical appraisal of archaeology provoked both enthusiasm and bitter reaction from the keepers of the faith, it has given little thought to feminism as a transformative force. In addition, the reflexive acknowledgement of context in archaeology tends to be ironically shallow in historical terms. Focused on the positional concerns of the author, the past strikingly resembles the present in many accounts, featuring the same concerns, obstacles, hierarchies and exploitation of the weak, along with inspirational acts of resistance (e.g. Hamilakis 1999; Hodder 1999; Miller, Rowlands and Tilley 1989).

Here a nod to etymology might be of use. The term 'theory' derives from the Greek *theoria*, meaning contemplation, and pertains to the human experience of seeing, to the field of vision. This became specified in a later period (Descartes) as a vision of true and universal objects and ideas perceived

with the eye of the mind. Considering vision a sensory experience helps us to think about theory as practice, as it allows for a reflection on practitioners, the privileging of certain eyes, and certain fields of vision. Lutz (1995) addresses this issue specifically through examination of the gender of theory in cultural anthropology. By reviewing edited volumes considered central anthropological writings, course syllabi of theory seminars, and journal citations, she concludes that women's writing is not considered theoretical and, if included, is seen as 'merely' setting the record straight (Lutz 1995, 251). Feminist writing is held separate from discussion of theory, creating a clear hierarchy that links the practitioners to a particular form of thinking and writing:

The valorization of a particular genre of writing (theory) as the top layer in a hierarchy of kinds of discourse has also marginalized work by gay and minority scholars The struggle over the nature and value of theory is just one example of the more general process of group struggle via the canons of taste in cultural objects (Lutz 1995, 260).

Lutz thus not only points to the masculinity of theory but also to the division of labour between 'thinkers and doers', and to the perpetuation of this hierarchy through the consumption of theory as an opaque, difficult subject suitable only for the top minds among us.

When asking whether archaeological theory exists, we may then do well to step back and consider what theory might have been in the past, and what it might be in the present. Taking theory as a form of practice, and approaching it with a greater awareness of divides of difference such as gender, our goal would not be to be specific in revealing our positivist or constructivist leanings but rather to survey the theoretical landscape, to see who is participating in this dialogue and whether they are being heard. If we accept that theory is historical practice of contemplation, we should recognize that under different circumstances contemplation may or may not result in dialogue. Rather than seeking to build a singular archaeological theory, we would do better to examine the contexts of theoretical practice, and admit a broader range of practitioners into the conversation. The world of theoretical archaeology may already have plenty of views; they just need to be recognized, and heard in public more often.

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Archaeology and theoretical culture *Matthew H. Johnson*

First, a qualifier: the 'response-to-the-response' section of *Archaeological dialogues* is often the most entertaining part of the issue, but frequently the least intellectually rewarding. One feels *Schadenfreude*, partisan cheering and jeering, and bafflement in equal measure as contributors lock horns over this

or that finer point of who has misrepresented whom. I am anxious to avoid readers reacting to what follows in such a manner, and so will concentrate on a very selective range of raised points that, to put it crudely, got me thinking.

Let me start by restating the aim of my paper. My argument is that ‘the relationship between explicit archaeological theory and other elements of archaeological thinking and practice has itself been under-theorized. Specifically, there exists a complex, shifting and historically particular relationship between different elements of the craft of archaeology’ (p. 120). The central thrust of my argument was to point out the way archaeological ‘theory’ frames itself, chooses to foreground certain discourses, and in particular does so within a certain academic and ‘practical’ context. As Smith rightly comments, my argument does ‘not diminish the integrity of modern archaeological theory so much as it locates a powerful vernacular archaeological imagination beyond the traditional history of the discipline’ (p. 163).

Three issues with the thrust of my paper arise from this point. First, for Coudart, there is a distinction to be drawn between theoretical archaeology and archaeological theory. She characterizes the latter as either a set of principles leading to a given practice or ethics, or ‘a discourse that describes the specificities of a particular domain’ (p. 134); my definition, she asserts, tends towards the latter view, and thus implicitly classifies archaeology as one of the liberal arts. I can see Coudart’s logic but feel it is rather typological. It takes statements made by the sciences about the way they produce theories at face value, and assumes an a-priori division of knowledge and the methods by which we go about constructing knowledge between the sciences and the arts. As my paper stated, even the mildest form of social constructivism (as for example advocated by Coudart’s friend Bruno Latour) tends to deconstruct these a-priori and face-value evaluations of scientific knowledge. Second, for Olsen, whatever its overtly stated aim, my paper is not really about the relationship between ‘theoretical’ and other kinds of discourse in archaeology, but about the philosophical essence of agency theory and phenomenology. He judges my paper against this latter aim and finds it wanting. The reader might care to ask, why does Olsen choose to redefine the argument of the paper in this way, contrary to my stated purpose? Third, I am puzzled by Tomášková’s assertion that I provide ‘little social or technical context for thinking’ (p. 166) – the aim of the two case studies was to put agency and phenomenology in the frame of a wider context, for example of field practice.

