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The origins of culture history in prehistoric archaeology: rethinking plausibility and disciplinary tradition

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ABSTRACT

This paper explores the fundamental question of what makes archaeological accounts plausible. By using a worked example related to the rise of culture-historical approaches to archaeology during the late nineteenth century, I conclude that the conditions of plausibility have frequently been, and continue to be, opaque, requiring detailed analysis by practitioners.

Introduction

After a long period at the margins of archaeology, the history of archaeology has begun to find its way into the disciplinary mainstream (Moro and Huth 2013; Murray 2002, 2014; Murray and Evans 2007; Schlanger 2002). There are now more histories of archaeology, its practitioners, institutions and the cultural and social contexts of its practice than ever before. Significantly there has also been an increasing interest in the historiography of archaeology and antiquarianism, inspired in part by the work of Alain Schnapp (1996) and the late Bruce Trigger (2006). The encounter between archaeology and philosophy has followed a somewhat different pathway with sporadic interest among archaeologists in the philosophy of science and to a lesser extent the history, philosophy and social studies of science (see, e.g., Wylie 1982). This interest was prompted by the adoption of positivist epistemologies in the early days of the new archaeology (at which time it was linked to accounts of scientific change flowing from Popper, Kuhn, Lakatos and, later, Feyerabend), which over the past thirty-five years has gradually morphed into a variety of positivist and post-positivist accounts of archaeological epistemology. Nonetheless, a focus on the value of the history of archaeology in fostering critical reflection about the fundamental structures of our discipline is still in its infancy, and my purpose here is to contribute to this reflection by arguing for the importance of the history of archaeology for the conduct of two fundamental disciplinary activities: first, the formulation and assessment of archaeological theory; second, the discussion of archaeological epistemology and archaeological metaphysics that together comprise the foundations of a coherent philosophy of archaeology.

My necessarily general and superficial argument springs from the now quite banal observations that archaeologists rarely build archaeological theory and that even those interpretative or explanatory systems they frequently borrow (from sources such as anthropology, biology, geography or cultural studies) are rarely adapted or transformed through their contact with

archaeological phenomena. I take these to be worrying signs that raise issues about the significance of archaeological data and the possibility of achieving an engagement with the past that does more than simply confirm contemporary social or cultural orthodoxies (or current *un*orthodoxies). Whether the structural properties of archaeological data can or should constrain our interpretations or explanations of them is an issue that resonates far beyond a conventional antinomy between the statuses of empirical and of theoretical knowledge. At stake are our capacity to reflect critically about the role of archaeology in society, our capacity to convincingly explore alternative pasts and presents and, of course, our capacity to speak honestly and directly about the ways we seek understanding of archaeological phenomena.

My discussion here is based on a brief and partial consideration of a complex question: what makes archaeological interpretations or explanations convincing or believable? Analysis of the history of archaeology, and comparison with the histories of sciences such as geology and physics, indicates that, notwithstanding appeals to determinate rules of scientific method, the primary basis of conviction is the cognitive plausibility of the interpretations or explanations on offer. I will exemplify this argument and consider its implications through a brief discussion of the origins of culture historical archaeology in nineteenth-century Europe.

At this point I need to stress that I am *not* arguing that archaeology is a discipline where interpretation is completely unconstrained by the empirical. Even the most superficial accounts of the history of archaeology make much of the impact of core discoveries such as scientific chronology, the reconstruction of past environments, our capacity to chart hominid evolution and systems of classification and typology. Indeed, the major expansion of research in the many facets of archaeological science and the management of very large archaeological databases occurring over the last thirty-five years have in many cases led practitioners to question their theoretical inheritance more frequently.

Plausibility and tradition

One of the consequences of the turn towards critical self-reflection in archaeology has been that we now understand more about the power of tradition. Tradition guides the socialization of practitioners (especially in matters related to the goals of archaeology, problem selection, methodology and assessments of the plausibility of knowledge claims). Tradition also structures the terms in which practitioners of disciplines cognate to archaeology establish the meaning or plausibility of archaeological knowledge claims. In this sense tradition both oversees the production and the legitimation of archaeological knowledge.

