

6.5 Landscape studies: The future of the field

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ABSTRACT

In this paper, I will go back to basics in landscape archaeology. How do we use the evidence of the landscape to find out about the past? I reflect on the distinctive nature of landscape studies as distinctive not as 'art' or as 'science', but as a field-based discipline, in which the [claim of an] apprehension and encounter with a reality 'out there' in the field is placed at centre stage.

Historically, field-based disciplines have a stress on what is claimed as 'direct experience', particularly in terms of their vernacular imagination – in other words, the colloquial language and everyday values deployed in field practice. Field-based disciplines rest, in part, on the urge to go out and see for oneself, rather than rely on others' reports.

'Direct experience', however, is a problematic concept. Most crucially, the claims of any discipline to be 'scientific' must rest not on the amount of direct experience gathered, but rather in the way that experience is brought to bear on concepts and theories about it, and vice versa. If landscape archaeology is to be rigorous and scientific, it must abandon rhetorical appeals to an untheorised category of direct experience and reflect more seriously on the relation of evidence to inference.

INTRODUCTION

This paper has as its aim the setting out of issues of knowledge construction and knowledge evaluation in landscape archaeology. It forsakes some of the more detailed and advanced discussions of the field (for which see Johnson 2007; David & Thomas 2008; Tilley 1994, 2004; Bender 1998; Thomas 1999; Ingold 2000; Bender et al. 2008; Smith 2003). Instead, it goes back to basics: asks how landscape archaeologists

find out about the past, and in particular, how they make a judgment as to whether their interpretations are 'good' or 'bad', are or are not supported by evidence. My desire is to strengthen these methods of evaluation and to make landscape archaeology a more rigorous empirical science.

I want to be as clear and precise as possible in what follows. Landscape archaeology, for reasons we shall explore, is an area of research that is full of woolly thinking. The field has been beset by a lack of conceptual clarity. In particular, thinking on landscape archaeology, particularly in Europe, is riddled with false, rhetorically loaded choices – between supposedly 'theoretical' and 'practical' approaches, between supposedly subjective and objective positions, between supposedly 'scientific' and 'humanistic' positions and interests, between those who supposedly stick to the evidence and those who supposedly go beyond it, between those who are supposedly positivist/empiricist and those who are supposedly postmodernist. These false choices most frequently surface not in analytical discussions but in chance, along-the-way comments by practising landscape scholars (for example, Liddiard & Williamson 2008) and are rarely explicitly set out (credit must be given to Fleming 2006 for putting into print much of this implicit discourse). The alternatives offered up can be given different moral evaluations (for Fleming: good/bad; for Tilley 2004, bad/good), but they are false choices nevertheless.

These false choices are seductive because, having been set up, an appeal is then made to have a 'debate' between them. Such a 'debate' has the surface appearance of openness, equality and respect between different positions. But a debate thus set out is bound to fail, because it rests on simplistic binary oppositions that are themselves symptomatic of a failure to understand the underlying issues. Alternatively, and more seductively, these two supposed traditions are framed in such a way as to present them as complementary or independent. Such a framing is just as damaging, because it implies that scholars in one tradition can get on with their work without having to worry about any critical evaluation that might be made by the other. Such a framing, then, is implicitly and crudely relativist, in that it ducks the issue of relative evaluation of two (falsely framed) traditions.

It is time to set these false choices, rhetorical appeals and implicit relativism aside and instead go back to basics, to think clearly and dispassionately about the nature of landscape archaeology and the way it constructs knowledge. This is what this paper tries to do.

SCIENCE

Archaeology is the study of past human societies from the material remains that are left in the present. Landscape archaeology, then, is the study of past human societies through the traces left in present landscapes.

Archaeology attempts to undertake this study of the past in a scientific and rigorous manner. Archaeology rightly sees itself as an empirical science which routinely and habitually seeks evidence to confirm or to disconfirm the statements about the past that it makes. However, it is equally true that the terms 'science', 'rigour' and 'evidence' are not simple or common-sense terms. In reality, science, rigour and evidence are very slippery, difficult and complex ideas. If they were easy, obvious and simple ideas, then the discipline of the philosophy of science would have nothing to talk about and the project of developing archaeological theory would have been successfully concluded a long time ago. Instead, the definition of science and the question of what counts as evidence continue to be hotly contested topics, however much we would like them to be simple (Wylie 2002).

