

Hannah Cobb · Oliver J. T. Harris  
Cara Jones · Philip Richardson *Editors*

# Reconsidering Archaeological Fieldwork

Exploring On-Site Relationships  
Between Theory and Practice

 Springer

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# Chapter 1

## Reconsidering Fieldwork, an Introduction: Confronting Tensions in Fieldwork and Theory

Hannah Cobb, Oliver J.T. Harris, Cara Jones, and Philip Richardson

### Introduction

The boat returns to its noost providing a spectacular view of the croft. All is in order, ready for the coming winter, the peat is cut and the animals graze by the shore. The house stands on a small knoll overlooking the sea at Tobha Beag. A warm light flickers in one of its narrow windows and smoke drifts across the bay from its chimney. A stone outhouse lies within the well maintained vegetable plot and a lambing pen stands empty, awaiting the coming spring.

On the damp, October day, when two of the editors of this volume encountered the ruins of Tobha Beag on North Uist, Scotland, this is not what they saw. Yet the ruins of this building, as an empty shell, and an inert object, were not what they encountered either. Instead this rich narrative of Tobha Beag in the past that we paint here emerged from a complicated, multilayered experience of the site.

This volume examines the tensions that surround the subject/object dichotomy within archaeology, particularly between theory and practice. In recent times it has become increasingly fashionable to criticize attempts to produce archaeologies which rely on dichotomies to understand the past. Such approaches, which separate

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the social from the material and culture from nature, have rightly been accused of imposing a particularly modern way of looking at the world onto the past (Olsen 2010; Thomas 2004; Webmoor and Witmore 2008). Such an imposition prevents us from seeing how people and things in the past were mutually constitutive, rather than the former simply acting on mute materials to produce the latter. Within the dominant perspective that still relies on these dichotomies both the present and the past can be divided, in an uncomplicated manner, into active, thinking subjects (who possess agency) and unthinking, passive objects (who do not). This is a deeply debilitating perspective. For example, reanalysis of the modern world has shown that we do not *always* experience the world in terms of separated subjects and objects, and this has been supported by ethnographic studies that have revealed that other people divide the world up differently, and that subjectivity may emerge by different means (e.g. Ingold 2000; Vilaça 2005; Willerslev 2007). Furthermore, these studies have shown how the line between *who* is a subject and *what* is an object need not be hard and fast but rather negotiable, temporary and transient. 'Are *all* of the stones about us here alive? [...] No! But *some* are' (Hallowell 1960: 24). This has led some archaeologists to develop new theoretical approaches that do not divide the world into subjects and objects but rather emphasize symmetry (e.g. Witmore 2007; Webmoor and Witmore 2008), relations (e.g. Conneller 2011) or animism (e.g. Alberti and Bray 2009).

The papers in this volume turn their attention to how the subject and object dichotomy plays out within fieldwork. In particular, at the heart of this volume is a central problem. Archaeological methods are explicitly designed to divide the world into objects and subjects. This may disguise the ways in which archaeologists and sites produce each other throughout the archaeological process (Yarrow 2003, 2008), but it is nevertheless a central procedure within our practices. Archaeological methods are explicitly aimed at distinguishing nature from culture, objects from subjects. Thus whilst we applaud theoretical approaches that seek to escape this bind, we cannot help but notice its continuing centrality to our practice. This then is a crucial conundrum: how can we resolve to approach the past in a non-dichotomous manner when our very methodologies act to produce such dichotomies? This is not a conundrum we aim to resolve in this volume – indeed it may not be resolvable. Instead the papers here examine various different aspects of the profession (CRM, academic and commercially driven excavations, historic and prehistoric archaeology, in the UK, Europe, Scandinavia and America) in order to profitably explore this question.

## Subjects Versus Objects in Archaeology

It is important to begin this volume by briefly elucidating what we mean by subjects and objects. To do this let us return to the encounter of Tobha Beag (Fig. 1.1). This site was one of many recorded by The North Uist Project, a coastal erosion survey undertaken in 2006 by CFA Archaeology Ltd, and sponsored by the SCAPE Trust (Johnson et al. 2007). This project worked within a methodology set out by



**Fig. 1.1** Map showing the location of North Uist on the west coast of Scotland, and the location of Tobha Beag (Produced with <http://www.nationalarchives.gov.uk/doc/open-government-licence/>)

best practice guidance (for example English Heritage 2007) in order to assess, through field survey, what archaeological sites may be at risk within the tidal zone of North Uist.

Field survey can involve a range of different strategies but usually employs the same technique of walking, often in transects, across the study area, to seek out upstanding archaeology. This was the methodology employed by the North Uist Project. Consequently, upon the discovery of Tobha Beag, the archaeological processes employed were simple and standard. The baseline condition was recorded – measurements were made and these were noted along with a basic description of the site, photographs were taken, and finally an attempt was made to put the site within a chronological typology. In this traditional framework, the records are packed away until returning to the office where an interpretation is developed in relation to a wider landscape characterisation of the study area.

This is a familiar experience and one that can be seen as being entirely underwritten by the subject/object dichotomy. In this example the archaeologist is the subject; finding the site, recording the ruins, characterising the landscape. The site, and the archaeology, is the object of enquiry. The site is measured, described, photographed and interpreted, and when this is finished, when it is no longer the object of enquiry, it is abandoned, the survey moves on, rounding the next headland and descending into the next bay. This understanding of the dichotomies between active, interpreting subject, and the passive, studied, gazed upon object is ubiquitous

in western modernity as numerous scholars have pointed out. Furthermore these bifurcations have a complex relationship to enlightenment thought, where many were first explicitly codified.

As such this perspective is historically situated and there is no reason to assume that it is universal. It may be the case, as Miller (2005) claims, that people around the world today continue to divide things into subjects and objects, but this is not necessarily true for the societies that archaeologists uncover. As anthropologists like Ingold (2000) and Viveiros de Castro (1998) have shown, in other groups subjects are not clearly divisible from objects. Stones, rocks and mountains can be seen as having elements of subjectivity, of being alive. As Ingold (2000: 91) points out, it makes no sense to see this as examples of anthropomorphisation – people are not imputing human like qualities to stones. Rather it emerges from a world view where the lines between subject and object are not fixed and the aliveness of things is revealed in their actions.

Not only does any simple division of subject from object not capture the realities of life amongst other communities, nor does it in reality capture the complexities of archaeological fieldwork either. Our encounter with the archaeology is affective. As Yarrow argues:

the material properties of the site act to modify the thoughts and actions of the people who excavate them. Just as the archaeologists created the objectivity of the things that were excavated, so the things that were excavated created the people excavating them as ‘archaeologists’. Without the material engagement with features that occur in excavation, the people would not be made to think or act archaeologically.

(Yarrow 2003: 71)

Thus a more considered examination of the craft of archaeology (*sensu* Shanks and McGuire 1996) reveals it is not simply a case of subjects uncovering objects, but rather subjects and objects emerging through mutually interlocking exchanges. Thus the archaeological method does not *reveal* a world divided into subjects and objects but rather *produces* one (Yarrow 2003: 67). Again as Yarrow (2003: 67) has pointed out, it is the very methods we use that allow archaeological objects and features to become visible by explicitly dividing off subjects from objects.

In the light of our earlier discussion of the particular and historical nature of this divide this is problematic. In dividing the world into subjects and objects we risk colonizing the past with a particular way of looking at the world that we can loosely term modern. Rather than building from the bottom up or tacking between theory and practice this perspective begins by imposing a particular way of looking at the world from above.

## Archaeology as Modernity

This of course relates to the history of our method of study. Archaeology as a discipline emerged as one aspect of modernity, becoming ever more prevalent in the nineteenth and twentieth centuries (Trigger 2006; Thomas 2004). As it did so it developed a methodology that focused on recording the past in a way that unambiguously placed

the archaeologist in the role of the subject, and the materials that were recovered in the role of objects. These objects could be excavated, classified, measured, recorded, published and archived in what seemed like a relatively straightforward manner. As Lucas (2001) and Thomas (2004) have shown, the emergence of this tradition was not arbitrary but rather was part and parcel of the ways in which modernity began to divide up the world it saw around itself.

This is not a surprise, nor is it sufficient merely to recognize this tendency to divide the world up as a philosophical problem. As Miller (2005: 14) has pointed out, in the modern world it makes no sense to go around telling people (we might say archaeologists) that they are wrong: that the world is not divided into subjects and objects. This may be philosophically true, it may even reveal truths about people's engagements with things they did not realize before, but it does not stop people from doing it (Miller 2005: 14). Thus it may be the case as Ingold (2000) argues that the Ojibwa do not anthropomorphize their world because they do not begin by breaking it up in a dualistic fashion. However, when a western man or woman curses their computer for failing them they *do* believe they are imputing human characteristics to a non-human object, rather than revealing the ways in which this split is false. Dualities of subject and object may not be philosophically true but they are part of the realities of life in the modern world, as people understand it. Thus whilst we applaud Webmoor and Witmore's (2008) recent call not to try and bridge the divide between subject and object, but rather to undercut it, we question whether this is entirely plausible by itself within field practice. Indeed if we take Webmoor and Witmore's favorite theorist – Bruno Latour – seriously this is exactly the point he makes.

For Latour (1999) everything caught up in a network is capable of influencing it, of being an 'actant', in his terms. This applies equally to material tangible things, like keys, soil and microbes, to list a few of his favorites, but also to ideas, images and concepts (Harman 2009). From this perspective whether or not we believe subjects and objects make the best tools for describing the world, from a philosophical perspective, is not the point. Conventional bifurcations like subjects and objects are indeed, as Witmore argues 'the outcomes of relations with particular entities of the world and not the starting point' (2007: 549), but these outcomes are real nonetheless. What matters, ethnographically (*sensu* Edgeworth 2006), is that archaeological methods are designed to *create* this dichotomy: it is this that makes them real in the present, whether or not they existed in the past. The logical conclusion to the arguments put forward by Yarrow and others is this: that archaeology is itself the very process that divides the past into subjects and objects. By no means does this describe the totality of what archaeologists do, but it is an essential aspect of it nonetheless.

## The Conundrum

This raises a difficulty, because not only does current archaeological field practice presume the split was true in the present, it also presumes the split was true in the past. Our methods, which divide the causes of a ditch being filled into either human



effort or natural silting, which divide worked from unworked stones, which presume that certain things are worth recording and others not, divides the world in countless ways in the present, and by implication in the past. Until recently such divisions were presumed to be unproblematic. As we have begun to wrestle with the problems inherent in such a dichotomous view of the world so we have begun to wrestle with consequences. This then returns us to the conundrum at the heart of this volume. Archaeological practice divides the world up in a way that directly imposes one set of views on to the past, yet archaeological practice itself only makes sense within that set of views. How then, can we deal with this?

One approach has been to deal with this difficulty ‘after the fact’, to view it as a problem for interpretation rather than one of practice. Thus plenty of archaeologists have in recent times set out the difficulty with dichotomous approaches to the past, and offered reinterpretations of sites which more or less successfully offer us new understandings (we too are guilty of this). Different authors have offered us the possibility of overcoming dichotomies through a counter-modern archaeology (Thomas 2004), through an escape from dialectics (Cochrane 2007), or through a symmetrical archaeology (Webmoor and Witmore 2008). A recent book edited by Russell (2006) is subtitled ‘moving beyond modern approaches to archaeology’. Yet there is little detailed engagement with archaeological field practice.

Where it does come up – practice after all is Witmore’s ‘first matter of concern’ (2007: 549) – the ‘concern’ is less with how we dig and record, but rather with *how we understand how we dig and record*. This applies equally to Yarrow’s (2008) excellent discussion of context sheets. This seems to us to be problematic as it suggests that how a site is excavated does not matter. Witmore’s strategy, that we should pay sufficient attention to the translations that take place through excavation and recording is excellent, and we entirely agree that much can be learned from this Latourian approach. However, we also believe wholeheartedly that how we actually dig and record really matters. This means that new strategies of recording in particular need to be developed (see the Ardnamurchan Transitions Project, this volume).

Far more troubling than this, however, are theoretical approaches that do not concern themselves with practice at all, which implicitly suggest that all can be fixed by correctly applying the appropriate theoretical balm, however defined, after the dirty work of excavation has been done. This in turn reduces the role of fieldwork to data collection (of objects) waiting for later interpretation (by subjects) (Lucas 2001). It seems strange to be so critical of modernist dichotomies in interpretation yet so content with their role in excavation. It also has political implications for how we view archaeological skill sets, privileging one level of interpretation and degrading another.

Let us be clear: we are not denigrating approaches to archaeology that have tried to move beyond modernist dichotomies, indeed we find them inspirational. What we want to draw attention to, however, is their comparative silence on the actual *practice* of archaeology itself and how this creates the very dichotomies we want to overcome. In many cases, of course, we can only interpret sites after they have been excavated, and from that point of view we have to apply our theories after the fact. However, the acknowledgement that interpretation takes place at the trowels edge

(Hodder 1997), or more accurately in the negotiations that take place around the trowel's edge (Yarrow 2003), means we cannot wash our hands of the way our excavations and wider field practices make certain choices and how these impinge on our interpretations. We are not suggesting that field practice can escape from the context it takes place in, but nor do we think it likely that theory can just smooth away these difficulties. Instead we need to face up to the inherent difficulties of this process, and tackle them head on. Let us consider this argument in practice by returning to our experiences on North Uist.

## Subjects and Objects in Field Survey

To outline the traditional subject/object divide we have used our survey work at Tobha Beag on North Uist as an example. But in reality, field survey, and our experiences at Tobha Beag provide an excellent example of how the issues we outline above and the tensions they raise, were confronted.

Small crofts, such as Tobha Beag, are a common element of the landscape of North Uist. They usually consist of small plots of land worked by extended family groups. People keep sheep, work small horticultural plots and fish. They cut peat for fuel and build from locally quarried stone. A typical croft may consist of the family house, outbuildings, associated quarries and small cultivation plots, known as lazy beds. The particular croft at Tobha Beag contained a suite of multi-period remains (see Table 1.1) including a house built in the 1930s, when the land was enclosed, creating the current series of small crofts. The house it replaced stands to the north, marked by concrete and stone remains, while the stone wall remnants of the first house are discernible to the south. The remains of a sheiling (a shepherd's seasonal dwelling) are by the shore and field walls and boat noosts complete the picture.

This is how the site was recorded, each element taken individually and described. Each element was noted to be post-medieval in date and was recorded in such a way that the past was flattened. The multiple temporalities in its construction were reduced to a single generalising category. While recording this site a bemused crofter approached to observe the events and began to tell *his* story of the croft. He talked about the people, about the way the land was worked and how life was maintained in a small crofting community. What we recorded as stone built ruin, he saw as the village shop, reused as a chicken coup. For him, all the elements recorded *were* the croft; each element was part of daily life and none belonged to the past. He recognised that each structure was not contemporary and that some had replaced others or been reused, but the archaeological knowledge we were creating was not recognisable to him. It didn't make sense because of the manner in which subjective temporalities were reduced to objectified periods.