Over the last thirty-five years disciplinary traditions have come under close scrutiny from practitioners operating under increasingly divergent epistemic and theoretical regimes. All sides in contemporary debates about the goals of archaeology and the nature of the discipline have noted the tendency for attitudes, concepts and categories to survive the process of 'doing archaeology' often unscathed. The result has been an increasingly diverse discipline where there is little effective consensus about how to evaluate knowledge claims or the utility of interpretative perspectives. I have previously described the current state of archaeological theory as increasingly atomized, rather than being simply polarized between long-running epistemic antinomies.

A variety of epistemic and theoretical evaluative strategies have been tried out during this period. Epistemic strategies have tended to focus on the search for explanatory logics (the context of justification) that have been argued to possess the twin virtues of internal coherence and the likelihood that they can take account of the special conditions of archaeological knowledge

production, rather than exploring an account of the context of discovery (which is a more broadly based inquiry into why some kinds of arguments are deemed plausible and others are not). At the same time theoretical strategies have been somewhat more diverse, but have tended to emphasize either the nature of archaeological data (ontological matters) or a belief that the archaeological record can be understood only in terms of a particular suite of theories (most often social theories that connect knowledge about people right across the humanities and social sciences).

Understanding the reasons why interpretations and explanations are found to be plausible or implausible when no evaluative consensus exists (or the pre-existing ones have broken down) has proved to be a more challenging problem. Work in the sociology of science undertaken during the same period has been of some assistance here, especially in the discussion of the power of disciplines. These discussions have fostered an understanding of two important and different senses of the term 'discipline' – as, on the one hand, a body of specialized knowledge and/or skills and, on the other, a political institution. While there has been no rejection of the former sense as a critical facet of the identity of disciplines (this was also the primary concern of older-style disciplinary histories), in practice research and discussion have focused on the sense of disciplines as institutions marking out areas of human knowledge and socializing their members.

In this latter sense disciplines act as socializing mechanisms where individual and community values and interests collide, and where practitioners acquire their perceptions of what explanations and interpretations are cognitively plausible, what theories materially advance knowledge of observable and unobservable phenomena, what problems are worth pursuing and what methodologies are likely to yield reliable knowledge of the phenomena under review. Analysis of the disciplinary 'culture' of practitioners allows us to chart the ways in which social and cultural 'givens' (normative values) can be incorporated as privileged assumptions analytically prior to induction.

Critical self-reflection, employing perspectives from both the context of discovery and the context of justification, has a significant role to play in understanding plausibility in archaeology, and more studies of the sociology of archaeological knowledge are badly needed. If we recognize that values are present in archaeological statements then we need to understand the ways in which those values 'produce' an archaeological record that is meaningful to us and to the publics that consume our product. Of course there is the danger of adhering to a simplistic relativism here and of arguing that the archaeological record is totally constructed by ourselves. Perceiving the fact that archaeology is social practice and that it cannot (and should not) produce ethically and politically neutral knowledge claims does not imply that 'anything goes'; in fact, it implies the reverse. What it does mean is that practitioners need to understand much more about the traditions of their practice (the context of discovery), to reflect on the implications of this richer and more nuanced history and to explicitly justify their acceptance or rejection of interpretations or explanations.

Critically, relativisms within archaeology (together with the lack of evaluative consensus) reflect the existence of divergent perspectives and interests among the community of practitioners. Histories of archaeology that probe deeply into the context of justification have shown that practitioners rarely manage to reach the standards set by their own brands of methodological rhetoric. I have stressed the importance of plausibility in this context because I am convinced that a core part of an explanation for our failure to build coherent and relevant archaeological theory can be found in explaining why there is a distinct difference between the methodological rhetoric of archaeology (this includes all the argument about a commitment to objectivity and the rigorous

assessment of hypotheses, and the need to build archaeological theory) and what archaeologists *actually do.*

Again, I stress that I am not considering low-range hypotheses, such as those about chronological sequences, or the description of artefacts (including sourcing studies and the like), which can usually be evaluated on empirical grounds. Rather, I am interested in those operating at mid or high levels where broad issues, such as the explanation of change, variation or broader historical processes, come into play. From this perspective, no matter whether one seeks assurance from either the context of justification or the context of discovery (or a combination of both), it can be argued that the values and meanings of different mid- and higher-level approaches to the past are assessed on primarily cultural (hence substantially unexamined) grounds. At these levels the presently overt bases for judgement, such as testability, connectedness to other areas of knowledge, empirical fruitfulness and synoptic power, tend to act more as scientific or hermeneutical conventions appealed to in the course of argument. It is a sobering thought that covert factors such as fundamentally unexamined (but culturally meaningful) presuppositions, the inertia of tradition or worse authority, prejudice, ignorance or fear might also have a significant role to play in establishing plausibility.