I take a pragmatist view of the definition and practice of science. Pragmatism is a school of philosophy developed in 19th-century North America, by (among others) Charles Sanders Peirce, William James, Dewey, and latterly Richard Rorty (Stuhr 2005 is an introduction; Rorty 1999; Baert 2005 is a good discussion of the application of pragmatist philosophy to the human sciences). Pragmatism asks: what are the practical consequences of a particular belief? In what ways does a belief lead us to think and act differently? Pragmatism distrusts the niceties of metaphysical assertions and avoids appeals to abstract Platonic Truth. Pragmatists instead prefer to think about practical action to change the world. Pragmatism is, I believe, particularly useful for landscape archaeology as it directs attention not simply to the abstract intellectual properties of an idea, but to critical scrutiny of the specific ways theories lead us to do archaeology differently (Mrozowski & Preucel 2010).

Science is, among other things, a way of learning about the world. Good science should enable and require us to learn more, and unexpected, things about the world. Conversely, a symptom of bad science is the absence of learning and the absence of unexpected results. If landscape archaeology has been a field of woolly thinking, I think it is also true that in many areas it has become a field where learning about the world is less and less likely to happen.

In the sense of science I am proposing here, the enemy of science is empiricism. Empiricism is a word that is apt to confuse, since it has several meanings: here I mean by empiricism the belief that the data just speak for themselves, or that the way to understand what data 'mean' or how they should be interpreted is self-evident. Empiricism in this sense is overtly disavowed by most serious scholars (though see Johnson 2011 for a discussion of the way it nevertheless lurks within much archaeological interpretation). But the whole thrust of the practice of landscape archaeology is to privilege empiricism, through its stress on the direct gaze, on bodily movement ('muddy boots'), and on trust within a narrow community (see below). The problem is compounded by a persistent misreading: when the epistemological difficulties with such an empiricist position are pointed out, as I did in *Ideas of Landscape* (Johnson 2007), this is treated as a rejection of the process of empirical method as a whole. I must insist: A truly critical and fruitful empirical method starts with a questioning of its means of inference, not an assumption that those means of inference lie beyond 'theory'.

Recent pragmatist philosophers have stressed that good science, in the sense of learning unexpected things about the world, is most likely to happen when a diversity of stakeholders are involved. Conversely, where the practice of science is socially restricted, learning is less likely to happen. A socially restricted practice of science is therefore impoverished and lessened in value. This point has been made in relation to the development of a proliferation of scientific standpoints, for example feminist, postcolonial and Indigenous thought (Baert 2005, 161; see also the discussion in *American Antiquity* 75 (2), particularly Silliman 2010 and Wilcox 2010).

Let us then ask the pragmatist question: How, in practice, do scientists find out about the world? It is worth turning to the early history of the 'scientific revolution' to think about this question. 17th-century thinkers rested claims of the distinctive nature of scientific enquiry on its direct observation of the natural world. There was a cultural and political element to this claim; British empiricists in particular couched the claim rhetorically in a rejection of medieval Catholic theology. Early philosophy of science was in this sense heavily influenced by Protestantism. The medieval Catholic Church saw the world as a complex system of signs and allegories, to be explained as part of the divine mystery. For natural scientists, many of whom were also committed Protestants, the world was to be seen as it really was, through direct experience, in a fundamentalist manner (Shapin 1994, 1996).

However, philosophers of science were immediately aware of a problem of trust. Science depended not simply on direct experience, but on the reliance of the experience of others, the acceptance of their work as a secure base upon or alongside which to work. In the 17th century, 'trust' was defined socially and also legally. Thus, the 17th century notion of 'trust' excluded women and lower social orders; indeed, anyone who needed to work for a living and therefore could not be counted as being disinterested was disqualified. At the same time, many of these scholars also being trained as lawyers, issues of trust and authority were settled using metaphors and practices from the law courts to settle questions of 'truth' and what counted as 'fact' (Shapiro 1999).