Within the field survey objects were split from subjects. These objects reflected particular concerns and methods, for example, to record all the sites we encountered within 100 m of the coast. However, it is in the tensions between subject and object that interesting archaeological knowledge is created. What may at first glance

**Table 1.1** Showing the data collected at Tobha Beag

Feature	Original date	Archaeological classification	Current use
House	1930s	Post-medieval	House
Sheiling	Early twentieth century	Post-medieval	Animal pen
Corrugated iron house/shop	Early twentieth century	Post-medieval	Abandoned, originally a house, then shop prior to construction of Loch Portain road in 1960. Iron reused as roof of blackhouse
Stone blackhouse	Late nineteenth century	Post-medieval	Former house, now byre
Boundary wall	1930s	Post-medieval	Part of land divisions derived from 1930s crofting activity
Peat cuttings	Still in use	Post-medieval	
Lazy beds	Early twentieth century	Post-medieval	
Stone quarry	Multi-period	Post-medieval	Stone quarried for building of sheiling, blackhouse, boundary wall and field dykes
Three boat noosts	Multi-period	Post-medieval	One still used for fishing boat
Stone field dykes	Early twentieth century	Post-medieval	System of field divisions and animal enclosures

resemble a single event actually incorporates a palimpsest of multiple events and time scales. Traditional approaches usually focus on one particular period or temporality when they attempt to reconstruct past landscapes, thus in the present day our understandings of landscapes as a whole are often presented in a fragmented series of different periods, each separated from the other. In contrast the English Heritage Landscape Characterisation Project (Aldred 2002) suggests that landscapes are not a collection of fragmented, fossilised scenes of various periods but a *historical process* '[...] incorporating multiple temporalities which have different resonances in the present day' (Lucas 2005: 41). Such a characterisation sees the recognition of the multi-layered temporality of the past in wider archaeological frameworks. One account that takes up the issue of a multi-temporal present is Olivier's (2001) 'house'. This building, constructed towards the start of the twentieth century, lies in sight of a range of older structures dating back to the seventeenth century. Time here, for Olivier, becomes compressed as multiple different periods coalesce and impress upon the present.

Whilst Olivier's account draws out the problem with multi-temporality the result reflects the ways in which the features would have been recorded during field survey. However, in practice the different 'periods' described are actually *the present* for the people who live and work in such places (cf. Witmore 2006). The conditions of any present are never separated off for its inhabitants. Subjects and objects never truly part company, and this was explicitly demonstrated at Tobha Beag, where with the insights of the croft holder and the detailed analysis of the phasing of the structures we were able to record a rich history of one small part of

North Uist. Returning to our earlier discussion of Yarrow's work, archaeological method (in this case survey work), did not *reveal* a world divided into subjects and objects but rather *produced* one (Yarrow 2003: 67). In turn recording and incorporating another kind of knowledge (the crofter's) revealed a different type of history. Crucially, *both* these forms of knowledge are of essential importance in interpreting this site and its past.

## A Return to the Conundrum

The field survey discussed here reveals in more detail the complex tensions within fieldwork that surround the production of subjects and objects and the manner in which historical experiences, in this case that of a local crofter, reveals other, more complex, realities. Is the crofter's view point truer than an archaeologist's? Not solely no, his history too is partial and incomplete, coupled with its own predetermined realities. Yet in recording his views alongside those of traditional archaeological methods alternative narratives can emerge.

How can we, in the absence of such eye-witness testimony, move beyond the simple dichotomies our very methodologies use to make the past visible to us? The answer we believe is not to call for the creation of some new methodology, one that would somehow free us from the creation of both objects and subjects through our work. How would the past ever be visible to us if we were not routinely willing to separate culture from nature and subject from object in the course of our practice? Instead we propose the need for strategies of *mitigation*, particularly in the manner in which we record what we find. The example of the field survey serves us well here: we need to find ways to record the crofter's memories as part of the archaeological process, to include forms of recording that trace and reveal the way in which archaeology divides up buildings and the consequences of us separating the past, flattening and hiding its differences. In part this could be seen as an argument for a wider implementation of Hodder's (2000) reflexive methodologies. Yet we would also suggest we need to go beyond that, to develop tools to allow us not merely to reflect on what we have done, but that captures the process as it unfurls, that is explicit about how and when decisions are taken that make particular kinds of past visible (see Ardnamurchan Transitions Project, this volume). We need to be more willing to think and record alternative possibilities in our practice, and these need to be implementable within real world budgetary constraints, which are getting ever tighter given the current global financial turmoil. The papers in this volume all provide examples of exactly this: real world means of exploring, recording and examining alternative possibilities and multiple pasts. This does not mean that they all agree, but rather they provide a range of diverse ways of examining the conundrum outlined here.

The first papers in this volume take a historical view of the development of archaeological practice. Carver, for instance, explores how the key principles of uniformitarianism, the three age system and evolutionary theory underlined the

eventual recognition, in 1859, of the antiquity of prehistoric artifacts found at Brixham Cave in the UK. Using this case study, Carver demonstrates that the tension between theory and practice is endemic not just in modern debate, but in the foundations of the discipline itself. Following Carver's arguments, Thorpe traces the more recent roots of commercial archaeological field practice in the UK. Here he highlights the reflexive and discursive practices of 1970s and 1980s rescue archaeology to argue that notions of the reflexivity of archaeology have much earlier roots than the standard narrative, which sees them first discussed in the 1990s. Yet, Thorpe suggests, what has subsequently eroded this reflexivity is not simply related to the prevailing theoretical paradigms in the discipline, but rather the manner in which archaeological practice has become enmeshed with consumer capitalism since the early 1990s. Everything from recording methods to staffing hierarchies are driven by cost and time restraints, and until we recognize this, Thorpe contends, we will not be able to return to reflexive field practices whatever our theoretical stance. Both Carver and Thorpe disagree with the central approach advocated by this chapter, and thus provide essential multivocality within the volume as a whole.

Wilkins' paper develops this critique further by critically examining present day commercial practice in Ireland, with comparison to the UK. In particular he interrogates the tensions between the strategies in place to mitigate against the impacts of construction on archaeology and the production and dissemination of archaeological knowledge. Following on from Thorpe's paper, Wilkins demonstrates that the disjunct in these two crucial areas is a product of the contradictions that arise from applying a consumer capitalist model to archaeological fieldwork. The archaeological market, Wilkins contends, is an artificial creation in which developers have no interest or control over the archaeological product that they are purchasing, which in turn keeps prices of archaeological work (and therefore wages, conditions, and the quality of archaeological output and dissemination) low. Under these circumstances, and without any challenge or changes in practice, it is clear that the division of subjects and objects, the alienation of theory and practice, and fundamentally the disenfranchisement of the digger, will remain simply perpetuated in an unending and unquestioned cycle.

How then do we challenge and break this cycle? To get at this question the following two papers in the volume turn to the relationship between epistemology and field practice. Lönn draws upon field practice in Scandinavia to interrogate the ways in which discoveries in the field, and our knowledge of what things are, develop through a series of discursive processes. Ultimately what Lönn points to is the hermeneutic nature of field practice. Of course hermeneutics in archaeology is not new – as early as the 1980s Shanks and Tilley (1987) drew our attention to this – but few have explored this specifically in relation to *archaeological practice*. The processes of knowledge production are similarly the subject of Jones and Richardson's chapter. They draw upon their own experiences of excavating field drains in the UK to critically analyze the consequences of how we weave unfamiliar encounters in the field into narratives of familiarity, classification and typology. By rendering the past, and our methodologies for examining this, comfortable, they argue that we can easily overlook the nuances of past lives. The time and financial

pressure that are often applied to current excavation practices drive us to apply known frameworks and seek easy answers, which in turn mask the complexities of our engagement with the archaeology in the present and our interpretations of the past.

The final papers in this volume turn towards a range of different methodologies to confront the subject/object relationship in their field practices. Carman and Carman, for example, explore the application of a phenomenological approach to historic battle-field sites in the UK. The Ardnamurchan Transitions Project instead applies different recording methodologies to capture the interpretive process, to empower the digger and to develop the pedagogical value of their project in western Scotland. Writing is also at the heart of Yamin's paper. She challenges exactly the kind of comfortability that Jones and Richardson outline, by exploring the process of writing narrative vignettes on two CRM projects in the United States. These vignettes draw upon the archaeological material in the present to explicitly develop a fictional account of life in the past. Yamin demonstrates that writing such "unscientific" narratives, and incorporating them into the final reports of what were two standard CRM projects, subverted hierarchical archaeological practice. It did this by giving voices to diggers in the present whilst at the same time producing a quality and engaging product that challenged the alienating nature of archaeological practice under consumer capitalism. Finally Leone et al. examine the role that archaeology played in the tercentenary celebrations of Annapolis, Maryland, USA. Here public excavations and the display of artefacts, in both museums and more accessible spaces, were intended to inspire people not simply to look backwards, but to explore how liberty and democracy were and are created in the past and the present. Inspired by the Marxism of Eagleton, Leone et al. explore how reflexive practice, and specifically the challenge of archaeology and materiality as socially alienated, were played out both in the act of excavation, and in the consideration of how excavated material was displayed, accessed and integrated back into present-day narratives of liberty and freedom.

Almost all of the papers in this volume hint at an explicit return to dialectics (McGuire 1992, 1998). We have already noted the recent critique of this tool: that it makes no sense to try and put things back together that we ourselves have divided (Cochrane 2007). However, the strengths of this volume are reflected in the recognition that division is precisely what archaeology does. Therefore, in the various strategies that the papers here have proposed to mitigate this, it is clear that dialectics offers a tool that we can incorporate into our recording strategies (as in the case of the Ardnamurchan Transitions Project), and into how we think about, write about and present the past (e.g. Lönn, Jones and Richardson, Yamin, Leone et al., all this volume) by allowing us to tack back and forth between competing strategies. However much we criticize the subject object divide for being a product of modernity, archaeologists will continue to operate under its auspices (cf. Miller 2005). As Latour (1999) would say, just because it's fabricated doesn't mean it's false! The only option therefore is to devise strategies for practice that allow us to both record the process of bifurcation as it happens and also to mitigate its consequences, to record other kinds of past, like those related by the crofter encountered on North Uist, or those proposed by Carman and Carman at historic British battlefields, Yamin in New Brunswick and Leone in Annapolis.

## Conclusion

The papers in this volume attempt to explore a conundrum: given that archaeological methodologies are designed to produce subjects and objects out of our encounters with the material, and given that this division is further enhanced under the pressures placed upon archaeological practice in modern consumer capitalism, how can we begin to interpret worlds where that material is not so divided? One option might be to declare that how we produce archaeological knowledge doesn't matter, what matters is what we do with it when we have it. Thus however much the subject/object dichotomy might lie at the heart of our methods an application of theory can cure it. This we find problematic not only in that it reduces fieldwork to the practice of simple data collection, when we are fully aware that it is actually a directly interpretive process, it also has unpleasant political consequences regarding the attribution and reward of both power and skill (Shanks and McGuire 1996). A second option might be to throw out the baby with the bathwater, to start again and attempt to envision an archaeological methodology that does not divide the world in this way. This, we suggest, would be doomed to failure because archaeological practice relies on these poles to make the past visible. They are unquestionably part of a modernist agenda, but then so is archaeology. It is hardly surprising that the two depend upon one another. Thus we propose what we hope is a very un-Blairite third-way. We need to develop strategies for fieldwork, including survey, excavation and post-excavation, which embrace archaeology's complex position. We need new recording strategies that are not only reflexive but also multi-dimensional, that can capture the emergence of the past as object, the archaeologist as subject alongside other emergent possibilities. Thus these methods need to be rigorous and continue to record the object data we are familiar with, and to offer alternative means for exploring the production of the past. It is through celebrating archaeology's position as creator of the past whilst mitigating against the realities of archaeological practice that new kinds of ontology can be proposed for archaeology that rely neither on theory-as-panacea nor fieldwork-as-neutral arbiter. Instead, as the papers in this volume demonstrate, archaeology can emerge as a discipline that mediates between different kinds of knowledge, between subjects and objects and between local meaning and wider truths. In developing new kinds of field practice, and with them new ontologies that celebrate its craft, archaeology offers a way of undercutting dichotomies not by ignoring them, but by recognizing their central role in what we do.

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## Chapter 2

# How to Archaeologize with a Hammer

Geoff Carver

Some of the excessively “theoretical” papers presented at the EAA conference in Gothenburg in 1998 got me thinking about how speculative theories are all very nice, but, in so many cases, how could you even begin to find evidence to support them? It sometimes seems like the apocryphal scholastic debates over the number of angels dancing on the head of a pin.

So it seemed like time to get back to basics, to step away from theory and look at practice. I had already excavated in Canada, England, France, Denmark, the Netherlands, Germany and Israel – I have since also worked in the United States – and had seen something of the variety of methods out there. As a result I was starting to wonder whether these differences were significant or not; if they affect data quality when you’re doing regional – European – studies and, if so, what we might be able to do about it. But first, there seemed to be a need to collect some very basic data on just how people excavate and document their excavations. So I organised a series of sessions at EAA conferences in Bournemouth (UK), Lisbon (Portugal) and Esslingen (Germany) under the title “Digging in the Dirt” (cf. Carver 2004a).

Things went OK. We didn’t have a big crowd, but it seemed like we were making progress and getting some discussion going until the Esslingen session was cancelled because (the organizers argued) methodology – excavation technique – was deemed to be a subject *not* suitable for discussion by archaeologists (Rüdiger Krause, personal communication [cf. Carver 2004b:vi]). This may seem strange, given that the present volume discusses this very topic, and I assume that many (if not *all*) of the participants and members of the audience at both the EAA and TAG sessions at which this paper was originally presented – plus the readers of this book – probably either are, or at least consider themselves to be, archaeologists. But this attitude does reflect something fundamental about the division of labour on German excavations, where archaeologists

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generally supervise and identify artifacts, while technicians (*Techniker*) deal with technique/methodology (Planck 1999: 16; VLA 2006: 9). So it is almost impossible to discuss some of the finer nuances of Single Context Planning or reflexivity in Germany, where field methodology and excavation technique are subjects for *Techniker*.

I should perhaps emphasise that I usually contrast British and German archaeologies because they make good examples. They provide clear historical, cultural, and institutional contrasts (the French and Swedish examples are not so clear since the French parallel the British fairly closely, while the Swedes seem to have been influenced by the Germans until the war and the British after). They may also be more familiar than, say, Albanian or Azerbaijani archaeology. Despite the assertion that “it is widely recognized that German archaeologists dig with a different method” (Hodder 1999: 9), this assumption of “familiarity” may be somewhat optimistic, though, given how difficult it can be to convince British archaeologists that a lot of things many British texts seem to take for granted – Single Context Recording, the Harris Matrix, even processualism and post-processualism – are *not* universal.

It is also interesting to see the reaction when my British colleagues are confronted with something as basic as the fact that archaeology is two disciplines in Germany. *Archäologie* (English-language *Classical Archaeology*) has stronger ties to the art history from which it derives than its British counterpart. German prehistoric archaeology is clearly a separate discipline, with separate university departments and institutes, societies, etc., and known as either *Ur- und Frühgeschichte* or *Vor- und Frühgeschichte*. The significance of this distinction can be underlined by trying to translate that old processualist adage that “archaeology is anthropology or it is nothing” (generally, although misleadingly, attributed to Binford 1962) into German. While there are difficulties just in translating this from an American to a British institutional landscape (where archaeology is clearly *not* a sub-discipline of anthropology), the German is further complicated by the fact that the German discipline of *Anthropologie* is what is known in English as *physical* – but not social/cultural – anthropology. Little wonder, then, that Bernbeck (1997: 37), Veit (1998: 122) and Kümmel (1998: 122) do not even try.

The point is: when trying to think cross-culturally, we must take into account such strange and unexpected factors as resentment towards what is often perceived to be Anglo–American hegemony in theory.

After “Digging in the Dirt” I went on to pursue a number of other projects which seem every bit as daft: trying to link reflexivity and AIS [Archaeological Information Systems], for example (Carver 2006). A dissertation on stratigraphic theory often seems equally pointless, given that:

Once the principle of stratigraphy was established as a reliable technique for inferring chronology, it continued to be used by archaeologists regardless of what other view they might espouse.

(Trigger 2006: 9)

The present volume and the conference sessions from which it derives aim at examining post-processual approaches to archaeological fieldwork. It is possible that something in the nature of post-processualism itself might provide an answer:

As post-processualists have so cogently argued, *reading* and *writing* are the real material practices of (academic) archaeology – which consist more of the production of texts, the

compilation of bibliographies, the reading of relevant literature and the citation of references, than it does of the actual digging of material remains.