To turn to agency, Smith points out that archaeology is not alone in its anxieties over the understanding of agency, and points to parallel debates in other disciplines. He is right to imply that these other disciplines should be acknowledged in a full discussion of the issue, but I maintain that archaeology nevertheless has a particular configuration of anxieties in this regard. These stem in part from its material and prehistoric nature – as Smith acknowledges a little later, the suspicion of texts is central to archaeology’s *raison d’être*. Art history and literary history frequently structure their discussions of agency in terms of the way their disciplines understand and apprehend the intentionality of a named author or artist. Where the production of archaeological theory,

then, stems in part from a struggle to understand agency, the output of disciplines such as literary history stems in part from a struggle on the part of the more theoretically aware in those disciplines to deny a simplistic conception of agency via the assertion of authorial intention. I am conscious of this from my position as a historical archaeologist and in particular from my research on historic buildings, where I have frequently had to assert the difficult and problematic nature of agency in the face of other authors' blithe and confident assurance that 'this castle was built by so-and-so, and we know from this document that he did so for defence/to assert his status' (Johnson 2002, particularly 181–82). For these and other reasons, I respectfully disagree with Peebles's implied conflation of agency theory with methodological individualism; they are intellectually related, but very different creatures. Drawing attention to some of the difficulties of agency theory and practice cannot plausibly be held (as Olsen asserts) to imply a conspiratorial view of archaeological practice, or to deny credit to the work of Hodder and others in their (only partially successful) attempt to challenge the structures of field practice.

As stated earlier, Olsen fails to focus on the stated aim of the paper, and as a result he misses the thrust of my comments on both agency theory and phenomenology. There is no attempt in my paper to trivialize any scholar's position. I find Coudart's description of archaeology as a single *chaîne opératoire* helpful here: to understand one link in the chain, one has to look at the other linkages. To point out these linkages is not to 'black-box' connections and discourses in any way. Underlying Olsen's distaste for the simplicity and brevity of my arguments is a deeper divide that has rarely surfaced overtly but, I feel, will be important to explore when the history of postprocessual thought comes to be written. From the start there was a tension over theoretical depth and purism on the one hand, and attempts to engage in a more eclectic and varied manner on the other. This tension had and continues to have a gendered dimension to it; Tomášková's points on theory-as-masculine are well taken in this regard (cf. Bapty and Yates 1990; 'versus' Baker and Thomas 1990; and Sarah Taylor's critique of 'brothers in arms' in that latter volume). Olsen is clearly on the purist side of this divide and I have always found myself on the other. His position may be rigorously engaged with the higher tenets of phenomenology but it runs the risk of theoretical opacity and sectarianism; he is in danger of locking himself into his own black box in which the only possible critical position must be prefaced by a depth of philosophical immersion which precludes debate with all but an exclusive brotherhood. It invites offhand dismissal from non-believers, for example Peebles and Coudart (I found Coudart's suggestion of a link between public-school repression and a stress on bodily experience quite wicked, but I do think she is onto something). I do not agree with Peebles's comments on phenomenology, but what is revealing here is the ease with which Peebles can categorize phenomenological thinking and then swiftly despatch it to the intellectual dustbin. Coudart engages in a parallel dismissal of Derrida and others, though she is a little disingenuous here; if it is really true, it is difficult to see why the 1997 attack of Sokal and Bricmont on Derrida and other poststructuralists caused such a storm.

My approach, on the other hand, may, I hope, win hearts and minds, engage with the concerns of theory with a wider audience, and persuade an often sceptical body of students and practitioners of the importance and relevance of theoretical engagement, but it does run the risk of oversimplification of what are necessarily very deep, serious and complex issues. Engagement with theory is necessarily open-ended and always invites accusations of lack of depth; as Culler writes,

the completion of one task brings not respite but further difficult assignments: ‘Spivak? Yes, but have you read Benita Parry’s critique of Spivak and her response?’ ... to admit the importance of theory is to make an open-ended commitment, to leave yourself in a position where there are always things you don’t know’ (Culler 1997, 16).