In time, early modern scientists and natural philosophers resolved this question of trust by developing the idea of the repeatable experiment. Arguably, the repeatable experiment worked well and continues to work for the laboratory-based natural sciences. However, for emergent disciplines such as geography, geology and archaeology, the main or primary location of observation or direct experience was, in these early phases, not in the laboratory but in the field.

THE FIELD

In landscape archaeology, a central arena of everyday practice is 'the field'. The encounter with primary data in the field remains central in the hearts and minds of archaeologists. 'Direct field experience' is routinely cited as a primary determinant of evidence. A routine device in the praise of archaeologists is to praise the length and arduous nature of their time in the field. Conversely, abuse is directed at 'clean-booted historians' (Crawford 1953, 198).

Fieldwork is, without doubt, much more than the gathering of data. It is a bodily, aesthetic and sensual experience as well as a dry gathering-up of information (Ingold 2008). Archaeologists love being 'in the field' and they do it as often as possible. A routine complaint about university administration is that it keeps archaeologists away from the field. Student assessments of courses routinely call for 'more field trips'.

The 'field experience' is different from what happens in the laboratory in several crucial respects, respects which speak to the cultural practice and sensibility of field disciplines as much as they do to formal or abstract properties of knowledge. First, the field produces a different set of bodily practices. Rather than being cooped up like Dr Frankenstein in an isolated, stuffy laboratory in a dark basement, the field scientist practices his or her craft in the open, through strenuous action, like Indiana Jones. Second, the field produces a different sensibility, and set of terms of praise or denigration: muddy boots, toughness in terms of immunity to climatic vagaries of heat or cold. Fieldwork has been argued by feminists to be gendered masculine and valued accordingly (Gero 1985; Joyce 1994). Third, the field introduces an aesthetic sensibility: the object of study is often not merely complex and fascinating, but beautiful as well. And with this beauty comes a string of other cultural assumptions and practices relating to the conception of Nature, again, an exceptionally complex concept, 'perhaps the most complex word in the [English] language' (Williams 1976, 184), from Romanticism to modern ecological sensibilities (Bate 1991).

Above all, the field experience elevates the primacy of the gaze. The direct gaze is a central part of field practice – 'I saw it myself' becomes the final arbiter. It is easy, too easy, to slip from an entirely proper insistence on the importance of field practice to the notion that the gaze, or direct field experience, is

somehow therefore beyond theory, or is somehow theory-free, or can be appealed to as an alternative to 'theory'.

It is a symptom of its very deep discursive nature that the notion of direct experience as prior to theory cuts across the usual theoretical divisions:

There can be no substitute for the human experience of place – of being there – and it is *only after this that the various technologies of representation come into play*... our attempts at thick descriptions of place contrast with the standard mode of thin technicist archaeological description which effectively dehumanises the past and makes it remote and sterile because such technical descriptions are based on abstracted Cartesian conceptions of space and time (...) (Tilley 2004, 221; my italics)

Such a critical slippage, between field experience as important and field experience as beyond or an alternative to theory, is assisted by the weight of the cultural freight that comes with appeals to the primacy of the gaze. Such appeals are not simply or only associated with the Enlightenment (Foucault 1976, 89); they go much deeper still into the very roots of European culture. This primacy is deeply embedded in the history of British empiricism and its legal context and background: in the 17th century, it is precisely this direct gaze that had a legal origin, in the testimony of witnesses in the courts, as noted above. Again, the all-encompassing gaze was a central element in practices of 18th-century European Romanticism (Urry 2002).

There is a large and growing literature that critically analyses archaeological practice, particularly excavation methods (Lucas 2001; Hodder 1999; Edgeworth 2006). It is interesting that relatively little of this literature engages with the field practice of landscape archaeology, as opposed to excavation. The debate between Gillings, Pollard and Bowden (Gillings & Pollard 1998; Bowden 2000) over the nature of representations of the Avebury landscape is interesting in Bowden's insistence, in reply to Pollard and Gillings' critique, that maps and plans are in fact critical interpretive practices – in other words, Bowden's position is that a critical approach is implicit in traditional techniques. He thus arguably mounts a successful critical defence of traditional field practice, but invites the obvious question: why is it then implicit and rarely spoken about rather than explicit and critically discussed?