(Edgeworth 2003: 3 [original emphasis])

I will go out on a limb (I have no data to support this, and am not aware of any cross-cultural studies) and suggest that, based on general impressions and extrapolating from the relatively limited spread of processualism, post-processualism is really only a major force in Anglo-American archaeology, and even then primarily in academia and not in the developer-funded (i.e. “rescue”) contexts where most work is now being done.

## Theory and Practice

So looking at this question of why post-processualist theory does not seem to have influenced method, we can start by recognising that this only seems to be an issue in Britain, where “it seems that almost every famous field archaeologist has written a book about excavation methodology” (Watson 2004: 75). Some of the more obvious (though not necessarily “famous”) examples include Petrie (1904), Droop (1915), Atkinson (1953), Wheeler (1954), Kenyon (1956), Crawford (1953), Barker (1998), Drewett (1999), Collis (2001), and Roskams (2001). This tradition has no parallel in German *Archäologie* – for example (compare Gersbach [1998] and Biel and Klöckner [1994: released in sections and – perhaps significantly – never completed]). And this is the tradition in which people like Ian Hodder (1999), Gavin Lucas (2001a), Adrian Chadwick (1998, 2003), and the contributors to the present volume discuss post-processual field methods. On a larger scale, there is suggestion that the initial premise of this discussion is misleading, in that *theory does not influence methodology quite as much as some people would like to suppose*.

## Brixham Cave

As an example, the following will focus on the early debate surrounding the association of stone artifacts with the remains of extinct fauna, specifically from the controlled excavation at Brixham Cave in 1858–1859. We all know the story about how everything came together in the *annus mirabilis* of 1859 for a “revolution in antiquarian thought which transformed the dilettantism of antiquaries into the historical research of archaeologists” (Daniel 1975: 52). That is the three-age system, uniformitarianism and the publication of Darwin’s *Origin of Species* allowed a *science* of archaeology to overthrow superstitious religious objections to the idea that human ancestors and extinct fauna were contemporary. There are several variations on this theme. Renfrew and Bahn (2000: 25), for example, list the “three great conceptual advances” that “offered a framework for studying the past, and for asking intelligent questions

about it,” as “the *antiquity of humankind*, Darwin’s *principle of evolution*, and the *Three Age System*” (original emphasis). Compare Daniel’s “three contributory sources” for “prehistoric archaeology”:

Prehistoric archaeology as we know it has three contributory sources – the advance of geology, the pushing backwards of the frontiers of history by archaeological means and, thirdly, the growth of archaeological technique out of antiquarianism.

(Daniel 1975: 54)

As has been noted, the French genealogy broadly parallels this schema:

At the start, in the middle of the nineteenth century, French prehistoric archaeology was influenced both by the natural sciences, geology and palaeontology, and by the new-born cultural anthropology. From the former two, it borrowed a chronological frame and notions of stratigraphy [...]. From the latter, it acquired an ethnological vision of prehistoric man. From all three, it adopted the leading paradigm of the century: evolutionism.

(Audouze and Leroi-Gourhan 1981: 170)

And as an example of a genealogy to a more general archaeology, consider the following:

Three major intellectual currents reached fruition in the middle of the nineteenth century, setting the conceptual basis for archaeological interpretation. First [...] the geologist Charles Lyell proposed his principle of superimposition, or uniformitarianism. [...] Second, Thomsen and Worsaae proposed the three-age system... Third, Charles Darwin published his *Origin of Species*.

(Redman 1999: 49)

Detailed, historical study of the primary sources shows that superstition and theoretical considerations actually played little or no part in the debate at all:

some older accounts of the establishment of human antiquity... tended to picture the issue as one in which conservative theological views were pitted against progressive scientific ones and often against the facts themselves, provide an inappropriate characterization of the questions involved. In England, for instance, the influential geologist William Buckland was not the stultifying force regarding the establishment of human antiquity that he has been painted to be, and the geology of the even more influential Charles Lyell was not the key to the answer.

(Grayson 1983: 9)

Up until Brixham, the evidence had been discounted for any number of very practical – *non-theoretical* – reasons. Daniel argues that “The discoveries in Devon and on the Somme were announced to a world which was *now prepared to accept them*” (Daniel 1975: 55 [emphasis added]), but as far as the geologists themselves were concerned – and the fact that Brixham and the Somme were *not* excavated by archaeologists is worth emphasising (cf. Grayson 1983: 2) – the evidence presented up to that point had not been convincing, largely because, it was argued, cave stratigraphy is complicated, difficult to understand, etc. (Grayson 1983: 83). Their objections were variations on a theme: the contents of strata can be mixed by *disturbance* and *intrusions* (or *intrusives*). The association of a human burial with the remains of extinct fauna found in Paviland Cave, for example, was “explained away” as relatively recent “ovenpits” dug into the bone beds:

Buckland's opinion was that ancient Britons had dug ovens in the travertine, and that stone artifacts had found their way through these openings into the diluvium beneath. MacEnery disagreed, both because of the difficulty in breaking through the hard crust and because there was absolutely no evidence of such prehistoric excavations: "I am bold to say that in no instance have I discovered evidence of breaches or ovens in the floor but one continuous plate of stalagmite diffused uniformly over the loam".

(Grayson 1983: 75)

There are a number of ways to respond to this. One is to ask whether the primary documentation from Paviland show evidence of these "ovenpits," or was that just an excuse Buckland made up to explain away evidence he didn't like?

This in turn raises two problems. First because "excavation is destruction," the evidence to either prove or disprove the existence of these "ovenpits" no longer exists. Granted, this "excavation is destruction" argument (cf. Lucas 2001b) may have been repeated too many times. The following examples are just from Hodder's *Archaeological Process*:

- Excavation itself involves the removal of deposits in order to record them. As a result it becomes impossible to repeat the procedure. Digging is thus not like a laboratory experiment which can be repeated at different times and in different laboratories (Hodder 1999: 26);
- As so many have pointed out, a badly recorded excavation is worse than no excavation at all. Since excavation is destruction, the record has to be as accurate as possible (Hodder 1999: 31);
- Once the excavation of a particular block of soil has taken place, it cannot be repeated. The sampling must therefore be carefully constructed. And the sampling strategy depends on what it is that the archaeologists think they are excavating (Hodder 1999: 52);
- Archaeological excavation is destructive and non-experimental – we cannot repeat the experiment of the excavation (Hodder 1999: 55);
- A fixed definition of objects and contexts is required in archaeology. This is because [...] excavation destroys evidence. While we can return to the excavated artifacts to remeasure and redefine them into new categories, this cannot be done with soil contexts, etc. (Hodder 1999: 93);
- This emphasis on the act of digging is essential because [...] excavation is destructive (Hodder 1999: 103);
- It is not possible to go back to the soil contexts which have been destroyed in excavation (Hodder 1999: 118).

It almost seems that this, like our concepts of the three-age system, uniformitarianism and the evolution, has become a mantra we repeat to ourselves over and over and over again in an attempt to convince ourselves that it's true, in part to avoid close examination of the second problem, that of data quality. *Who* recorded the evidence? *Who* did the digging and *how* did they excavate? Was Paviland actually excavated and recorded in such a way that would have allowed traces of "ovenpits" to be recognised and/or properly interpreted, had they in fact existed? Or does it all come down to a question of who do we trust? Buckland<sup>1</sup> or Father John MacEnery, "a local naturalist" (Van Riper 1993: 61)?

In order to prevent interpretive problems like those at Paviland Brixham Cave was deliberately excavated *stratigraphically*, removing “only one stratum at a time [left] no doubt that all the bones removed during a given phase of the excavation belonged to the same stratum”, and “Pengelly’s documentation of the specimens removed from the cave was meticulous to a degree then unknown in geology or archaeology. It ensured that there could be no confusion, even long after the excavation, about where a particular specimen had been found” (Van Riper 1993: 87).

Even so, there was criticism, including complaints “that the excavation was moving so slowly that the London committee’s funds would be exhausted before the first layer had been dug out” (Van Riper 1993: 88–89). Perhaps the most revealing line of criticism comes from a debate between Charles Babbage and Joseph Prestwich published in the *Proceedings of the Royal Society of London*. Babbage – described as “one of the most interesting intellectuals of Victorian Britain” (Gould 2000: 162) – was a mathematician, computer pioneer and inventor of the locomotive cowcatcher (Swade 2004), and Prestwich was one of the members of the “Cave Committee” composed of a number of contemporary scientific celebrities,<sup>2</sup> sent by the Geological Society to supervise the Brixham excavation (Van Riper 1993: 82).

## Prestwich Versus Babbage

A preliminary report was presented by Joseph Prestwich to the Royal Society in 1859 (Prestwich 1860), and a final report in 1873 (Pengelly et al. 1873). Unlike a number of his contemporaries, Babbage accepted the identification of the stone tools: “Having examined a few of these flint-instruments, I am satisfied that several of them have been worked by human hands,” explaining that “this opinion is founded upon the previous examination many years ago of the mode then used for making gun-flints” (Babbage 1859: 69). He did, however, argue that stone tools could have been mixed with the bones of extinct fauna in earlier strata not as a result of “ovenpits” but rather because of earthquakes, because earthquakes *sometimes* cause cracks in the ground, and that artifacts *could have* fallen into these cracks:

Amongst the phenomena occurring during earthquakes, it has been observed that large cracks have suddenly opened and as suddenly closed, either immediately or shortly after. During these momentary or temporary openings, the remains of the arts of man, and even man himself, may have dropped into the chasm. Under such circumstances, remains of man and his arts might occur in formations of any date.

(Babbage 1859: 68–69)

Nowadays, there may be a tendency to dismiss such objections as unnecessary and extreme, given the associations between earthquakes and catastrophism (one also wonders how Babbage had the nerve to lecture Lyell, in his capacity as member of the “Cave Committee”, on uniformitarian principles). Prestwich did not object, however, and agreed that “[r]ents may have arisen from desiccation of the surface or from earthquake movements” (Prestwich 1860: 300). It is only when considering observable consequences that the two differ. Babbage suggests that direct evidence

(i.e. besides the presence of stone artifacts) of such “cracks,” “chasms” or “rents” might not be visible. Prestwich disagreed, noting that “such gaps would necessarily be filled up from the sides or from the surface, and a vertical seam of matter, differing more or less from the beds it cut through, would be traceable from the surface down to the flint-implements”, adding that, at the sites he had visited, “there is not the slightest appearance of such a state of things in these pits” (Prestwich 1860: 300). Rather, he pointed out that the deposits were of fairly loose gravels prone to crumbling when disturbed:

The same objection would apply to openings produced by earthquake movements, though to a lesser extent, as such might have closed up again and not remained open until filled up. Still, with gaps in such loose materials, and on the assumption that the flint-implements themselves fell into such gaps, other stones, dirt, and parts of the walls must inevitably have also fallen down and shown traces of the presence of materials foreign to the several beds; this is not the case.

(Prestwich 1860: 300)

In other words: *if* artifacts had managed to fall into these cracks, *then* other material would have fallen, too, so the cracks should have been obvious. Also that that the artifacts he had seen – hand-axes and so on – had all been found lying horizontally; and he argued that *if* they had fallen into cracks, then they should have been wedged into them vertically. He also noticed that patterns of mineral staining were consistent the artifacts having been in a horizontal position for a long time: on some pieces, only one face – but never one end – had been discoloured (Prestwich 1860: 300). He also noted the undisturbed stratigraphy *above* the layers in which the stone tools were found mixed with the bones of extinct fauna: “Also the fine lamination common in the bed of sand... forms continuous and unbroken lines (Prestwich 1860: 300)”, and so on. The whole debate – with numerous objections raised by one met by sober observations by the other – is well worth reading.

Fighting Babbage was easy for other reasons as well: his paper is so full of hypothetical cases, so many things which *might* or *could have* happened – even “extra” caves for which there was no evidence – that he strains credibility, especially when compared to Prestwich’s numerous, even excessive (and the Victorians loved detail), first-hand observations. Overall, this was not a fair fight. Babbage was a mathematician, and whatever qualities he may have had as a mathematician, he was unlucky enough to be arguing against a professional geologist at about the time when geology was starting to get serious about being a discipline. Maybe Babbage’s problem was that he had the misfortune of responding to Falconer’s reports on evidence from Maccagnone Cave, near Palermo (Falconer 1860), whereas Prestwich was presenting new evidence from Brixham and the Somme which was intended to address any shortcomings in the Italian evidence. But most importantly, and in contrast to the way this debate has been presented here, Babbage presented *his* paper *after* Prestwich had spoken. And one can almost imagine – even sympathise with – the poor man’s humiliation at having to get up and read a paper full of criticisms that had already been addressed, if it were not for that little matter of trying to lecture Lyell on uniformitarianism.



## Personal Testimony

Ultimately, though, it did not really matter what Babbage or any number of other critics said, because people like Prestwich and Falconer and others always had one insurmountable argument to fall back on: *I was there. I saw it, and you did not.* At the time when a gentleman was seen as being true to his word, Babbage's only recourse was, as in the case of Buckland's interpretation overriding MacEnery's observations at Paviland (cf. Lyon 1970: 75), to accuse Prestwich either of having been deceived or lying, neither of which (one assumes) a gentleman would do. One of the problems with Boucher de Perthes had always been the question of whether his workers could be trusted (cf. Falconer 1863; Grayson 1983: 131–132, 213). But then this was a problem that even the great Pitt-Rivers had trouble solving, with his crews comprised of

from 10 to 19 men [...], consisting chiefly of men of the neighbourhood, who happened to be out of employ, and who consequently could not be expected to prove themselves amongst the most efficient of their class. No more useful organization could be established for archaeological purpose, than that of a permanent Corps of efficient workmen.

(Pitt-Rivers 1892: 23–24)

Questions of training, good manners and breeding aside, this all goes back to the very origins of archaeology as a means for addressing the shortcomings of historical documents, of a systematic, scientific reaction against medieval scholasticism, an approach which emphasised personal experience and first-hand evidence over the Bible and Aristotle. Early archaeologists – antiquaries – opposed scholastic adherence to the written authority of classical authors and later commentaries, drawing on the ancient Greeks' emphasis on the experience of the senses, or "*autopsía*" (Vico 2001: 204 [499]; cf. Burke 2003: 276). In this context, one might even consider the derivation of "the original Greek term for 'historian,' which means an 'eyewitness'" (Mali 2002: 214).

If he needed to cite an authority, Prestwich could appeal to Bacon and this idea of *autopsia*. He could play on the myth of the liminal experience (Rudwick 1996), contrasting armchair and field geologists in a way that should be all-too-familiar to archaeologists today. But mostly he could appeal to common-sense scepticism, a reaction against past theoretical abuses – Babbage's "speculations" – of the kind which made early geology a target of ridicule. Lyell (1990: 25 [footnote]) noted, for example, that he had "happened to attend a meeting of the Geological Society of London" in April, 1830, where:

the president, in his address, made use of the expression, *a geological logician*. A smile was seen on the countenances of some of the audience, while many of the members, like Cicero's augurs, could not resist laughing; so ludicrous appeared the association of Geology and Logic.

Traces of this scepticism, still found in the divide between many "theoretical" and "field" archaeologists (cf. Bahn 1999: 13–15), helps explain why post-processualism has apparently had so little impact on field methods. For in contrast to Prestwich, Babbage only had hypotheses or theory to fall back on, at a time when

theory was not held in high regard. We see this reflected in contemporary views of what are now considered to be the foundations of the discipline: the three-age system, uniformitarianism and evolution. A reviewer of the English translation of *The Primeval Antiquities of Denmark*, for example, found the three-age system somewhat less than overwhelming:

The system of classification adopted is that of three periods – the stone, the bronze, and the iron, – to which all the antiquities preceding the epoch of Christianity are referred. Although this arrangement may be open to objections, *it would*, perhaps, be *difficult to substitute a better*, it being of course understood that objects which abound in one period may occasionally be found in another.