In this analysis we can speculate that many of these hypotheses never come under threat during the process of 'doing' archaeology for one of three reasons: first, because we do not know how to derive subsidiary test implications that allow us to use archaeological data to probe core provisions of theories, assumptions or hypotheses (a shortcoming of middle-range theory building); second, because the relationships such hypotheses purport to address have no clear empirical referents (we do not know what would constitute critical test data); third, because practitioners find it difficult to imagine how the human past could be made intelligible without culturally meaningful, but archaeologically un-assessable hypotheses.

Taking this speculation a step further, while we can confidently expect that the interpretative dilemma will strike at more abstract theoretical levels, the survival of hypotheses (and outmoded exemplars of archaeological practice) might well have more to do with the fact that our failures of theory building are hidden by the power of disciplinary tradition than with any robustness on the part of the theoretical instruments involved. In this analysis plausibility links closely with the power of convention and disciplinary tradition, and this is one of the most compelling reasons why plausibility matters and why we need to understand a great deal more about how it operates in our practice. What follows is a very brief discussion of the genesis of one of the most important archaeological concepts – the archaeological culture – as a means of exemplifying how traditions are created and solidified in archaeology.

The rise of culture history in nineteenth-century prehistoric archaeology

From the 1830s in Europe (as well as elsewhere) the goal of prehistoric archaeology came to be the creation of racial, ethnic or national prehistories that were based on what became known as culture history. These culture historical prehistories are considered to be quite different forms of prehistory writing from the general works of prehistoric synthesis represented by the work of Lubbock (1865, 1882) and *Tylor (*1865, 1870) (see Leopold 1980) and the work of anthropologists with a more pronounced leaning to the physical rather than the mental (see, e.g., Stocking 1968, 1987). Nonetheless, the need for chronology flowed into all types of prehistories. Indeed, the three-age system, which advanced no theory to explain cultural change, variation or succession, was a relative chronology in and of itself (Rowley-Conwy 2007).

The rise of the culture history in archaeology in all its varied forms was made possible by the work of Gustav Klemm (1802–67) (see Manias 2012, 2013). The link between mind and environment, which had long been part of Western thought, became itself linked to a more fundamental association between physical form, the mind and the culturally distinct products of mind. Soon after the broad outlines of European prehistory had been synthesized by Lubbock and Gabriel de Mortillet (1821–98), antiquaries began to consider more deeply the causes of cultural change and variation. In *The Primitive Inhabitants of Scandinavia* (English translation 1868) Sven Nilsson (1787–1883) had explained change in Sweden as being the result of movements of racially distinct peoples, engaged in racial conflicts that were so much a part of the way in which nineteenth-century Europeans construed political conflict in their own day. J. J. A. Worsaae (1821–85), although never really being explicit about the role of migration and conquest, did allow for it within his scheme of environmental changes. Importantly, Lubbock and Tylor had considered the causes of similarity and variation in material culture with greater care than the causes of culture change itself.

Klemm's clear association of mind with race focused attention on material culture as being a marker of racial history. In this view technology assumed a significance equal to that of language as an ethnic marker, but, more importantly, the newly acquired significance of material culture was a function of linguistic theory itself. Although Max Muller (1823–1900), among others, rejected the equation between language and ethnic group, the archaeologists of the nineteenth century were not nearly so fussy. For them the problem remained: were the changes in material culture, and its multitude of variations, the product of distinct groups of people with distinct histories or was the prehistory of Europe really the result of many racial and ethnic prehistories? But, if this last was the case, were all changes to be explained by migrations and conquests or was there to be a mixture of independent/parallel development and diffusion? Pitt-Rivers (1827–1900) had outlined two plausible explanations for cross-cultural technological similarities: diffusion and independent invention. During the late nineteenth century the relative merits of the two explanations were vigorously canvassed by the supporters of Oscar Montelius (1843–1921) and Salomon Reinach (1858–1932) (see Daniel 1971, 1975). These debates continued well into the twentieth century, perhaps reaching their apogee in the hyper-diffusionist work of Grafton Elliot Smith (1871–1937) and W. J. Perry (1887–1949) and the propositions of Gustaf Kossinna (1858–1931). It is important to remember that significant elements of what began in the second half of the nineteenth century remain at the conceptual core of twenty-first-century archaeology.