We see Culler’s point when both Peebles and Coudart offer very different versions and assessments of phenomenology than that of Olsen; this does not mean that Olsen is ‘wrong’, rather that phenomenology, like so much theory, is open-ended and leads in a diversity of directions. In such a context, accusations of trivialization are not very helpful. There is nothing wrong with either a position of theoretical purity or one that is more ‘vernacular’, eclectic and engaged with a wider field – both are individual responses to a difficult choice, and most archaeological theorists position themselves somewhere along a spectrum of this kind – but both have obvious weaknesses. As such, they require self-reflection and review.

Such a review has to start with the goals of archaeology. I agree with Stephen Plog that there is a striking diversity of goals, rather than a common goal to which all archaeologists subscribe. Plog implicitly deplores this situation, and Coudart’s argument tends in a similar direction; I see their point, but find I have a more mixed reaction. One of the lessons that has been learnt from feminist critiques such as those discussed by Tomášková is that generalizing ‘common goals’ of this kind all too frequently serve to silence or marginalize certain viewpoints, especially those of women. This lesson is well worn but perhaps needs repeating. An arguably more recent and emergent critique, arising from indigenous archaeology, suggests that the Western encounter with the archaeology of ‘native peoples’ in search of such a common goal all too frequently turns their past and the archaeology of that past into a playground for Western science. In respect to their construction as ‘marginal’ by Western knowledge and to their opposition to much of established discourse, and in their attempt to construct a different way(s) of telling the past, feminist and indigenous archaeologies share some important intersections (Conkey 2006).

The central thrust of such positions is, to my mind, irrefutable. Nevertheless, it leaves me uneasy (given that I write from the standpoint of a white, Western male, some of the sources of that unease are not difficult to guess at). In part, I feel that my unease at some of the consequences of this diversity underly the apparent conservatism that Leone playfully, but quite correctly, identifies in my paper. I cannot lose sight of a basic curiosity and confidence that underpins why I do archaeology – curiosity in general questions about human existence, and confidence in the ability

of archaeology to sustain serious and empirically informed attempts at answering them. After the poststructuralist critique of ‘origins’, these general questions might be better characterized as foundational histories, as Leone characterizes them, rather than as essences or points of origin. To expand on the point I made in the conclusion of the paper with respect to Smith (2004) and his characterization of the ‘end of the essential archaeological subject’, archaeology stakes much of its claim to be important, to command intellectual attention and respect, on its ability to address very general questions of humanity and historical process. Implicitly Smith, and Kohl in his response to Smith in that issue, endorse the position that many archaeologists have lost sight of these general questions and archaeologists’ responsibilities in this regard. Such comments have echoes of Richard Bradley’s (1993) complaint of archaeology’s loss of nerve.

None of these assertions, I think, are necessarily or logically inconsistent with or contradictory to a position that asserts the importance of feminist and indigenous archaeologies as projects, however much a generalizing perspective has been historically complicit with their marginalization. Indeed, the terrain of a really responsible, serious and (arguably) mature archaeology ought to be (at least in part) on the intersection of these terrains. I feel this is where Leone’s fascinating story leads us, to the physical intersection of Steno as geologist and Steno as saint. Smith’s elegant argument in his response reaches this position also, I think, albeit by a rather different route, and I now consider his comments before exploring this issue further.

Smith gently and implicitly reproves me for stressing an overly sociological view of ‘the archaeological’. He is correct to point to the dangers inherent in such a position of denying interdisciplinarity and historicity. His definition of archaeological theory as ‘the form in which a culture expresses its consciousness of the pastness of its things’ (p. 160) is exciting and thought-provoking, not least because it opens up a space in which to take what Smith terms ‘vernacular expressions of archaeological theory’ (p. 160) critically and seriously. I would go further and suggest it offers the potential to avoid some of the rather sterile attempts to ring-fence what is or is not proper archaeology (discussed by Hodder 1999, 15–17) or the attempts to locate archaeology in this or that defined space within a disciplinary configuration.

The exploration of theoretical boundaries – what is or is not archaeology or archaeological theory – becomes, then, not an exercise in ring-fencing or ruling ‘out of court’ this or that approach, a position implicit in Peebles’s and Coudart’s contrast between science and other forms of knowledge, but rather an empirically engaging one, particularly with reference (as Smith notes) to structures of power. I would like to think that it addresses Tomášková’s insistence that what we need is to recognize other views, and to hear them in public more often. To put it another way, the question is not ‘is this proper archaeology?’, but rather ‘what are the intellectual, political, cultural structures that lie behind the definition of archaeology in this manner?’ I think that such a construction addresses Coudart’s concern that some theoretical positions are not simply ones to be picked up at will from a supermarket shelf but need a more critical evaluation, and it addresses Tomášková’s related point that to enquire about the existence of archaeological theory is to ask

a range of counter-questions about place and history, for example political questions about the dominance of language.