(Anonymous 1850: 161–162 [emphasis added])

We see this also in the way the prominent antiquary Thomas Wright criticised Lubbock for uncritically accepting the three-age system – a “mere delusion” sprung from “too hasty generalizing” and too little hard data (Van Riper 1993: 203; cf. Podgorny 2000: 21). Similarly, it is clear in the way that one of Lyell’s colleagues on the Brixham Cave Committee refers to uniformitarianism as a “doctrine” (Prestwich 1895: 1, 3, 6 [footnote]), a “*Fetish*” (Prestwich 1895: 8) and “an infallible faith” (Prestwich 1895: 3), with Lyell himself labelled [as] a “great prophet” (Geikie 1901: 181) and “great high priest of Uniformitarianism” (Geikie 1901: 281) in a work described as “a ‘standard’ history of geology for several generations,” and “the source for much continuing textbook dogma” (Gould 1987: 23).

That no one who includes uniformitarianism among the founding pillars of archaeology noticed Lyell saying that dinosaurs will return someday,<sup>3</sup> seems to suggest that, despite this tradition of *autopsia*, a failure to actually read Lyell may have lead archaeologists to misinterpret what uniformitarianism means, and what it implies. This interpretation derives in part from Redman’s rather strange equation of uniformitarianism with “superimposition” plus “a common tendency to dismiss uniformitarianism with a maxim that ‘the present is the key to the past’” (Gould 1965: 225–226; cf. Baxter 2003: 132; Carter 2007: 187; Geikie 1901: 281; Gould 1987: 67, 105; Stein 2000: 20), a tendency which actually

solves nothing; for this supposed explanation is as ambiguous as the original term itself. The present is a key either because we can extrapolate observed rates or conditions to past times [...] or because we establish our natural laws by observing present processes and then extrapolate the laws [...].

(Gould 1965: 225–226)

As for evolution what does it have to do with archaeology, anyway? If archaeology is concerned with *human* remains and palaeontology deals with ancestral hominid forms?

Prestwich dealt with none of these things. Despite the fact that he did not set out to prove the three-age system, uniformitarianism or evolution, 1859–1860 has been selected as the date of the glorious antiquarian revolution (cf. Evans 2009), when antiquarianism was replaced by the science of archaeology. We have repeated this particular mantra to the point where

This connection is now so intimate that it might be surprising that the establishment of human antiquity and the publication of Darwin's *Origin of Species* were not causally connected events, even though they occurred at virtually the same instant in time.

(Grayson 1983: 3)

Among the other problems with Glyn Daniel's scenario is the fact that this "antiquarian revolution" only seems to have happened in *British* archaeology, and only makes sense if – as is often the case, according to British usage, and in contrast to (for example) German – the word "archaeology" is understood to mean *prehistoric* archaeology (i.e. Crawford 1960: 15), everything else having been hyphenated: *classical* archaeology, *historical* archaeology, etc.

## Conclusion: Practice Against Theory

But mostly, the problem seems to reflect a mindset which values theory over practice. We have Bruce Trigger's *History of Archaeological Thought* (1989, 2006), for example, but no corresponding work on the history of archaeological praxis (Lucas 2001a and Eberhardt 2011 being possible exceptions). Given that "one of the most conspicuous traits of Trigger's analysis is the [...] importance he gave to works written in English in years when French was as (or even more) important as English for writing science and debating" (Podgorny 2000: 29), is it any wonder that, in academic, theoretically-oriented histories, the "antiquarian revolution" was not depicted as a victory for stratigraphic excavation methodologies?

My own interest in all this relates to stratigraphy, where Brixham Cave was a landmark. As has been noted, evidence of artifacts found with the remains of extinct fauna had been rejected either as "disturbance" or "intrusions" until Brixham. At Brixham, and later at the Somme, Prestwich looked, and he saw there were no cracks, that all the hand-axes had been oriented horizontally and that discolouration was consistent with the interpretation. In this case there had been no mixing, and a static model of stratigraphy, one in which strata are sealed and contexts are closed, was valid.

What Prestwich did not say was that this holds true in all cases. Because he and those of his predecessors who had rejected evidence from places like Paviland recognised that cave deposits, like soils, are dynamic, fluid ("soil is alive" [Frink 2003: 10] or "like a liquid" [Neumann and Sanford 2001: 137]) and prone to mixing as a result of any number of post-depositional transformation processes.

How did this dynamic model disappear? Why, after Brixham, is there a general assumption that all strata are sealed, that contexts are closed until Schiffer raised the issue again over a 100 years later? And what basis did Trigger have for argument that "the principle of stratigraphy was established as a reliable technique for inferring chronology" (Trigger 2006: 9)?

There are theoretical reasons: a perceived need to apply – or appeal to the authority of – Steno's "law" of superposition, for example (cf. Harris 1989: 5). And there are very practical reasons: how can evidence of disturbance be recorded? How can unclear layer boundaries be represented graphically? You can use a dashed or a

dotted line, but how do you draw a transition zone that is more than a line-width wide? Or what is the 3D equivalent of a dotted line in 3D computer graphics? The problem is with the way that theory so often (*too* often?) conflicts with reality. There are also ontological problems in defining layer boundaries. Usually we assume that they are clear, but this does not have to be the case, and so “the boundaries of the context are not ‘given’ – they are defined theoretically” (Hodder 1999: 85). In addition, archaeologists recognise that layers are defined on the basis of pragmatic and informal decisions made in the field (Stein 1987: 347; cf. Barker 1998: 173; Franken 1984: 17; Roskams 2001: 227).

More important, perhaps, from our perspective, is the fact that Prestwich faced another dilemma, one we still face today: how do you document the lack of – or negative – evidence for post-depositional transformations of the archaeological record? Reflexivity might offer a framework for recording uncertainty and something of the “pragmatic and informal” processes whereby we derive our decisions, usually after the fact, but not in real-time, while digging.

So long as documentation is interpretive and/or incomplete, we still only have Prestwich’s solution as our ultimate authority: *I was there, I saw that, so – in the words of Indiana Jones – “trust me.”* Prestwich was lucky enough to be going up against Babbage with all the big guns of the “Cave Committee” behind him; he wasn’t some lowly MacEnery fighting against a Buckland. Ultimately, though, none of these are processual, post-processual or even cultural-historical questions, or questions of English- or German-language archaeology. They are questions of methodology, fieldwork, analytical scale and documentation technology, and relate to some of the most basic questions of philosophy: how do we know something? And, more important for all scientists: how do we prove it?

Like any scientific discipline, archaeology requires “[t]he coordination of observers all over the globe,” and especially because of this problem that “excavation is destruction,” “observers [are] morally bound to report absolutely truthfully,” meaning archaeologists have to be what Ian Hacking (2002: 10) called a “scrupulous observer”:

“We have to have a kind of integrity most fields don’t need. I need your data, and you need mine, and we have to be able to trust each other on some basic level. There can’t be any backstabbing, or working in total isolation, or any of this sitting on a rock in the forest interpreting culture in ways no colleague can duplicate” (Flannery 1982: 276).

## Notes

1. “The first academic geologist in England, and he was the first to teach a geology course at an English university” (Repcheck 2004: 181).
2. In full the cave committee consisted of “Dr. FALCONER, F.R.S., F.G.S., Chairman and Secretary; Mr. J. PRESTWICH, F.R.S., F.G.S., Treasurer; Mr. W.M. PENGELLY, F.R.S., F.G.S.; Prof. RAMSAY, F.R.S., F.G.S.; Sir CHARLES LYELL, F.R.S., F.G.S.; Mr. R.C. GODWIN-AUSTEN, F.R.S., F.G.S.;

Mr. GEORGE BUSK, F.R.S., F.G.S.; Dr. PERCY, F.R.S., F.G.S.; Prof. OWEN, F.R.S., F.G.S.; Rev. R. EVEREST, F.G.S.; Mr. BECKLES, F.R.S., F.G.S., and the President and Secretaries of the Geological Society” (Pengelly et al. 1873: 475). Some of the more important members are worth identifying: Sir Charles Lyell, in addition to his status as a geological theorist and synthesizer, had defined the boundaries of the Pliocene and the other subdivisions of the Tertiary Period. Richard Owen, professor of comparative anatomy at the Royal College of Surgeons, was the most respected vertebrate palaeontologist of the day. Andrew Ramsay was a leading member of the Geological Society and an expert on the Drift. Joseph Prestwich had literally written the book on the Tertiary and Quaternary strata of England and France, and Hugh Falconer was Britain’s leading authority on the fossil animals they contained. Pengelly, the only committee member who was not also a member of the council, knew more than any of the others about the contents and geological setting of Devonshire caves” (Van Riper 1993: 83).

3. “Then might those genera of animals return, of which the memorials are preserved in the ancient rocks of our continents. The huge iguanodon might reappear in the woods, and the ichthyosaur in the sea, while the pterodactyle might flit again through umbrageous groves of tree-ferns” (Lyell 1990: 123; cf. Rudwick 1975; Grayson 1983: 78; Eliade 2005).

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# Chapter 3

## Often Fun, Usually Messy: Fieldwork, Recording and Higher Orders of Things

Reuben Thorpe

### Introduction

This paper has had a long gestation which began in 1997 as an article Chris Cumberpatch and I (Cumberpatch and Thorpe 1997) began to put together where we questioned the focus of the debate, played out in the pages of *Antiquity*, between Fekri Hassan and Ian Hodder (Hassan 1997; Hodder 1997, 1998). Later, in 2004, I was fortunate enough to be asked to contribute an overview paper to the proceedings of the Stratigraphy Conference held at York in 2001. Unfortunately the first paper was never completely finished and the publication of the Stratigraphy Conference proceedings has been cancelled. This chapter then draws together aspects of both papers, as the debate is still one with relevance today and includes an expansion of my thinking (up to June 2010) on other areas addressed by my original paper given in the *Reconsidering the on-site relationship between subject, object, theory and practice* session of the Theoretical Archaeology Group conference in at York in 2007.

In the following paper I agree with Shanks and McGuire (1996), Berggren and Hodder (2003) and Chadwick (2003) that for the actual excavator and specialist much current practice is characterised by alienation from the process of interpretation. Where I disagree is with the attribution of the causes of this alienation. In my view the causes do not lie in a tradition of pseudo-objectivity within British Archaeology, nor are they to be found in revisionist and partial readings of the history of the development of approaches to archaeological fieldwork in Britain. Alienation from the process of interpretation is not a consequence of processual field methodology, nor the specific absence of a post-processual field method. Instead, I argue that the current state of archaeological field practice in Britain is due almost entirely to the

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social, political and economic context of the production of archaeological data. I conclude that any attempt to (re)empower the interpretive arm of the excavator must actively engage with and address, first and foremost, these circumstances.

## The State We Are In?

Accounts of the conduct of the craft of archaeology in Britain (Berggren and Hodder 2003; Carter 2001; Chadwick 1998; Lucas 2001) have at their heart a premise that the practice of archaeology is made ‘bad science’ (Berggren and Hodder 2003:426) for four main reasons:

- A processual approach to methodology
- Standardised procedures, routine practices “practices of industrial production” (Shanks and McGuire 1996:80)
- A tradition of hierarchical and disempowering approaches to managing excavation
- That the hierarchies and standardised procedures have been accentuated and exacerbated by their transposition into commercial archaeology.

The result, it is argued, has been to foreground productivity, “efficiency and profit” (Shanks and McGuire 1996:81) through an “industrialised archaeology” (ibid.) which at best restricts and at worse removes the opportunities for the excavator to explicitly interpret that which they have dug. Furthermore it has been argued that this alienation, caused by a shift towards industrial style production, discourages active links, despite the existence of guidelines explicit on this (see English Heritage 1989, 1991, 2006), between excavator and specialist in the processes of excavation and assessment and research (Blinkhorn and Cumberpatch 1998, 1999; Cumberpatch and Blinkhorn 2001).

## Relations of Knowledge, Experiential Interpretation and Archaeological Data

You know these things as thoughts, but your thoughts are not your experiences, they are an echo, an after effect, of your experiences.

(Nietzsche 1969:12)

Contrary to what Pitt-Rivers and Wheeler had said,[...] the personal equation is always there and all of them did in fact choose what to record. The trick was to say how hard you looked, and why. Flinders Petrie by contrast had known this. Archaeologists did not dig up or record data; data was the result of an encounter in the ground, an encounter between earth and brain; you dug with your head.

(Carver 2006:7)

In the later part of the 1990s much was made of the divergence between the theoretical approaches to 'data' taken by processualist and post processualist archaeologies in the field (Chadwick 1998; Hassan 1997; Hodder 1997, 1998). In effect an extension of a wider debate in society that Wylie (2000:227) characterised as "science wars". In its most polarised form this debate contrasted ideas on one hand that the universe is knowable, that the truth is out there and that there are laws which can be discovered, and on the other hand a relativist, mystical worldview, which brings into doubt the "credibility of rational thought as a method of inquiry about the nature of the world and the place of humans in it" (Clark 1996:3).

Underpinning much of the post-processualist contribution to approaches to archaeological fieldwork is the assertion that reflexive method brings a radically new approach to the practice and management of 'data collection' (see below for a consideration of this). Key to this new approach is an argument concerning the nature of subjectivity and objectivity in archaeological field recording. It is to this that I would like to turn first.

As human beings, as archaeologists, we are socially situated and context-bound organisms. We use culturally specific criteria to make judgements about the nature of the world, our place in it, our conduct towards it and our conduct towards each other. We accept the potential existence of a category of objective perception and judgement but, at a practical level, we can never attain it given our contextually situated position in the world. Archaeological field methodology, in common with other aspects of human experience and practice, is not objective in any absolute sense, nor as a body of practice is it a "big science [...] like physics" (Clark 1996:3). Rather it is (and we as archaeologists are participants in) a culturally specific approach to understanding and interpreting the past and to the traces of human activity in the past. In the pursuance of our enquiries we archaeologists have collectively made certain decisions regarding the limits of our technical practices. These involve a core set of observations designed to be as fully representative of the observable and real, or experiential, world as is practical.

The creation of data and the derivation of meaning from archaeological sites is, on one level, achieved through the constant interplay of physical observation and recursive levels of interpretative association and reinterpretation. This process of generating both data and what Binford (1972:159) called 'running analysis' involves the dynamic interplay of participatory observation and association within a framework of stratigraphic possibility. Hirst (1976:11) characterised this as a "circular process of observation – question – hypothesis – and testing...against observed data", and elsewhere it has been called a dialectical process (See also Chadwick 2003:107; Edgeworth 2003: ix; Thorpe 1998; Yarrow 2003:69). This reasoning is, in actual fact, a form of trialectic (See Lefebvre 1940, 1980, 1991, Lefebvre et al. 1996).

The entities we deal with as excavators have physical form, area, volume and often mass, in addition we conceive their temporality which we conceptualise in a stratigraphic sequence (thinking stratigraphy) there is also for the excavator the experiential quality of stratigraphic entities as experienced physically, emotionally and intellectually while digging. The reasoning skills brought to bear on these entities are not based on thesis-antithesis-synthesis, nor affirmation-negation-negation, they

are ones in which, intuitively, *dialectical materialist thought is brought to bear* on objects or attributes of observed phenomena which seem to contradict one another, in effect “bringing together the conflictual and contradictory, linking theory and practice” (Lefebvre et al. 1996:10). Hodder (1999:84) also terms this a hermeneutic triangle. Examples of this process of thinking and reading stratigraphy, that is reconciling and regenerating theory with/through the observed and the experienced within the framework of a canon of axioms reflecting that which is actually possible, has been illustrated elsewhere with recourse to simple (Hodder 1999:40–42) and complex (Thorpe 1998) examples.