I now consider two late nineteenth-century national prehistories to demonstrate archaeological problems that provided fertile ground for the developing popularity of the culture historical approach. The two works are very different in their treatments. Montelius held fast to a notion of civilization and culture that is the common property of all people. William Boyd Dawkins (1837–1929) advanced a more particularist characterization of individual cultures. Montelius argued for a common European prehistory, a common European experience of the past. Boyd Dawkins presented a view that the variability of European prehistoric material culture was the outward expression of real historical processes.

Montelius's *The Civilization of Sweden in Heathen Times* (English translation 1888) is a self-conscious contribution to the history of civilization, of culture in the abstract as that which distinguishes human beings from the lower forms of life. Woods, Montelius's translator, had this to say:

It is true that it deals directly with the progress of one particular people; but all archaeology tends to show that there has been a remarkably similar process of development, not only among European peoples, but among all races of the world. It follows that a clear and succinct account of the progress of any one people helps to give us a clear notion of the successive stages of civilization through which all races have passed. (Montelius 1888, v–vi)

For Montelius, 'national' prehistories should, first and foremost, contribute to the elucidation of the general progress of civilization. Yet that elucidation required much greater detail, as what was known about European prehistory in particular expanded. There are further echoes of older concerns. For example, Montelius still felt it necessary to emphasize the historical significance of archaeological remains, and the scientific reliability of the knowledge about the prehistoric past produced by archaeology. In so doing he distinguished his perception of admissible archaeological data sharply from that of Nilsson:

It is true that we meet with no line of kings, no heroic names dating from these earliest times. But is not the knowledge of the people's life, and of the progress of their culture, of more worth than the names of the saga heroes? And ought we not give more credence to the contemporary, irrefutable witness to which alone archaeology now listens, than to the poetical stories which for centuries were preserved only in the memory of the skalds? (Montelius 1888, 3)

Two further cautions in the introduction indicate Montelius's unwillingness to present a truly historical reconstruction of Swedish prehistory. The first deals with the causes of culture change:

How far the beginning of each period coincides with the appearance of a new race which subdued the earlier settlers in the country, is a further question which we must for the present distinguish from that which concerns only the order in which the several heathen periods followed each other. (Montelius 1888, 4 [sic])

Here Montelius at least implied that Nilsson's older argument, that the gross changes in culture experienced in the north were most likely the result of invasion and conquest, has at least some validity. The other caution concerns the fragmentary nature of the archaeological record as a natural limitation on induction:

Before we now make an attempt to set before our readers a picture of life in Sweden during heathen times, we must observe that if that picture shall prove imperfect and blurred, it is partly perhaps owing to the insufficiency of our sources of information about a period so wanting in historical materials. … Only a small part of what once existed was buried in the ground; only a part of what was buried has escaped the destroying hand of time; of this part all has not yet come to light again; and we know only too well how little of what has come to light has been of any service for our science. (Montelius 1888, 5)

Cautions aside, Montelius introduces the Stone Age with an absolute date of '*[t]o about* B.C. 1500'. Then follows a traditional listing of the artefacts and monuments, along with appropriate comparisons with contemporary ethnographies and to other finds from European prehistoric sites. One major absence is the analysis of skeletal remains. Instead of craniometrical measurements and inferences of race or ethnicity, we find that grave goods are described, the various tomb forms are classified and even causes of death discussed. This makes Montelius's assertion of a near-continuous racial history for Sweden quite different from the racial conflict model proposed by Nilsson. However, it should be noted that Montelius also rules 'evidence' from philology as inadmissible, thereby restricting the possibilities of culture-historical interpretation even further. In Montelius's estimation, significant data for this inquiry are simply lacking, and the dictates of science require that we should pass from it with no signs of regret:

At the end of the Stone Age the inhabitants of the North were not only still entirely ignorant of metals even gold, but also of the art of writing. And consequently we have no remains of the language of this age to show us what the people was which then called Sweden its fatherland. An attempt has been made to answer this question by means of the skulls found in the graves of the Stone Age. Some are very like those of the Laps, but most bear a close resemblance to the Swedish skulls of the present day; which seems to show that a mixture of two different races had at this very early time already taken place. (1888, 37)

If the Teutonic ancestors of the Swedes were already living in Scania during the Stone Age, how was the shift to the Bronze Age to be explained? Montelius reviewed the explanations, ranging from Nilsson's Phoenicians to Ludwig Lindenschmidt's (1850–1922) Etruscans, but rejected them in favour of trade (1888, 43). The similarity between late Stone Age and Early Bronze Age graves argued against any 'great immigration of a new race'. What Montelius called the 'Bronze Culture' had spread out of Asia in a north and northwesterly direction across Europe, diffusing bronze technology and artefacts during period '[f]rom about 1500–500 B.C.' (42). It transpires that diffusion is to be appealed to as the explanation for the shift from the Bronze to the Iron Age as well, but by the Iron Age the historical value of legends, sagas and customs has been increased.