It is on this terrain that a critical engagement with indigenous archaeologies lies, as noted above. Such a terrain occupies a very productive location: between, on the one hand, an offhand dismissal or redlining of other ways of knowing in the name of Science and, on the other, a superficially tolerant ‘plurality’ of which Coudart is rightly suspicious and which too easily slides into a patronizing refusal seriously to engage critically with other positions. Such a ground also rests on an empirically informed understanding of the genealogy of different forms of knowledge and practice (yet again, archaeologists are led back to the work of Foucault).

Let me make this point in a slightly different way. As I was reading the different responses and considering what I wanted to say in reply, the first issue of *Archaeologies. Journal of the World Archaeological Congress* arrived, containing an article and responses strikingly similar in format to the one you are reading now in *Archaeological dialogues*. The responses to the review article by Meg Conkey are all thoughtful and serious, but all come from a broad spectrum of agreement with the basic premises of her position, which is both feminist and supportive of indigenous perspectives. There is little explicit engagement here with a conception of archaeology as a generalizing and scientific discipline. But the different positions here are clearly dialectically related and indeed mutually involved in the construction of new forms of knowledge, for various reasons, the most obvious of which is because we all write within the living cliché of a global community.

Dorothy Lippert hints at such a position when she writes in her response to Conkey, ‘Native Americans who chose archaeology know that we were reaching for the master’s tools, but rather than choosing to dismantle the house, we may instead be constructing a new home under which we will invite everyone to shelter’ (Lippert 2006, 65). She identifies a sense of ancestry and shared experience as central to the intersection of feminist and indigenous perspectives, and raises the possibility that everyone may be invited to join. A partial response would be that a sense of ancestry and shared experience have often been part of archaeological traditions not normally classified as either feminist or indigenous, but that the scholars involved have not been self-conscious or self-critical in this regard; they have attempted to wrap what is actually an empathetic and emotional engagement with the past in a cloak of objectivity (as my paper briefly indicates is true for an enduring tradition of British encounters with the landscape, most obviously Stukeley). This deeper pattern of sentiment and motivation has often been ‘written out’ of conventional histories of archaeology (Johnson 2007, 193–94).

Where does this leave archaeological theory? As ‘the order we put facts in’, it is inclusive of ontology unless one believes that such a definition leaves the status of facts unquestioned – which it clearly does not. As ‘the form in which a culture expresses its consciousness of the pastness of its things’, it has the strength of encompassing vernacular understandings of archaeology. Let me make a third suggestion, following Tomášková: theory is the language archaeologists use to communicate with each other. Language is always a form of power; it is loaded, value-laden; it is contested terrain; and just as

everyday culture fights its wars out over common language, so archaeologists battle over the discipline in the words they use.

Tomášková draws attention to the importance of language. I am grateful that her comments give me the opportunity to clarify: I never consciously intended to make dismissive statements about theory outside the English-speaking world. Any dismissive sentiment of this kind would stand in contradiction to the challenging and rewarding dialogues I have had with scholars from many different areas outside the anglophone world, particularly younger scholars and students. The passage she refers to was intended as a ‘qualifier’ to make the necessary acknowledgement that different situations pertained in different parts of the globe and in different areas of archaeology, rather than to assume the experience of the English-speaking academic to be a universal one. As such, I think its point stands, but I accept that such a short statement needs amplifying along the lines she proposes. Tomášková also points to critical differences between the English and American worlds, and in particular to ‘native’ versus ‘non-native’ uses of language. Her arguments remind me of Easthope’s elegant and witty discussion of how the values of empiricism and English national culture are bound up together in a certain pattern and style of writing in contemporary English, which Easthope traces back to the common origin of 17th-century essay-writing, the empiricist philosophy of Bacon and others, and the formation of the early modern nation state (and indeed other related concepts such as the prioritizing of ‘masculine’ discourse; Easthope 1999). If Easthope is correct, and I think he is, we might care to ask, is it the case that, insofar as English has achieved the dominance identified by Tomášková, to the exclusion of other voices, values of certain kinds have also inscribed themselves, to the exclusion of others? There are insights to be had here from postcolonial thinking on the use of the dominant language.

To conclude: I am grateful – let me thank all the respondents for their thoughtful and critical comments, from which I learnt much. The purpose of my paper, as requested by the editors, was to initiate and inspire debate, and I am glad it has succeeded insofar as it would take a book to respond adequately to all the points raised by the respondents. This issue of *Archaeological dialogues* demonstrates that, of all the words at stake in archaeological discourse, ‘theory’ is one of the most contested, and that this contestation is part of what makes our discipline so difficult, so exciting and so intellectually vibrant.

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