Though we create archaeological data via our own interplay with the relative percentages of sand, silt and clay that lay at our fingertips this data creation and data gathering is undertaken within the framework of a series of rational axioms. No one who lives in a material world can seriously and rationally question the underlying principles of either geological or archaeological superposition. Thus it is axiomatic that, for example, the *deposition* date of the pottery from the infill of a pit which has demonstrably been cut through a pre-existing context is temporally later than the deposition date of the material within the context that has been cut. This is not a matter of equivocation and it cuts across culturally specific approaches (whether accepted or not), this axiom establishes the framework by which we can interpret complex physical phenomena, it allows us to explain instances where pottery is earlier, the same date or later through recourse to attempting to understand the processes of formation of physical entities. Indeed, so central is this axiom to the conduct of stratigraphic archaeology that it forms the mechanism through which we seriate (Carver 1983; Crummy and Terry 1979) our ceramic sequences.

We also have an implicit framework for the recognition and labelling of phenomena. We call a certain type of hole in the ground a pit because, as archaeological practitioners, we have an agreed index of attributes that qualify a hole in the ground to be recorded as a pit, as opposed to a ditch or a foundation trench. We think we *know*, or that we *know how to tell*, where the edges of said pit are via our observations of relative differences in colour/texture/constituents and coarse components between infill and that through which it is cut. Routinely, as excavators, we question the evidence of our senses, we rework our assumptions, we deploy our own contextually tailored versions of Cartesian (Descartes) (1984 [1641]:12) systematic doubt, doubt which we exercise in those fractions of a second between, and during, the movement of the hand and the impressions of the senses. We modify our individual strategies as we try to reconcile often conflicting impressions of what we think, what we think we see, what we think we feel and what we experience during the process of excavating. This practical knowledge, which shapes the archaeologist’s approach to her/his raw material, can be said to constitute part of the archaeological *habitus* and the acquisition of these practical, interpretative and reasoning skills forms a crucial part of an archaeological apprenticeship.

It is as an outcome of the recursive tensions between observation and interpretation that we decide on the use and exclusion of methodological practices. We exclude, for example, the routine use of soil micromorphological analysis on all deposits, on every site as impossible (justifying this with reference to cost based criteria). Instead

we restrict the technique to certain defined situations and conditions. We do not routinely extract and identify every single pollen grain, snail shell, or other microscopic element of the soil choosing to justify the circumstances of their application through a sampling strategy.

In the light of these observations the question of objectivity, in the philosophical sense, does not enter into the argument and this is the point that Hodder (1997:691–692; 1998:214) and Hassan (1997:1022) have overlooked or failed to comprehend in their exchange. When we debate the question of objectivity and subjectivity in archaeological recording what we are actually considering are the parameters and definitions of the agreed descriptions which we employ to represent and categorise the complex ‘real’ or experiential world of observable phenomena in the material world. It is here that the debate takes place.

## **The Praxis of Record: Recording, Hierarchies of Inference and Influence and Relations of Power**

...the principles of archaeological stratigraphy are simple. They do not require that the excavator be a genius or even a university graduate in order to do a good job of interpreting and recording stratification.

(Harris 1989:53)

Archaeology is not difficult. Anyone who tells you that it is dishonest.

(Max Adams 1990, personal communication)

So when I awoke from the depths of space, I looked up and saw a familiar face, the time warp in space had made a change in me, for I was the Captain and the Captain was me.

(Spizzenergi 1979)

In this section I intend to examine whether the history of the development of archaeological methodology in Britain supports the contention implicit in the TAG 2007 session abstract (in which this paper was originally given) that, before the emergence of a specifically post-processual field practice, the tradition of excavation and recording in the UK was

perceived as a process of objectively recording the nature and extent of archaeological layers and deposits unquestioningly accepted as a means of data collection, where the material uncovered and the record produced by the individual excavator is seen as impartial and a-theoretical (Harris et al. 2007 TAG session abstract)?

Or that excavators were expected to act as

meticulous (but unthinking) collectors of data, leaving interpretation to their supervisors and managers after excavation has been completed (Carter 2001)?

As an initial observation the contention itself is flawed because it implies current field practice is the continuation of a single tradition. It implicitly assumes that field craft, the procedures and processes of excavating and recording, is born from and

manifest in a single recording practice. Modern recording practices in the UK, while based on the principles of stratigraphic excavation, do not stem from one single tradition but are the result of a negotiation of tensions within field practice and between existing traditions. Is excavation *perceived as a process of objectively recording by meticulous (but unthinking) collectors of data?* Is this the perception of those doing the excavation and recording? The answers are contingent on to whom you address the question, where they have worked, how old they are and which decade you are asking the question about.

Hammer (2000:143–144) describes three recording traditions in British archaeology; the feature-group, the CEU<sup>1</sup> group and the DUA<sup>2</sup> group. The recording systems used today in the UK are derived from one, or an amalgam, of these traditions and these traditions are, broadly speaking, defined by the stage in the archaeological process at which they integrate and embed higher levels of abstraction and higher order interpretations within the framework of the “primary”, *site*, record.<sup>3</sup>

On the surface the most interpretive of these traditions is the feature system, or what Hammer calls the “feature-group” (ibid). This system, which in reality includes the CEU-group, involves the grouping of numbered units of stratigraphy, for example the foundation trench of a drain, its stone base, its walls, its roof and subsequent fills, under an overall feature number. The system works largely for cut features or built structures and brings together related contexts into closely related physical events building up to landscape units. Traditionally a single number, a feature (or F) number, would act as the reference “handle” for an entity, such as a ditch and the feature sheet would serve as the forum through which higher levels of interpretation could be articulated. In its developed form context numbers are also allotted to separate stratigraphic units (see Connor and Buckley 1999 as an example) but these numbers are subsumed, on site, into feature numbers. The example, on the left in Fig. 3.1 shows a feature sheet as used by one archaeological unit in the UK in 1990. It is (or was in 1990) an on-site recording form dedicated almost entirely to interpretation and sequence and it was expected to be filled out by the excavator during excavation. The “CEU-group” (Hammer 2000:143–144) essentially recorded in the same way but added codification to its recording fields to facilitate computer input, storage and dissemination. The example, on the right in Fig. 3.2 dates from 1977. Notice in both examples the use of a field “category” which required a low level interpretive description of what was being recorded such as ditch, pit, floor, foundation trench, an interpretation of the entity being excavated was required at the very beginning of the recording process.

This system also required that where intelligible structures were encountered each individual element was directly and overtly related to the structure of which it formed a part, creating hierarchies of interpretive association, thus an oven, (191) could consist of (*contain*) a stoking pit (190), itself containing construction cut (163), construction trample (162), backfill of the construction cut (182), stone lining (166), and stone floor (161). This would be related to other elements of the oven such as its flue (192) which itself would *contain* flue walls (168, 322), a flue base (169), and so on. These lower order interpretations, placed within quite convoluted processes of creating hierarchies of interpretive association, were recorded by the excavator on site.

ACC No.	AREA No.	FEATURE No.	
Category		Grid Ref.	
Length	Width	Dia	Ht./Dp.
CONTEXT NUMBERS:			
Cut Number:			
F	Same as		
E	Below		
A	Above		
T	Cuts		
U	Cut by		
R	Uncertain		
E	Abutts		
	Butted by		
R	Bonded to		
E	Part of		
L	Contains		
CHECK LIST: Tick when task COMPLETED			
PLAN: pre exc.	exc	SECTION	
PHOTO: mono	colour	Section on PLAN	
SAMPLE taken		LEVEL	
Feature MATRIX		On COMPUTER	
DESCRIPTION/INTERPRETATION (Continued overleaf)			
DUG BY	NOTES BY	DATE	

DOE CENTRAL UNIT				CONTEXT RECORD			
01 GRID REF	02 SITE SUB DIV	03 SITE CODE	04 CONTEXT NO				
05 CATEGORY							
06 LENGTH		07 WIDTH	08 DIAMETER	09 HEIGHT/DEPTH			
10 MATRIX COLOUR							
11 MATRIX TEXTURE							
12 MATRIX CONSISTENCE							
13 COARSE COMPONENTS							
14 MATRIX							
15 CONSTITUENTS							
16 DESCRIPTION							
17 PART OF							
18 CONTAINS							
19 SAME AS							
20 BELOW							
21 ABOVE							
22 CUTS							
23 CUT BY							
24 UNCERTAIN							
25 BUTTS							
26 BUTTED BY							
27 BONDED TO							
28 INTERPRETIVE COMMENTS							
29 PLAN NO		30 SECTION NO		PHOTO (M)		PHOTO (C)	
SUPER		EXCAV		PASSED			
FLOTATION		SAMPLES		CONTINUATION SHEET			

Fig. 3.1 Feature/CEU Group recording sheets

UNIT SHEET	TYPE	SITE	P.P.N.	UNIT
	Description			
	Grid Refs			
	Stratigraphically Earlier Than			
	Later Than			
	Physical Relationships			
	Plans			
	Matrix			
	Photographs			
	Levels			
ARCHAEOLOGY	Method of Exc.			
	Finds present but not collected			
	Risk of Contamination			
	Samples			
	Special Finds			
	Provisional Date			
	Interpretation			
	Continuation Sheet			
	Signature			
	Date			

AUB/ACRE		CONTEXT RECORDING SHEET	
GRID SQUARE	AREA	SITE CODE: BEY 045	CONTEXT
		TYPE: Deposit / Cut	
Context Type:			
DEPOSIT			
1. Compaction		CUT	
2. Colour		3. Shape in plan	
3. Composition		4. Corners	
4. Fracture size over 10%		5. Dimension/Depth	
5. Inclusions		6. Break of slope - top	
6. Particle size under 10%		7. Sides	
7. Thickness & Extent		8. Break of slope - base	
8. Other comments		9. Base	
9. Method		10. Orientation	
		11. Inclination of axis	
		12. Truncated (if known)	
		13. I.L.F.H. No.	
		14. Other Comments	
		15. Draw Profile Overleaf	
Risk of contamination: High Low None Unknown PTO			
Clarity of Horizon			
Stratigraphic Matrix			
Your Interpretation:			
Your Discussion:			
Context same as: PTO			
Weather	Overcast	Rain	Fine
Plan: None P	(X)	Other Drawings: S/E	Hot
Matrix Location:		Photos:	
Levels: No.	Highest:	Lowest:	Initials:
Bone CBM Coins Glass Metal Pot Stone Tess None Other:		Checked:	
Environmental Sample No:			
Finds Seiving: Dry		Wet	
Checked Interpretation:			
Provisional Period: Group: Date: Name & Date			

Fig. 3.2 SCP/DUA-group recording sheets

The interpretive responsibilities of the excavator however did not stop there, the feature/CEU approach facilitated, in principle, the incorporation of middle and higher order interpretation by the excavator into the primary record, on site, within an extended hierarchical structure of inferential links moving from the context, to the feature, to the structural unit, to the landscape unit. Thus, for example, a foundation trench would be recorded as part of a wall, the wall part of the defining elements of a room, the room part of a building, the building part of a land use block. These higher levels of interpretation could and would encompass overview and higher theory and were provided for within this recording tradition with no institutional block on the excavator and recorder to give free reign to their interpretive schemes at the point of recording. All this pre-empting the flagship projects of post-processual archaeology (see Andrews et al. 2000) by some 20 years.

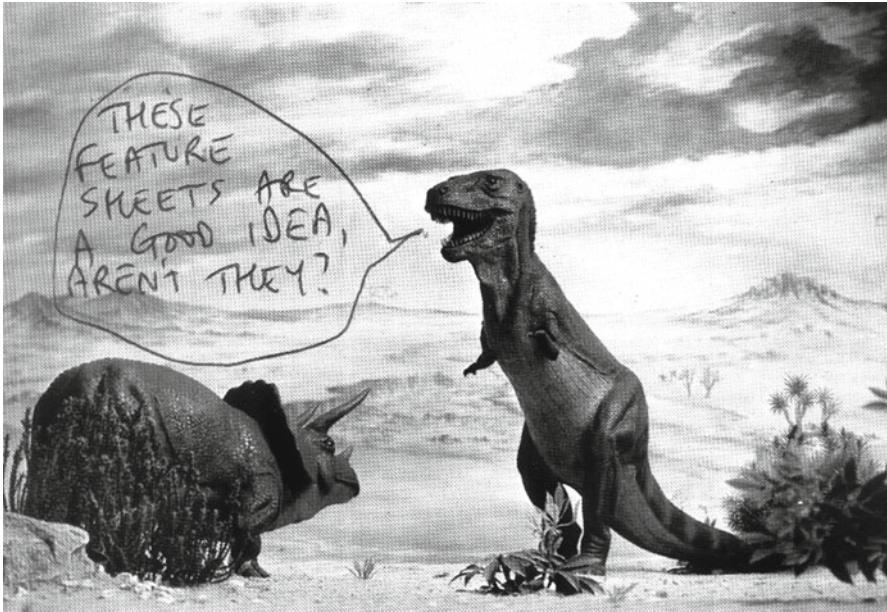
The third (or second) type of recording tradition, single context planning/recording (SCP), or the DUA-group (Hammer 2000:144), was devised specifically for urban excavation. These sites, typified by deep stratification often extensively truncated into islands of complex stratigraphy, are less readily open to higher order interpretations at the immediate point of excavation. This system, first implemented as policy by the Department of Urban Archaeology of the Museum Of London in 1977, was first used in embryonic form, to the authors knowledge, in 1975 (Harris and Ottaway 1976). Figure 3.2 shows a context sheet of this recording tradition taken from the first ever SCP site manual (Boddington 1978:28; Fig. 9). The record sheet on the right of Fig. 3.2 is derived from the deposit/cut recording sheet of the museum of London though this example comes from the Anglo-Lebanese rescue excavations in Beirut of the mid 1990s.<sup>4</sup>

As the caption to Fig. 3.3 intimates, the form of engagement with the process of interpretation and developing higher order and theoretically informed interpretations can to some degree be dependent on the disposition of the site being excavated. It is more difficult to develop, as an individual excavator, a detailed overview of site wide sequence if excavating in islands of physically disconnected stratigraphy. It is different if one visibly has features extending across an excavated sample of a landscape that have relationships only with other features or the natural (see for example Alexander and Armit 1993).

The seemingly more interpretive archaeology of the feature/CEU-group tradition often encounters problems of validation. There are problems with validating higher order interpretations made in the field, especially on stratigraphically complex sites, if the system of creating the drawn record is not based on the creation and use of plan as a stratigraphic document (Harris 1975:110–112). Multi-context, phase, planning is problematic on complex sites because the drawing itself, as artefact, is difficult to interpret as a stratigraphic document. It can be argued that the act of planning what is about to be removed, by the excavator who uncovered it, in advance of its removal, facilitates a greater depth of interpretation and engagement on the part of the excavator (see Bateman 2006:77). What single context planning did bring however was a tool which enabled higher order interpretations to be checked and validated. There is no necessity, philosophically speaking, for there to be a correlate between the demands of rigour of process and a reduction in the requirement for interpretation at the trowels edge by the excavator.







**Fig. 3.4** The feature/CEU group and multi-context planning – a users view circa 1990 (With thanks to Luke Fagan)

Single context planning and the “reductionist simplicity” (Chadwick 1998) of the matrix requires that it is the stratigraphic chains of related associations, recorded by direct stratigraphic relationship and interpreted by processual association, that provide the basis for expanding on site narratives in which the excavator has interpretive range but with recourse to demonstrable and rigorously recorded stratigraphic relationships. Figure 3.4 is a postcard I was sent by a colleague in 1990. It conveys the zeitgeist on one deeply stratified site concerning the application of what, on the face of it, was the more interpretive feature system (albeit with modifications) and aptly outlines the reduced potential conceptually of feature based recording systems and arbitrary phase planning to articulate and formulate complex interpretations of complex sites.