If Montelius's prehistory of Sweden stressed the value of the direct historical approach to the Iron Age, it also stressed the argument that migrations and invasions should be argued for by the archaeologist only if large-scale changes in the background material culture could be detected. In this sense Montelius was appealing to culture-historical theory in its linking between racial and ethnic groups and distinct cultural inventories. Major changes of this kind not having occurred at any time in Swedish prehistory, the most parsimonious explanation was for an early settlement of Sweden by the ancestors of the Teutons and for change in the material culture to be explained via diffusion and local adaptations of diffused technologies and styles.

Boyd Dawkins opted for a different approach. His *Early Man in Britain and his Place in the Tertiary Period* (1880) has a far wider agenda than Montelius's Swedish prehistory; it offers the framework of a complete prehistory of Britain that reviewed the general European evidence for human and the later periods of prehistory. The fourteen chapters of this massive work of synthesis (over 500 pages long) take the reader from a defence of methodology to Britain in the historic period. Although Boyd Dawkins also wished to contribute to a more general history of civilization, this was to be primarily a work of prehistoric synthesis, stressing both sequence and variation.

Boyd Dawkins was not reluctant to begin his project back in the time of what he called the Cave-Men and the River-Drift men. After presenting detailed information on the relative chronological positions of the two classes of evidence from the river drift and from the caves, he queried whether these different artefacts represented two distinct groups of people or whether the changes were evidence of a progress in human culture. 'How are they related to each other? Is the culture of the latter the outcome of the development of that of the former? Or is it to be viewed as having been introduced into Europe by a totally different race?' (1880, 229).

Boyd Dawkins considered the problem from the perspective of the material culture, and from palaeontology, geology, ethnography and geology. He was in no doubt that, after considering the range of evidence available, 'they may be referred either to two different races, or to two sections of the same race which found their way into Europe at widely different times' (1880, 233). Further, while the ethnology of the older River-Drift men was essentially unknowable, being lost to the mists of time (significantly *not* the view of Lubbock or Tylor), the ethnology of the Cave-Men was a comparatively easy matter. On the basis of similarity in material culture, the Eskimo should be considered to be the most likely direct descendants (233–42). Furthermore, Boyd Dawkins saw

absolutely no connection between the people of the Palaeolithic and those of the Neolithic, either in cranial shape or in material culture (242–3).

Boyd Dawkins was to use this argument of complete population replacement on more than one occasion, and he justified it on the grounds of racial hatred known to exist between contemporary primitive peoples, an argument used by Nilsson as well (1868, 253). For example, Boyd Dawkins posed a similar question concerning the fate of the people of the Neolithic. In a chapter headed 'The Neolithic Inhabitants of Britain of Iberian Race', which followed a section where he derived the entirety of Neolithic civilization from southwest Asia, Boyd Dawkins broached the delicate topic of whether any survivors of original Neolithic populations can be found in Europe.

Characteristically, Boyd Dawkins considered the problem to be solvable, as long as evidence from osteology, philology, history, ethnology and geography was to be used. He considered the Neolithic to be a period of race wars between Celt and Iberian, each with a separate homeland within Europe. Here the attribution of classical tribal names for the races of the European Neolithic emphasized the belief that the remnants of such tribes still inhabited parts of Europe (such as the Pyrenees). It is worth quoting Boyd Dawkins's general conclusions for the Neolithic at length:

> The Iberic peoples were probably driven from the regions east of the Rhine by the Celts, and they in their turn by the Belgae, just as within the Historic period the Belgae were pushed farther to the west by the Germans, who in their turn were compelled to leave their ancient homes to be occupied by Sclaves. … The progress of civilization in Europe has been continuous from the Neolithic Age down to the present time, and in that remote age the history of the nations of the west finds its proper starting-point. (1880, 341)

Boyd Dawkins's image of race war as the basis for cultural change and succession was specifically rejected by Montelius, who explained change through the presence of trade and other less violent means. Nonetheless, it was a picture of European prehistory that was to introduce the cast of tribes and cultures that formed the basis of the cultural taxonomy of European prehistory.