Field experience is essential to the craft of landscape archaeology. This does not mean, however, that field experience is therefore immune from critical enquiry. Indeed, the status of landscape archaeology as an empirical science makes such critical enquiry all the more essential. To repeat, scientific rigour demands that the links made between evidence and inference are examined closely and critically, not assumed or placed beyond the domain of theory. To place field experience in some category beyond theoretical reflection, as if it were somehow transcendent or ineffable, is to insulate it from critical scrutiny. Rhetorical appeals to the primacy of raw field experience are, therefore, unscientific, and approaches which divide views of landscape into 'practical' and 'theoretical' are misleading.

I want to dwell on this point, because such appeals have a high visceral or emotive charge because of the cultural freight associated with field practice discussed above. Most damagingly, a false choice is invited between those who supposedly place fieldwork at the centre of their method and those who supposedly do not. It is precisely this emotive emphasis on the field that leads to the central element of woolly thinking I want to discuss, namely the empiricist fallacy that data gathered will tell us about the past without the benefit of intervening theory.

In and of itself, 'the field' tells an archaeologist nothing about the past.

If you stand in the middle of a muddy field or on top of a hill surrounded by ancient earthworks and listen very carefully, you will hear nothing about the past. If you walk across many more muddy fields, you will still hear nothing. Further, if you take lots of photographs, you will still hear nothing. If you draw what is around you, however accurately and meticulously you do the drawing, and however carefully you measure in your observations, you will still hear nothing. When Barbara Bender writes that "the evidence" does not of itself deliver an understanding' (Bender 1998, 7), this is not a dangerously postmodern or relativist sentiment to be deplored (as others have alleged, cf. Fleming 2006); it is a statement of the blindingly obvious.

Much modern landscape archaeology is preoccupied with ever more complex techniques for mapping field observations of different kinds, many of which will be found explicated in the pages of this volume. These techniques are exciting, and offer tremendous potential. However, they do not, in themselves, tell us more about the past. If you rectify air photographs in new ways, you still hear nothing about the past. If you use LiDAR techniques to produce ever more complex mapping of traces in the present, you will still hear nothing.

Science is not about accumulating lots of empirical observations. I can leave my office and count the blades of grass in the lawn; I can do this very carefully, and my back and eyes will ache at the end of the exercise. However, without a clearly defined and formulated set of research questions linking propositions to data, I will not be doing science, either in a pragmatist or in any other sense.

The accumulators of evidence par excellence are the ley-line hunters. Ley-line hunters get out in the field. They spend their time walking across the landscape, and directly observing it with their own eyes. They make systematic recordings of their observations which are careful and accurate. However, they are, of course, completely misguided – in part, because of their uncritical reliance on raw experience. Anyone can 'see' the evidence for a ley line: walk along one marked on a map, and you will find its existence confirmed over and over again as the route yields further evidence of its ancient nature as the walker discovers banks, ditches and other ancient features (Willamson & Bellamy 1983).

What makes landscape archaeology into a scientific and rigorous discipline is not then, primarily, the raw experience of fieldwork. Rather, it is the processes of inference and deduction that are used to translate between data gathered in the field to general statements about the past, and back again. If these processes of back-and-forth translation are faulty, or have not been clearly thought through, then no amount of field observation or muddiness of boots will turn archaeology into a science, any more, with apologies to David Clarke, than a wooden leg turns a man into a tree. This very basic point was absolutely central to early New Archaeology. It remains fundamental to any kind of critical and scientific archaeological practice, and deserves to be repeated over and over again, in part due to its counter-intuitive nature (Clarke 1976; Binford 1983).

The problem with much of landscape archaeology is that 'raw field observation' is placed as primary, and placed beyond theoretical reflection. Such a placement resonates with the great love and passion for fieldwork that landscape archaeologists have, for the reasons discussed above. However, by this placement, the question 'how do we use the results gathered to tell us about the past?' is bypassed.