The underlying point however is that within these two traditions, emergent since the late 1960s, the requirement was placed fairly and squarely on the excavator to interpret that which they dug, to keep interpreting it and to make that interpretation explicit. This is the methodological history of the development of the *habitus* of the British field archaeologist (*contra* Berggren and Hodder 2003:421–425; Hodder 1997:691).

Professionalisation in archaeology in the UK in the 1970s and 1980s was as a result of the demand for an increasingly organised, full time, response to the threat to the archaeological resource through development “every endeavour should be made to retrieve the maximum historical data for money spent” (Boddington 1978: forward). Increased efficiency in excavating did not diminish the potential or opportunity for interpretive input at the highest level. Without exception the units

established in the 1960s and 1970s (Jones 1984:6–8) were bound by a tradition of rescue excavation as research, “rescue archaeology is research” (Lambrick 1991:24). Nor were these units naively established to save the nation’s archaeological heritage through dispassionate objective observation. Early recording manuals acknowledged the impossibility of ‘objective’ recording. They accepted that the excavator would take to the trench her/his own set of biases, borne as much out of differing levels of training, experience and competence. This was even seen as a strength in the excavation process and one which should be, had to be, explicitly recorded, “it is.... necessary to make our biases explicit....If they are not....we will not know why we are emphasising certain kinds of data.” (Hill 1972:72).

British Archaeology or large parts of it accepted that, ‘there is more to seeing than meets the eyeball ... more to observation than merely standing with sense organs at the ready ... it is all interest-directed and context related and context dependent’ (Hirst 1976:13). The appropriate methodological response *was* to explicitly acknowledge the interplay between interpretation and observation and to accommodate and incorporate interpretation at the trowel’s edge within the primary record, but to separate it out as far as practicable in an attempt to minimise bias, in an attempt to afforded equal weight to all archaeological entities, in an attempt to “avoid confusion of often important detail” (Boddington 1978:3). This was facilitated by providing a structured yet flexible framework (context/feature/group sheet) in which it could be articulated (Jefferies 1977:3, 5–6) and was in contrast to the more conventionally liberated yet ‘inconsistent, incomplete and (of) uneven quality’ (Hirst 1976:4) of recording in site notebooks, often by the trench supervisor alone.

Recent work (Cumberpatch and Thorpe 2002; Thorpe and Cumberpatch 2005) on the archive from the 1970s excavations in Chesterfield, Derbyshire (see also Ellis 1989) shows that the archives from the North Derbyshire Archaeological Trust, one of the first units to be established in the UK, allow re-interpretation precisely because the excavators (who completed their own recording sheets) were a vocal part of the dynamic of site interpretation. Re-assessing the extant archives it soon became clear that not only did these excavators document the entities they were digging, they documented the reasons why they were interpreting entities in a certain way, they articulated any ambiguities in the observed evidence and they interpreted/related what they had excavated within an evolving and debated site wide interpretation. What is more, they also recorded their own metadata (data about data) statements relating to interpretational shift, the evolution of the interpretation and were also intimately involved in the post excavation work. This was reflexive method 70s style, in the context of Rescue archaeology, paid for by the Town Council and the National Coal Board, in a small but globally significant industrial town informed by emerging (see DOE 1975:3) archaeological practices at the time (Fig. 3.5).

That there was an emergent tradition of reflexive excavation and recording seemingly from within a processualist paradigm, and that this tradition was facilitated by the adoption of more professional organisational patterns of labour, I think has been established. The empowerment of the individual excavator afforded by the adoption of the context sheet extended from the recording and interpretation of individual units of stratigraphy through to groupings of contexts, and the recording



**Fig. 3.5** Reflexive excavation 1975 style (Station Road, Chesterfield, Derbyshire. Courtesy of Chesterfield Museum Service)

and interpretation, by the person with the trowel in their hand, of structures and feature complexes (see Jefferies 1977:21, Fig. 2, fields 17 and 18). This production of higher order groupings on site by the excavator (see Central Archaeology Service 1992:15, fields 49–50 for example; Central Excavation Unit 1985; Milton Keynes Archaeology Unit 1989:10) hardly reflects “the excavator as non-thinking shovel” (Berggren and Hodder 2003:424).

It can be seen then that, for a time, the development of the excavation and recording process was one (say, in Britain, from 1973 to 1992) designed to add interpretative power to the arm of the excavator because this helped address the purpose of rescue excavation. The development of recording systems to do this however was entirely outside what have been held up (Hodder 1997:291–292) as canonical texts of archaeological methodology. Joukowsky’s *A Complete Manual of Field Archaeology* (1980) has never been illustrative of field practice in Britain. Barker’s *Techniques of Archaeological Excavation* (1993) first published in 1977, was influential as a student text book and was a reflection of his own ideas about excavation methodology (it is significant that he altered the title from *The Techniques*). His proselytisation of open area excavation and excavating in plan (Barker 1969), of using a combination of plan and section as arbiters of sequence and the requirement for structured and detailed recording was effective when combined in one volume, but they were ideas that had been or were, variously, in use in other areas of British and European field archaeology at the time. Importantly though, at the very point in time that Barker’s first edition was being published, the dynamic in methodological development had moved away from university departments to emergent field archaeology units. The resulting field and procedures manuals, notably Hirst (1976) Jefferies (1977)

and Boddington (1978) provided the subsequent texts for professional field practice in the UK, not Alexander (1970) not Barker (1993) and certainly not Joukowsky (1980), nor the many text books on excavation that followed until relatively recently.<sup>5</sup>

Rather than being bound by an authoritative canon of texts, archaeology is characterised by networks of personal interactions (see also Carmen 2006 at site level) and connections through which approaches and experiences are disseminated. The itinerant lifestyle of workers in archaeology in Britain is a process of flow and has been since the advent of a 'circuit' in the 1970s. This circuit once encompassed a broad church of people, of differing social backgrounds (from Cambridge graduates to those who began their archaeology on training schemes for the unemployed) all of whom brought differing social and excavation experiences to their work. Archaeological methodology was talked, debated, contested in the field, manuals were re-drafted to take into consideration the observations and objections of the people working to them. In this instance then, the development of procedures manuals and site manuals was as a consequence of experience in the field, a reflexive process that encapsulated negotiated practice. These manuals may have been written by the supervisors but they were negotiated, mediated through applicability and fitness for purpose and were periodically re-drafted and re-crafted by those who had used them as diggers.

To summarise; the role of the excavator in British Archaeology from the early 1970s to the advent of competitive tendering in the early 1990s was usually far from that of a low skilled labourer with no input into the process of site interpretation, or the construction and creation of higher order site narratives. It was not even the professionalisation of archaeology, nor the rise of developer funded archaeology, that has led to a removal of the excavator from the process of interpretation, or a decrease in the role of on-site training to novice excavators. South Yorkshire Archaeological Field and Research Unit, Milton Keynes Archaeological Unit, Leicestershire Archaeological Unit, to name a few (though now all sadly defunct), provided paid trainee places on commercially funded excavations. The adoption of the 'polluter pays' principle and developer project funding, in the late 1980s, did not undermine the excavator's engagement with the process of interpretation.

In 1990, the Tattenhoe and Westbury excavation projects (Ivens 1995) in Milton Keynes were developer funded rescue excavations in advance of housing development. Both provided exceptional levels of training, both required excavators to be engaged in the interpretative process at context and higher order level. Both held weekly site meetings where individual excavators would provide a tour of the areas they were responsible for excavating to the whole team and discuss their interpretations. Each excavator was positively encouraged to look around at adjacent areas, talk to excavators of adjacent areas and develop, engage in and debate site/area interpretation. While exceptional, this form of collaborative ethos was not unique within projects and stemmed from a tradition in British archaeology, summed up by Carver (2006:6), where "[w]e sang or chattered continually [...] Our best shared conversations were about the site: we talked at breaks, we talked in pubs, we did site tours together ... we talked, as we dug, about what we were digging. We were professional, but did not assume that any expert knew more than we did about what we were turning up: this was shared discovery".

Finally in this section I would like to examine the extent to which the relationship of the hierarchies within organisations and between people might be responsible for the feelings of alienation within contemporary archaeology. It is perhaps to be expected, as a hangover of the social control that was/is the class based society of Britain, that hierarchical systems of formal management are the norm within archaeology. Collis (2001: Fig. 2.14, 44–45), traces the historical development of site management strategies through four main stages all of which have a director (project manager) at the top with supervisors, area supervisors, specialists, excavators, etc. in relationships to one another. He makes the point that “[o]pen area excavation(s) [...] are ideal for communication” (2001:164) embedded in which is the fact that just because the organisation of people working on site, or in a project is hierarchical, the free flow of information does not have to be conditional on that hierarchy and indeed informed decisions on strategies and interpretation can be taken collaboratively at an area level. In an ideal world the key is discussion, inclusion, on the job training and open fora for negotiating fieldwork practice in response to changing conditions. In my own experience in the 1980s and 1990s this happened, even in commercial archaeology. There was a meritocracy of sorts, ideas were passed around, informal training was given via an implicit buddying/mentoring system. You learnt what to do from the people nearest to you, you argued about process and interpretation in detail. Skills were encouraged to develop, were nurtured and developing skill levels were rewarded with greater responsibility, much like an apprenticeship. Sadly, from my own experience as a manager in commercial archaeology in the twenty-first century, this is no longer the case.

## A Prognosis

If I have shown that the diagnosis of the causes of the malaise of alienation (and not just alienation) within British archaeology is specious, and that prior to the development of a post-processual field method,

- Excavators *were* expected to undertake and give training on site,
- Excavators *were* encouraged (informally) to be methodologically informed participants in a collective interpretative process of excavating,
- It was *within* local authority archaeological units on the sites they excavated that fieldwork practice was modified, negotiated, implemented;

Then it would seem that there is already a strong tradition of interpretation at the trowels’ edge within British archaeology. Whether this tradition is disappearing, or is in the process of disappearing, is a different matter.

The practice of field archaeology in Britain is largely situated within the context of a commercial environment where contractors are selected by clients on the basis of competing on cost. I would argue (as does Lucas 2001:12) that there is as much mileage in the argument that it is this context, or more particularly our responses to it (see also Everill 2007:132–133 and Heaton 2007, 2008) that has diminished the

role of the individual excavator in the process of interpretation, and impoverished British Archaeology rather than the absence of a specifically post-processual *reflexive method*.

Archaeologists in the UK recognise that the past is a fragile and non renewable resource. We welcome that mostly our work is undertaken within the sphere of development control as an essential part of the planning process. We realise that excavation is not an “unrepeatable experiment” (Chadwick 2010:11) and that it retrieves samples of potential evidence which may address or re-cast research agendas [where these exist and are actually used]. It is only right that our work is obliged to be framed within the context of research priorities, and it is *just* that we finance this research via developer funding. However, the mechanism by which contractors/researchers are selected is via compulsory competitive tendering. The usual basis on which a contractor is selected to do the research is usually cost alone, perhaps more explicitly -who will do it for the least amount of money? Unequivocally, value for money is important. There is a difference though between best value and cost effectiveness.<sup>6</sup> Simply, the concern of the developer is not what Carver (2006:8) termed the research dividend, it is removing the archaeology as quickly and as cheaply as possible (Heaton 2007:15). Clearly, there are divergent narratives about purpose and, importantly, for whom archaeology is undertaken (McGuire 2007:10 and compare Andrews et al. 2000:526, Carver 1989:666, 672, Aitchison 2007:122, Tarlow and Pluciennik 2007:125).

In 1990 the CBA<sup>7</sup> was prophetic in its statement “competitive tendering may on occasion result in a minimalist approach to research and could exacerbate existing tendencies towards inadequately designed field programmes” (Lambrick 1991:23). This certainly appears to be the case to a whole raft of freelance specialists commissioned to provide dates and catalogues as part of a process, a “sausage machine” (Cumberpatch and Blinkhorn 1998), which sees them denied aids to understanding site sequence, spatiality or ideas of stratigraphic groupings (Cumberpatch and Roberts 2012) (things have moved on since this was written in 2007/10) and to diggers who are factored into projects as being able to excavate between 0.75 and 1 m<sup>3</sup> of soil per day, every day, and who typically cost less to employ (£14,500<sup>8</sup>) per year than an unskilled manual labourer on a building site (£15,000–20,000<sup>9</sup>), a bin man in Basildon (£19,610<sup>10</sup>), or a litter picker in Atherstone, Warwickshire (£15,725–16,830<sup>11</sup>). The quality of our excavations are conditioned almost entirely by the market and that market is weighted against archaeology as research.

To my mind what is problematic are two things. Firstly the way our profession almost wholeheartedly and uncritically accepted a vision, promulgated in the 1980s, which hand in hand with the social ills of mass unemployment, cuts in public spending, cuts in education and reductions in access to free healthcare, was part of a spectacularly divisive, partisan and ideologically driven vision for Britain. This vision privileged commercial interest, refused to recognise the collective good of society, and asserted the free market and its mechanism of commercial competition as the best, indeed only acceptable, mechanism to govern the relationship between customer and service provider in archaeology. Secondly, and here I agree with Heaton (2006, 2007, 2008), having opted into a system of lowest cost wins tendering we have not been as good

as we could have been at using that system to further our collective interests and better the material and research conditions of archaeology and archaeologists. We have remained pegged to public sector salary scales while moving away from public sector service and in competing with one another, in the way that we do, are we actually driving salaries and standards down?

Developer funding, underpinned by the polluter pays principle, had been on the agenda in English Archaeology since 1969, at which time it was eschewed in favour of state funding being more in the public benefit (Walsh 1969:144). Developer funded projects however were undertaken by local authority units before 21st November 1990 and the introduction of PPG 16.<sup>12</sup> Importantly though, unlike other European countries (Collis 2001:166, Carver 2006:9) we in Britain signed up to, or were signed up to, the ideological hostility that the government of the time had against a “system where local government units could instruct developers on what work needed to be done” (Collis 2001:166). Of course a cogent case for splitting the curatorial role of local authorities from their role as service providers was put forwards and the IFA<sup>13</sup> “played a decisive and pivotal role in this issue” (Wainwright 2000:929). This case was largely built around issues of potential conflict of interest, transparency, consumer trust, consumer choice, but the merits of such arguments were all too readily and incautiously accepted in the UK, unlike in France, Germany and Sweden, and required a redefinition of consumer/client. Alternatives could have been sought, a middle way could have been found. Undoubtedly more archaeology is being done in the UK, than ever before and there are more people employed in archaeology than in 1990. Given the wider number of contractors and the widening of consumer choice it appears, to one senior commentator at least, that it has taken us a short time to achieve a position, where

Standards in the field are the worst they have been for 50 years, and we are not shy about showing the whole world our lightweight theories, shoddy trenches, limp strings and messy layers on television. Most telling of all, professional excavations have fallen silent; there is no more singing – probably a good thing in some cases; but also no more talking, no more thinking aloud.

(Carver 2006:8)

The CBA identified in 1990 that “developer funding and competitive tendering are not automatic bedfellows” (Lambrick 1991:22) and both the CBA (ibid:21) and RESCUE<sup>14</sup> (Sheldon 1991:6) intimated that the mechanism of competitive tendering was being foisted onto archaeology by English Heritage, a quasi-autonomous part of what was then the Historic Buildings and Monuments Commission.

Professional archaeology in the UK is entirely dependent on one ideologically driven mechanism of service provision. We engage in a discourse which views archaeology as a problem to be overcome and we sell our selves using language accordingly. We portray ourselves as fixers, not as innovative professionals working to “balance the demands of the market upon the historic environment” (Aitchison 2008:388). This historic environment is, metaphorically, a common treasury, it belongs to the communities of which we are a part. Our privileged place, as archaeologists with the skills to investigate the past and engage with its materiality, give us



a unique position as stewards, sometime guardians, of the research potential of that resource (contra Aitchison 2007:122). We undertake research within a commercial environment that is situated within a framework designed to ensure clients fulfil their legal and ethical responsibilities, but on balance does the way we have situated ourselves within the commercial environment work in the favour of balance at all?