From this witches' brew of race conflict, differential rates of progress and cultural diffusion, prehistorians of the period between the start of the 1880s and the end of the Second World War fashioned the stuff of the archaeological culture, a development that was to become so powerful in the archaeology of the twentieth century. It is worth noting, by way of a conclusion, that the explanation of European prehistory in terms of the interactive products of discrete archaeologically definable groupings of people owed much more to ethnological theory than to any inherent cultural properties of the archaeological database.

Indeed, it was precisely during the period following the application of the three-age system outside Scandinavia that prehistoric archaeology attained the status of a discipline and all the trappings of scientific societies, international conferences and venues for publication that went with it. Moreover, at least part of the reason for the resurgence of culture-historical archaeology toward the close of the nineteenth century was provided by the developing significance of variability within the archaeological record itself. Nonetheless, there is a real sense of archaeology's disciplinary status being achieved as a sub-department of anthropology or ethnology, a sense reinforced by the fact that during this phase of the discipline's history there were still few professional archaeologists who were not practising ethnologists or anthropologists.

By the end of the nineteenth century, prehistoric archaeology possessed puzzles and problems, as well as methodologies for their solution, that were ample testimony to its right to stand as a

coherent discipline. This does not mean there was anything like general agreement about the causes of social and cultural change and variation in the prehistoric past – far from it. Yet, these puzzles, particularly the link between archaeology and the geographical readings of culture-area theory, were to exercise archaeologists for much of the next century and they continue to resonate today.

They were also seen by practitioners to be taking place within the cognitive boundaries of history (the use of source criticism in ethnographic analogy, the perceived need to specify causal relations between historically linked events), physical anthropology (the link between the mental and the physical) and sociocultural anthropology, which had by the beginning of the twentieth century become much more concerned with describing and understanding specific cultural states.

Above all, during the last half of the nineteenth century we see the maturing links between the practice of archaeology and the communities that were most interested in its discoveries. The history of archaeology before the nineteenth century has many examples of antiquarians and archaeologists using their studies to support contemporary political agendas (or having those studies used by others in that way). Leland, Camden and the early members of the Society of Antiquaries of London, for example, clearly understood this. In the nineteenth century the forces of revolution (in 1789 as well as 1848), independence movements (such as in Greece), the creation of new nations (such as Belgium, Germany and Italy) and the creation of empires provided significant challenges to antiquarians and archaeologists. Some practitioners (such as Worsaae) were ardent nationalists who sought to enhance popular understanding and acceptance of the nation through a demonstration (in the case of Denmark) of its long history. Others were more exercised by a search for the 'essence' of particular nations – a kind of bedrock cultural foundation that made the nation eternal rather than the product of contemporary politics. Archaeology, along with ethnology, played a highly significant role in all of these 'nationalist' manifestations in the nineteenth century. More importantly, it was to continue doing this in the twentieth century in ways that were to pose serious moral and ethical challenges to practitioners.

Concluding remarks

Re-establishing evaluative consensus in archaeology is not desirable because the presence of relativisms within the conceptual and epistemological realms of the discipline cannot be eradicated by applying determinate rules of the scientific method or some notion of universal rationality. However, we can be clear about our need to evaluate archaeological knowledge claims in a consistent and transparent fashion. The history of archaeology allows us to investigate the terms under which rival archaeological knowledge claims are produced and justified. By doing this it also aids our efforts to facilitate communication between them, as well as assisting in theory building, so that rival positions could be made clear enough to allow informed judgements to be made by practitioners about their strengths and weaknesses.

I have argued that strategies derived from the context of justification and the context of discovery all have roles to play here, and all have to be used with caution. Not one of these strategies is universally upheld as being the most powerful, rational or objective. Significantly each strategy has its own context and its own costs and benefits, all of which need to be carefully considered by archaeologists and their audiences. What really is important about this exercise is not constructing an abstract calculus of rules which we will abandon (or make elastic) when the situation demands, but enhancing our power to reflect on what we want from archaeology and

what we think its role in our search for understanding might be, to make judgements and to accept the responsibility for making those judgements.

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Notes on contributor

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