Instead of addressing this problem of inference in a clear and direct way, archaeologists – of all theoretical stripes and affiliations, empiricist and postmodernist, Marxist and culture-historical, 'traditional' or avant-garde' – simply end up telling stories about the past. Monographs on landscape archaeology fre-

quently consist of lovingly-gathered data, beautiful air photographs, complex maps showing clusters of settlements. Then, in the final chapter, we get a narrative about the past. The reader is led to believe that this narrative is empirically informed, because the preceding bulk of the book has been taken up with so much data. But if one stands back and asks the question 'how do these data inform our judgements – how does it allow us to judge this statement about these convincing past processes and that judgement wanting?' things are often not so clear.

HUMAN BEINGS

If landscape archaeology needs to address issues of inference, what kind of past is it attempting to make inferences about? By definition, such a past has to be a human one. Landscape archaeology is exactly that: it is not geology, nor is it physical geography. Archaeology is the study of past human life. Landscape archaeologists, then, make statements about human subjectivity (their view of their world), whether they like it or not.

In human sciences, we have to think about human beings and their view of their world. Consider for a moment the alternatives. First, the proposition that we do not have to bother: mental attitudes are irrelevant to understanding long-term human impact on landscapes. Second, past peoples were just like us: human subjectivity in the past was just like it is in modern, bourgeois society in the present. The first proposition is just about tenable, and is held by a few hard-line cultural ecologists and behaviourists; it remains nevertheless very much a minority pursuit. The second proposition is not seriously tenable for more than a few seconds.

If this proposition is accepted, then a necessary part of rigorous and responsible science is a questioning and thinking-through of human subjectivity in the past, and in particular a rejection of the assumption that human subjectivity can be dealt with as a matter of common sense. This is precisely the task that phenomenology sets itself. If there is a bogeyman that has been uncritically deployed to denigrate others and rhetorically reinforce empiricist approaches, it is phenomenology: thus Liddiard & Williamson (2008, 525), citing Fleming, write: 'phenomenologists (...) believe (...) that the rigorous testing of the ideas that are derived from it is either impossible or unnecessary (...) having to a significant extent freed themselves from traditional concerns about the verification of ideas, many archaeologists of this persuasion have effectively given themselves permission to say more or less whatever they liked about the past (Fleming 2006, 269)'. Ironically, phenomenology as a philosophical project sets out quite explicitly to *problematise* the uncritical imposition of modern ideas of human subjectivity on to the past. It is true that in practice, some work under the phenomenological banner has tended to reproduce rather than question modern subjectivities. However, this is an issue with particular scholarly applications of the theory, not the theory itself. Fleming's criticism, and Liddiard & Williamson's uncritical repetition of that criticism, betrays an inattentive reading of the theoretical literature, and betrays an assumption that 'rigorous testing' is a clear and unproblematic procedure that needs no further definition or explication beyond being noted as a 'traditional concern' – in short, that it is somehow beyond theory.

I have been critical of some aspects of phenomenology in the past (Johnson 2007), but here I want to acknowledge that its central aim is unavoidable. Or to put it another way, *we are all phenomenologists*, insofar as we acknowledge that the exploration of different ways of living in and experiencing the world

is a necessary and legitimate part of what we do as archaeologists. The only question at stake is: how, by what methods, do we do so?

The dismissive comments made about phenomenology by some critics are a classic example of how a questionable rhetoric of appeal to direct experience has taken over from reasoned and informed argument. I explored above how much of landscape archaeology, particularly of historic periods, ends up telling stories about the past, however lovingly large bodies of empirical material are brought together and presented, because the critical links between evidence and inference are not examined. It follows, then, that no *a priori* judgement can be made as to the extent to which such traditional studies are more or less evidentially based than any other form of landscape archaeology, for example explicitly phenomenological studies. The epistemological criteria by which traditional, culture-historical arguments are designated 'cautious' whereas theoretically informed arguments are 'wild' or 'speculative' are, then, entirely unclear; it is difficult to believe that they rest on more than an untheorised gut feeling, hardly the basis for rigorous scientific evaluation.

The response to critics of explicitly phenomenological approaches should be: if you are unhappy with the philosophical basis of phenomenology, tell us your alternative. Do you either a) reject any possibility of exploring past human experience or b) simply impose your own assumptions onto the past? If you do neither, will you set out the philosophical basis of your approach and open it up to critique? It is striking that the most recent and authoritative discussion of the issues surrounding phenomenology (Barrett & Ko 2009) does precisely this. Barrett & Ko forsake easy platitudes and engage in a much deeper – and more fruitful – exploration of the underlying theoretical issues.