I believe our mistake is to have allowed ourselves to be incorporated into an economic mode of production where the value of our product is measured almost entirely by financial cost to the developer. We have, no matter how much we as individuals and as organisations care about archaeology and care about it being done well, dug ourselves into a hole where

Specialists, suffer more indignity than even Diggers: Invariably highly qualified and knowledgeable, they are at the bottom of the food chain, making do with left-overs from over-run site budgets and having to justify every penny spent – but to other archaeologists, not to the clients.

(Heaton 2008)

We might be digging more sites, but are we digging them technically as well as once we did? If digging sites as Shanks and McGuire (1996) argue, is a craft akin to, say, furniture making, or painting and decorating, are we now making flat pack beds and off the rack interiors rather than bespoke, circumstance tailored, context sensitive, creations?

Where advocacy of post-processualist method is especially strong is its call for more democratic field practice. Democratising field practice is about radically rethinking and re-constituting project teams, including diggers and specialists as stakeholders within research, engaging them within the realities of research, monitoring, budgets, timescales, best value and quality. One could try a bottom up framework, flatter but greatly improved pay structures, flatter hierarchies, devolution of responsibilities for training onto all that constitute a project team, sharing strategic decision making and taking joint responsibility for those decisions and the achievement of goals. This will take an organisational and cultural shift which, unless implemented at root and branch is unlikely to sit well with managers who have to keep consultants, developers, their own line managers, Historic Environment Officers and unit accountants happy, all of whom have hierarchical relationships of power within any project. It is, then, crucially within the attitude of commercial archaeology as a whole, to the practice of archaeology, that we have to change things, and in the professional bodies and the non-governmental organisations that represent us.

We have to stop pretending that what is wrong with archaeology in Britain is just wages, just conditions, just alienation from the process of interpretation but instead see these things as symptoms of a wider malaise. Post-processual approaches to excavation and approaches to stratigraphy (see McAnany and Hodder 2009 and commentary, particularly Berggren 2009; Helwing 2009) are no more empowering and no more prioritise interpretation over description than the approach to excavating the environs of Milton Keynes in the late 1980s by the MKAU or Chesterfield by NDAT in the 1970s.

Post-processualism has failed to further the practice of excavation and interpretation of stratigraphy because in making its initial case (Hodder 1997, 1998, 1999), against straw people, it has either ignored or created a jaundiced historiography of the development of British archaeological fieldwork and has re-cast practices which were not uncommon 25 or 30 years ago as new, specifically post-processual, developments. Importantly, in deferring from contextualising the practice of fieldwork today within consumer capitalism it has critically failed to engage in the core debate on the effect that the current commercial context of archaeology has had on training, standards and the development of interpretative skills. In doing all of this it has failed to offer real alternatives to established practice, or the context of that practice, and renders itself sadly - purely in terms of the process of excavation - effectively and practically irrelevant.

## Notes

1. Central Excavation Unit. The English state field unit, part of the Department of the Environment until the creation of the QUANGO English Heritage in the 1980s, rebranded the Central Archaeology Service (CAS) in 1991/1992 and the Centre for Field Archaeology (CfA) in 1999.
2. Department of Urban Archaeology. The Field Unit of the Museum of London, rebranded MOLAS, Museum of London Archaeological Service, around 1990.
3. As defined in The Cunliffe Report (DOE 1983).
4. The SCP approach to recording on excavations now underpins the methodological approach to excavation by the Direction General of Antiquities in Lebanon and Beirut Museum.
5. There is some indication however of a counter shift under way as the influence of archaeologists who having worked in archaeological units in the 80s and 90s primarily involved in fieldwork are employed in universities to lecture and teach is being felt in the regeneration of a theory of methodology.
6. Program Evaluation Glossary. United States Environmental Protection Agency <http://www.epa.gov/evaluate/glossary/c-esd.htm> accessed July 1st 2010.
7. Council For British Archaeology.
8. Institute for Archaeologists minima salary level in 2009 for archaeological practitioner who will usually have a degree.
9. Advertised salary rate on Jobs 4 You careers database <http://www.connexions-direct.com/jobs4U/>. Accessed may 16th 2009.
10. <http://www.jobisjob.co.uk/basildon-district-council/job-offers>. Accessed July 1st 2010.
11. <http://www.jobisjob.co.uk/north-warwickshire-borough-council/job-offers>. Accessed July 1st 2010.
12. Planning Policy Guidance Note 16 "Archaeology and Planning" Guidance note to planning authorities in England, property owners, developers, archaeologists, amenity societies and the general public on the state policy on archaeological remains on land. It was replaced by Planning Policy Statement 5 on 23rd March 2010.

13. Institute of Field Archaeologists, now the Institute for Archaeologists, a professional association for all who work in the Historic Environment field within the UK with some 2,400 corporate members in 2010 and some 900 in 1990.
14. RESCUE- The British Archaeological Trust.

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## Chapter 4

# Where the Rubber Hits the Road: A Critical Analysis of Archaeological Decision Making on Highways Projects in Ireland

Brendon Wilkins

There was once a time when archaeologists could rebut all criticism of their professional judgement with the simple retort: ‘the spade never lies.’ Whilst history is written by winners, archaeological excavation reveals the past as it was, unsullied by the duplicitous meaning of words. At least that was the holding line, until Philip Greigson pointed out that even if ‘the spade cannot lie, it owes this merit in part to the fact that it cannot speak.’ The essential materiality of the archaeological record (and its resistance to subjectivity) belies the fact that everything we find requires interpretation (Taylor 2001:491). This is problematic; when even the most basic of archaeological observations are heavily theorized, our methods of inference are particularly susceptible to contemporary bias. So what kinds of methods do we allow, and what is the social context of our decisions?

In January 2006, Richard Bradley presented an influential paper to a meeting of the Society of Antiquaries in London called ‘*Bridging The Two Cultures – Commercial Archaeology And The Study of Prehistoric Britain*’. He argued that in both Britain and Ireland there are two different cultures of archaeology: academic, committed to research and the pursuit of knowledge, and commercial, devoted to the ‘preservation by record’ of archaeological remains threatened with destruction (Bradley 2006:1). Largely an accident of history, the consequence of what he calls a disciplinary ‘schism’ is that academic and commercial archaeology are undertaken by different people, paid for by different sponsors, and the results disseminated in different ways.

This raises an important issue, for if this volume is concerned with archaeological practice, and in particular, the nature and integrity of excavated data, it should begin by acknowledging that the vast majority of fieldwork that takes place in Britain and

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Ireland is commercially focused. This chapter compares British and Irish sampling strategies on infrastructure archaeology projects, focusing on the current debate regarding quantity and quality in the commercial sector.

## Background

There is a broad global consensus that the impact of change on archaeological remains must be controlled and managed, although the precise mechanisms can vary significantly between nation states. This rests on the principle that the historic environment is a finite, non-renewable resource that must be protected, managed and studied for the benefit of present and future generations. The ultimate loss of the material remains of the past must be balanced against their significance, and the ability of the practicing archaeologist to produce a measured, drawn and written record; appropriately conserved and archived finds; and a fully synthesized final report lodged with the relevant authorities. At a fundamental level, development-led archaeology can be organized according to either socialist or capitalist principles: projects can be delivered either as a public service or procured through a market of service suppliers (Demoule 2002:170; Thomas 2002:236). In keeping with the majority of Western Europe, the trend in Britain and Ireland since the 1990s has been towards a commercial basis for delivering what was previously organized exclusively as an academic discipline (Willelms and van den Dries 2007:1).

The market-based model became viable when planning regulations enforced an obligation to consider the potential impact of proposed development on archaeological remains with all costs met by the developer. The foundations of the commercial sector were consolidated by a revision of the European convention (the Valletta Convention) on the protection of the archaeological heritage by the Council of Europe (1992). Article 3, relating to ‘the authorization and supervision of excavation and other archaeological activities,’ makes clear recommendations as to how projects should be managed, whilst Article 6, relating to the financing of archaeological research and conservation, indicates that costs should be budgeted for by all developers – public and private. The Valletta Convention was ratified and implemented by most member countries, and was followed in 1997 by European Union legislation on environmental impact assessment (council directive 97/11/EC) that included archaeology (Willelms and van den Dries 2007:2). This is administered at a national level in Ireland through the National Monuments Act (1930–2004); PPS 5 in England; NNPG 18 in Scotland; PGW in Wales; and PPS 6 in Northern Ireland (Hunter and Ralston 2006:37).

## Celtic Tiger Archaeology

The Irish ‘Celtic Tiger’ boom resulted in a dramatic increase in the scale of construction projects, stimulating demand for commercial archaeological services. With an annual budget of €1.5bn, the road building program initiated some of the



largest infrastructural archaeology projects ever undertaken in Europe (O'Rourke 2005). A national development plan was implemented from 2000 to 2006, underpinned by generous European funding and attractive tax incentives to foreign investment, with ring-fenced resources for improving the country's inadequate infrastructure. The impact of this growth can be gauged by the increase in excavation licenses, and survey data of the number of archaeologists employed in the sector. The number of excavations reported in the annual *Excavations Bulletin* rose from 214 in 1993 to 2,044 in 2003 (Eogan 2008). In 2002 there were an estimated 650 professional archaeologists working in Ireland, 45% of which were in the commercial sector (CHL 2002b). These figures increased exponentially, with a high watermark reached in 2007, by which point the estimated number of archaeologists in Ireland had expanded by over 260% to 1,709, with 89% of this figure employed in the commercial sector (McDermott and la Piscopia 2008).

The commercial sector responded to these changes with significant developments in professional methods and technical processes (O'Rourke 2007:39), and the consequent discovery of new data has challenged accepted understanding of regional sequences in a way that was unthinkable before the boom (Eogan 2008). But despite the successful identification and excavation of a vast quantity of previously unknown archaeological sites, the system is currently under government review in Ireland with new legislation currently being drafted (DoEHLG 2007). Calls for change have come from within and without the discipline and focus on the question of 'fitness for purpose' and 'public benefit' – questions that were not considered relevant when excavation was only practiced as an academic discipline (see Waddell 2005:7, 2007:4). This follows a long-standing debate regarding the viability of archaeology in a market-based economy (Adams and Brooke 1995:93; also see Thorpe this volume), and uncertainty as to what exactly constitutes a quality archaeological product (Willems and van den Dries 2007:4).

In keeping with Bradley's 'schism' between the two cultures of archaeology, the commercial sector has been criticized by some academics for becoming a specialist sub-discipline, where the concept of an objective, quantifiable resource can be managed in the manner of a production line (Adams and Brooke 1995:93). In this analysis commercial archaeology is criticized as a service industry where interpretive decisions can be delayed to a later phase of the project (Barker 1982:147). The material uncovered by the excavation and the record produced by the individual excavator is seen as impartial and a-theoretical. Translated into the language of cultural resource management, the concept of 'the archaeological record' and the strategy of 'preservation by record' become equivalent. The process of excavation therefore ceases to be exploratory and inquisitive and instead becomes led by the service requirements of discharging a planning condition (Hodder 1999:170 and see Thorpe this volume, for a discussion of the social, political and economic context of commercial sector methodology).

Other commentators have focused on methodology, noting subtle differences between European states depending on how archaeologists conceptualize what lies in the ground and how those material remains relate to past human action (Carver 1989:669). In two neighboring localities, similarities may exist in terms of the

actual, physical remains of the past, but the ‘archaeological record’ can be ‘preserved by record’ in quite different ways depending on what side of a modern political border they are found. Rather than perpetuate the disciplinary schism with overt criticism of the commercial basis for development-led archaeology, the more pertinent issue is the question of value for money. Given that archaeological work is paid for in the public interest, what relationship does the quantity excavated have to the quality of the results?

## The Anglo-Irish Disagreement

In Britain, the norm in commercial circumstances is to sample excavate sites, normally at a rate of 10% of all linear features, 50% of discrete features and 100% of structures. Construction impact is controlled through planning guidance and a problem-orientated methodology of sample excavation is practiced to filter out the irrelevant (DoE 1990, 1999:17). In Ireland all archaeology is treated as potentially unique, requiring 100% excavation and preservation by record. A committed legal framework underwrites all decisions that may potentially impact on the archaeological heritage, and any proposed development must be preceded by full excavation of all sites and features (DoAHGI 1999:24). In essence, this is the Anglo-Irish disagreement: the presumption to ‘total archaeology’ on the one hand and ‘sample archaeology’ on the other. But digging larger quantities also entails larger costs, and if this work is undertaken in the public interest then it begs the provocative question of which approach is better value?

Arguably there are historical antecedents for both these methodologies that date back to the foundation of the discipline (Carver 1990:257; and see Carver this volume). General Pitt-Rivers outlined the problem that “excavators, as a rule, record only those things which appear important to them at the time. But fresh problems in archaeology and anthropology are constantly arising...” (Pitt Rivers 1887, quoted in Hodder 1999:22). He maintained that this could be overcome if “every detail should therefore be recorded in the manner most conducive to facility of reference...” (ibid.). In contrast, Flinders Petrie, proposed a discriminatory approach: “The old saying that a man finds what he is looking for in a subject is too true; or, if he has not enough insight to ensure finding what he looks for, it is at least sadly true that he does not find anything he does not look for” (Petrie 1904:49).

Recognizing the influence of the cost factor, the British model of ‘sample excavation’ (of deliberately digging less), could be seen as undermining the public interest in the face of commercial pressure. This makes the British sound like the poor relations, with excavations understaffed and under-resourced, not so much preservation by record as destruction in denial. However, it could be argued that this is actually a better way of working, a way of boxing clever; not working harder, but working smarter (Roskams 2001:35). From this position the Irish model of ‘total excavation’ is characterized as an indiscriminate and simplistic process of information gathering, disenfranchising the field archaeologist from the decision-making process

(Roskams 2001:31). The argument for the defense is that the National Monuments Act loves all her children equally, and it is precisely this indiscriminate approach that safeguards the archaeology from commercial pressure.

## Public Benefit

Recent concerns in the Irish media over expenditure on infrastructural archaeology have focused debate on the use of total excavation methodology, questioning the resulting public benefit from such large-scale excavations (O'Connell 2009). In November 2007, under duress from the controversy surrounding the construction of the M3 through the Tara environs, John Gormley, the Minister for Environment, Heritage and Local Government, invited submissions from all sectors of the heritage industry to debate the issues confronting Irish archaeology, and appointed an expert committee with international representation to steer the review and development of new legislation (DoEHLG 2007).

With little exception the response from both academic and commercial sectors has been united in agreement that 100% excavation of all sites and features should be practiced as a minimum quality standard (Swan 2007:40). If this is a holding line, then a concession has been made to the problem of 'public benefit', perceived as resulting from the failure of the commercial sector to convert technical survey and excavation reports into published and accessible information/knowledge (O'Sullivan 2003). Both the *Royal Irish Academy* (2006) and *The Heritage Council* (2007) have recently published detailed analyses of commercial issues in relation to 'Celtic Tiger' archaeology. The main problem is seen to stem from a lack of cohesion and inter-institutional collaboration between contracting archaeology companies undertaking fieldwork and university based archaeologists involved in research.

The success of any archaeological project must be judged primarily by the research questions/issues it sets out to answer and the knowledge it produces. With some exceptions, the current preoccupation of the development led archaeology is largely with data/information collection and management rather than the quest for knowledge. To address this situation, immediate priority must be given to the standardization of data collection/recording and to its interpretation by directors and other archaeologists involved in excavation projects.