THE WAY FORWARD: A PRAGMATIST VIEW

I have the following suggestions, then, for the future of landscape archaeology.

First, abandon the highly rhetorical use of theoretical bogeymen and the invitation of false choices. Every time the reader encounters such rhetoric, the question to ask is: what is your alternative? Are you seriously asserting that fieldwork practice is somehow beyond theory? If you are not making such a naïve move, then what is your alternative, and how do you justify that alternative?

Second, maintain and extend the critical examination of field practices. In recent years, there have been a series of developments that render the direct gaze problematic and expose its underlying theoretical geology. The Visualisation in Archaeology project (www.viarch.org.uk) has gathered together and presented much of this work, which includes reflexive approaches to computer 'reconstruction' (Earl & Wheatley 2002), critical reflection on air photography (Hauser 2007; Barber 2010); and critical histories of landscape archaeology (Stout 2008; Johnson 2007).

Third, open the practice of landscape archaeology up to different groups and stakeholders. We have seen above how good science is aided by the opening up of method to a diversity of groups. Landscape archaeology, then, is intimately connected to issues of access to the countryside, of multiculturalism, and of diversity and inclusion. Such issues have been raised, for example, by Barbara Bender's work at Stonehenge (Bender 1998), and developed in the last decade by heritage agencies like English Heritage (Fairclough, this volume).

Such developments are not in opposition to science: rather, they expand and deepen scientific enquiry. They render it more critical and more rigorous by exploding the narrow circle of trust. Questions over science, and over the social and political context of science, are not an either/or option. The work of a generation of feminist standpoint theorists (for example Gillian Rose in geography: Rose 1992) has shown how good science is also inclusive and diverse. Science needs a diversity of hypotheses to drive its learning forward and a diversity of stakeholders to inform its thinking.

Approaches which emphasise a diverse and inclusive view of science, and question a naïve empiricism, are judged by their critics against an implicit yardstick of a tried and tested method. This tried and tested method cannot be set out or defined, because it is non-existent. A neutral, value-free method of doing field science is an impossibility, as the VU University of Amsterdam acknowledges as a matter of official policy: 'we take the view that academic work cannot be divorced from the concerns of society, in terms of standards, values, philosophy and religion (<http://www.vu.nl/en/about-vu-amsterdam/mission-and-profile/index.asp>, accessed 1 January 2010)'.

CONCLUSION: IN PLACE OF A 'DEBATE'

If a value-free science is an impossibility, it follows that the way forward is not in 'debate': there is no debate to be had. Rather than a 'debate', what we find around us is a deepening crisis over the place of academic knowledge in a Europe in which landscape and nationalism have been and are linked in disturbing ways. This linkage between the study of landscape and its wider social, political and cultural context provides the reason, I suggest, why heritage organisations are at the forefront of new approaches, whereas Universities and the Academy have become increasingly inward-looking in their theoretical discourse; see for example recent work by English Heritage (Fairclough 2001; Fairclough et al. 2008).

I wrote above that there is no debate to be had. This may be true in a strictly intellectual sense. Of course, landscape archaeologists, as a group, do have a choice ahead. If we want, we can cling to a myth of academic detachment that has long since been comprehensively discredited, and we can watch our work not simply become more and more narcissistic and irrelevant, but also increasingly reactionary, uncritical and incurious, too easily harnessed by others to unsavoury nationalist agendas, but most crucially, become less scientific, to generate fewer and fewer new insights about the past even as we work harder and harder to accumulate more and more data. And we can defend that choice with a shrill rhetoric of untheorised objectivity and increasingly embattled and desperate denunciation of postmodernists, feminists and other bogeymen outside the narrow circle of 'people like us'. But that is not really a choice.

Landscape archaeology makes a claim to be a responsible and rigorous field science. If it is to substantiate this claim, it needs to think carefully and self-critically about how it moves from evidence to inference. The amassing of evidence in itself, however carefully and arduously done, is not the single defining or primary feature of science; theoretical reflection on the means of inference is at least equally important. It is this point which has been lost in some recent commentaries on the field, and which needs to be placed at the centre of everything that we do.

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