(UCD 2006:35)

A result of this 'disconnectivity' is that development-led archaeology has been undertaken purely to facilitate infrastructural projects and the vast amount of sites excavated remain unpublished (UCD 2006:25). But in echoing the traditional view of archaeology as objectively recording the nature and extent of archaeological layers and deposits that can then be used to generate knowledge, the *Heritage Council* document reinforces this disconnectivity. The emphasis is placed on creating a quality product, without questioning the excavation strategies on which that rests.

## Rumsfeldian Archaeology

The quality/quantity debate has a resource and financial implication, but perhaps it should be reframed to ask how far different excavation strategies impose limits on the type of archaeology identified, and therefore narrow the potential interpretations of the available data. The commonsensical conclusion is that a larger sample will result in the identification of new and unexpected archaeology. This oversimplifies the problem, as there are two different types of sampling strategies employed in the commercial sector: informal and formal. Informal samples are purposive, and generated by deliberate choices based on archaeological criteria such as time, cost and convenience. Formal sampling strategies are selected from well-defined populations based on rigorous statistical procedures. This can be used to make valid statements about relevant populations, and assess this information with a degree of confidence. An appropriate approach is to combine both informal and formal sampling strategies, balancing statistical rigor with prior information. The precise percentage is a moot point, and when applied to development-led projects, will depend on whether archaeologists are involved in evaluation or excavation work.

Highways projects entail a phased program of works, from constraints study, route selection, environmental impact statement, testing and excavation. This structure has been called ‘Rumsfeldian’ archaeology, as it recalls the ‘*known knowns, known unknowns, and unknown unknowns*’ of Donald Rumsfeld (Wilkins 2009:43). Following a staged process of constraints study and route selection (designed to avoid ‘*known knowns*’ wherever possible) an environmental impact assessment is undertaken to assess the land adjacent to these sites for *known unknowns*. Irrespective of these results, the entire road corridor is comprehensively tested with a centre line trench running from start to finish designed to find those *unknown unknowns* that under any other system could well have fallen through the net.

During all but the final stage of works, it is the landscape that is sampled, with a transect survey designed to evaluate archaeological potential. At excavation stage the site is sampled, ensuring that sufficient information is recovered to preserve by record what could potentially be lost. The quality/quantity issue must therefore be assessed at two different levels, because sampling strategies necessarily take place at two different scales. The question of whether sample size matters (or if it is what you do with it that counts) was assessed by an *Oxford Archaeology* pilot study into archaeological decision-making processes and the yield ratio of differing sampling strategies on infrastructural projects carried out in southeast England (Hey and Lacey 2001). Twelve projects were assessed with an accumulated coverage of 240 ha. By comparing the predictions made following evaluation of these projects with the actual results identified during excavation, computer simulation of alternative trenching patterns could be used to assess the likely identification of different types of archaeology.

Non-intrusive methods of site investigation were of limited value; geophysics identified the limits of sites with magnetically enhanced soils, but this always required testing. Field walking was successful in identifying early prehistoric sites

containing durable artifact remains in the plough soil, or low visibility multi-period landscape features (Hey and Lacey 2001:58). In addition to desk-based studies, these techniques were considered useful in developing evaluation strategy, but the only way to assess the extent of subsurface features was judged to be by trial trenching (Hey and Lacey 2001: vii). The pilot study assessed a range of different trenching configurations with sampling strategies varying between 0.8% and 5.6% of the overall land-take, with an average of 2.4%. The percentage of a site needed to be trenched in order to adequately evaluate sub-surface archaeology was judged to be contingent on the character of the features on the site.

Where linear boundaries, substantial features and clustered remains survive, and Roman sites are obvious examples, a lower sample could be adequate, though even here 35% would be required to expect a moderately good assessment. However, more scattered and ephemeral remains, and Bronze Age and early medieval settlement sites are a good example of these, could be missed entirely by sampling at this level.

(Hey and Lacey 2001: vii)

In a procurement system of competitive tender, sampling strategies are embedded in business models and their effectiveness is rarely open to question. Sampling decisions take the form of ‘professional judgment’ with the implicit belief that sufficient information can be recovered from a preset percentage of features. The law of diminishing returns is cited in support of these strategies (Orton 2000:7), as fixed budgets mean excavators must concentrate their resources on the specified brief (Locock 2008). Hey and Lacey advocated a ‘strip, map and sample’ approach to large-scale infrastructural projects, with follow-up excavation work on critically selected areas (2001:55). This methodology is now widely practiced in the UK, with the monitored removal of topsoil – or watching briefs – undertaken on all large-scale developments. The logistics of removing such quantities of earth mean that archaeological work takes place at construction stage – with additional time pressures on the archaeologists, and a potential threat to the quality of the work undertaken. This differs in Ireland where all archaeological work on highways projects is completed far in advance of construction, with 12% of the road-take tested with trial trenches. These are then backfilled until the next phase of works, when areas of potential identified during testing are then stripped-mapped and 100% excavated. Infrastructure schemes are often financed through Public Private Partnerships, and in this respect it is vital that the National Roads Authority offload the road corridor to the private consortium of developers minus archaeological risk. Compared with UK Planning Guidance, the legislative power of the National Monuments Act means that the potential for encountering nationally significant archaeology (that could then be declared a National Monument) could delay construction to a financially unacceptable extent. The business models and regulatory frameworks operated in the UK and Ireland account for the reason certain practices remain embedded in commercial archaeology, but they do not explain how these practices developed in the first place. A consideration of the social context of these ideas reveals that differences between excavation strategies are less a product of design, and more the pragmatic result of divergent disciplinary histories.

## Dead Generations

Following what has been called the ‘Rescue Revolution’, the institutional framework in which excavation projects were undertaken in the UK changed from state control to commercially funded (Jones 1984). The rescue movement reacted to post-war reconstruction, laying the foundations for a network of regionally based field units. Widespread development preceded adequate legislation, forcing the archaeological community to lobby for recognition and develop working practices that could deal quickly with the archaeological ‘problem’ (Hunter and Ralston 2006:45). Preceding this, in the pre-war period fieldwork was predominantly undertaken by national organizations such as the Royal Commissions, major museums and nascent university archaeology departments (Wheeler 1954). Collingwood’s insistence on clearly formulated questions was influential in archaeology’s adopting a scholarly approach to excavation. Field projects had three main objectives: to prepare sites for public display, elucidate stratigraphic sequences, and define type sites where possible (Lucas 2006:17).

From the Second World War, field archaeology began to move away from the academy, and projects were less about the presentation of monuments and more about rescuing sites threatened with development. The growth of Cultural Resource Management through the 1970s and 1980s depended on standardization and repeatable procedures, a move that was further consolidated in the 1990s in Britain with the issue of PPG16/15 and MAP 2 (Hunter and Ralston 2006:45). The programmatic aims of processualism lent scientific credibility to a discipline that had come to be funded on a commercial footing, and provided the methodological tools such as sampling strategies, field surveys and statistical management of the resource. Codification, structured management systems and objective methods were driven by the needs of a market created through legislation to operate a level playing field in the public interest (Adams and Brooke 1995:96).

In Ireland, the opposite was the case, with state control of archaeological impact and provision of a license-based system enacted in the 1930s, long before widespread development became an issue. Starting in the 1980s and particularly from the later part of the 1990s, the ‘Celtic Tiger’ economy in Ireland fuelled intensive infrastructural development, consolidated by an amendment to the National Monuments Act (1994) and issue of frameworks and principles (DoAHGI 1999). As already noted, these foundations were set in place in the early 1930s with the establishment of many of the features that are still central to Irish archaeology, and on which the rise of Celtic tiger archaeology has been based.

On Easter Monday, 1916, Patrick Pearse read out a declaration on the steps of the GPO building that signaled the start of the Easter rising and the beginning of independence. His first words were this: “In the name of God and of the dead generations from which she receives her old tradition of nationhood, Ireland, through us, summons her children to her flag and strikes for her freedom”. The National Monuments Act was enacted in 1930, providing for the guardianship, preservation and acquisition of monuments, restrictions on the export of monuments,

and the licensing of excavations. It set in place the state apparatus to answer Pearse's original invocation to the 'dead generations' and made all archaeological remains the property of the state. A key issue in the 1930s was to deliberately identify archaeology as practical and non-speculative, and invest control with a professional elite whose main role was the scientific recovery of information about the past. This was supported by the licensing system, which insulated the discipline from criticism from related disciplines (Cooney 1995:268).

Adolf Maher, from Vienna, became Keeper of Irish Antiquities and Director of the Museum from 1927 until the beginning of World War II (Mullins 2007). Two of the most influential of the first generation of Irish archaeologists, O'Riordain and Raftery, worked directly with Maher in the National Museum and carried out doctoral research in German universities. This laid the foundations for the empirical tradition in Irish archaeology, incorporating a focus on the detailed, systematic recording of material evidence and reluctance to theorize that some would see as characteristic of German archaeology in that period (Cooney 1995:268). Continuing this tradition, there were only about 50 archaeologists working in Ireland in 1970, based in university departments, museums and the monument service. Excavation was research focused, but from the 1970s a second wave of development took place with the growth of the profession in the commercial sector. Building on this in the 1980s and particularly from the later part of the 1990s, the 'Celtic Tiger' economy in Ireland fuelled intensive infrastructural private and public sector development (Gowan 2007:24). What followed was an innovation in techniques and a shift to single context recording, in response to a service need that the state could not meet. Unlike Britain, where 'sample archaeology' was becoming established with the programmatic aims of Processualism, the Irish presumption to 'total archaeology' had more in common with a Culture-History approach.

## Fit for Purpose

While archaeologists disagree how policy and regulation should be implemented, all would agree that the purpose should be to achieve quality and best practice according to internationally agreed standards. The emphasis has been on generating a quality product (such as publication) without considering quality process (or how this product might be realized by our excavation strategies). This is an important distinction, because in commercial sector archaeology a quality archaeological product (generating new, secure knowledge of the past) is not necessarily the same thing as quality management of archaeology (managing a program of archaeological work within time and budget). The challenge is to ensure that our historic environment strategies are 'fit for purpose' – a measured approach derived from manufacturing industry that equates quality with the fulfillment of a specification or stated outcome (Woodhouse 1999:29). If the goal of archaeological excavation is to generate a quality archaeological product, then it could be argued that a conflict of interests has emerged between the wider archaeological community and the construction industry.

In ‘quality management systems’ the concept of quality is defined as the “means of satisfying the needs of the customer, outspoken or not” (Willelms and van den Dries 2007:6). In commercial archaeology the needs of the customer, in this case the developer, will be judged in terms of time and money and not in terms of the quality of the end product: new knowledge about the past. It is a frequently used business cliché that a bad product can only be sold once. In commercial sector archaeology this maxim only applies to the management side of business operations. A highly successful archaeological business can trade on an exceptional reputation in the construction industry, whilst simultaneously producing poor quality results for the archaeological community. Commerce depends on market principles to operate, but the extent to which these can be applied to commercial sector archaeology is limited (Hinton and Jennings 2007:106).

The archaeological market is an artificial creation that exists because the state wants archaeological information and creates legislation with which developers must comply. The product bought from an archaeological contractor is of no interest to the developer and has to be delivered to and shared with the State. As buyers do not have exclusive control over the product they purchase, this is an additional motive for wanting to keep the price as cheap as possible. In this situation, there is no market logic driving the impetus for quality of the archaeological product, and in an increasingly competitive market the quality of the archaeological results are placed in jeopardy (Hinton and Jennings 2007:107).

Different European states have addressed this challenge in different ways, but measures introduced to ensure ‘quality archaeology’ can be summarized as working at two distinct levels. One regulatory mechanism deployed by the state to monitor quality may be through controlling access to the market, another may be by supervision by government agency. At an organizational and policy level, the market may be regulated with voluntary or enforced guidelines on standards and methodology, and supervised with monitoring systems or regulated permits and licensing. In the UK, State interference in business practice is normally limited, so market problems are dealt with through the creation of professional associations to establish standards of performance and ethical systems of control (the *Institute for Archaeologists* in the UK, with an equivalent body, the *Institute of Archaeologists of Ireland*). Another approach would be to guarantee the quality of the product and its relevance and contribution to knowledge about the past, supported with a research agenda and peer review system. The current call in the Republic of Ireland for a quality product relates to this secondary level of regulation.

Margaret Gowen, in her capacity as vice-chairman of the *Institute of Archaeologists of Ireland*, has explained the shortfall in publication as a consequence of how the market is structured and regulated (Gowan 2007:29). Whereas both the *DoEHLG* and the *National Museum of Ireland* monitor excavation through the license system, the quality control of post-excavation is much harder to track. License holders are professionally liable for projects but not commercially liable. While this may work at excavation stage (because their clients will ensure work is completed without delay), a lack of accountability ensues during post-excavation. With no mandatory structure or mechanism for control of reports, individual directors rely on voluntary,



self-motivated efforts to publish for peer review, often without the support of the archaeological companies that tendered for the work in the first place. As the market has no perceived stake holding in the archaeological product, achieving quality in this area cannot be left alone to market regulation. In short, the license system as it is operated provides no guarantees that license eligible directors will produce a quality archaeological product, only that they will provide a state accredited service to discharge. Commercial excavation may be characterized by its stance of objectivity, but the market model is far from neutral, and regulation is clearly needed to mitigate the negative effects of commerce on knowledge production.

## Conclusion

British and Irish commercial sampling strategies are a product of divergent disciplinary histories and business models. Development-led archaeology is a phased programme of works, with different issues at stake at different parts of the cycle. In the context of archaeological highways projects, sampling strategies necessarily take place at two different scales. At evaluation stage, the landscape is sampled, with a transect survey designed to test the archaeological potential. At mitigation or excavation stage the site is sampled, ensuring that sufficient information is recovered to understand what will be lost. Evaluation works benefit from the additional information derived from sampling a larger percentage of the road corridor, and in this respect Irish methodology can be seen to have clear benefits over British counterparts. But does the equation of quantity and quality also extend to the excavation stage of archaeological works?

The commercial framework is designed to effectively mitigate construction impact on the archaeological heritage. It is concerned with quality ‘management’ of archaeology generating large quantities of information about the extent and character of the archaeological remains on which to base a planning decision – and in this respect the more information the better. But the problem with equating quality with quantity is the underlying assumption that if enough records are made and sufficient phenomena observed, enlightenment will be experienced – simultaneously arriving at the other definition of quality: new secure knowledge about the past. Irrespective of the quantity of material excavated, the archaeological categories used to interpret the data must release understanding that has credibility in terms of the past processes they represent, whilst the ensuing narratives must remain democratic and comprehensible to the wider public.

In 1995, Gabriel Cooney noted that the international debate concerning theory and practice had passed Ireland without impact (1995:264). He interpreted this skepticism as the reaction of a nationalist archaeology to an imperialist tradition. With a continual stream of new discoveries capable of throwing light on all prehistoric and historic periods, he saw an inverse relationship between engagement with theory and the wealth of the archaeological data excavated. The concept of an objective past, ‘preserved by record’, continues to justify the collection of ever-increasing

amounts of data, on the proviso that “if enough records are made and sufficient phenomena observed, we will experience some kind of enlightenment” (Bradley 2006:6).

The previously buoyant Celtic Tiger economy and the concomitant rise in commercial sector archaeology have arguably consolidated the problem. Developer-funded archaeology is modeled on a system of competitive tender, and the commercial imperative created by this structure does not easily accommodate uncertainty, which could be perceived by clients as ‘navel gazing’ (Lucas 2001:2). In this climate, the dramatic new finds arising from road schemes will be quickly rewarded in the disciplinary hierarchy if they conform to expected preconceptions (Cooney 1995:272). But if the right questions are not brought to bear on our observations in the field, then new secure knowledge of the past will remain elusive.

Recalling Bradley’s two separate ‘cultures’ of archaeology, the consequence of a fully privatized sector is that many field teams are out of touch with the latest interpretive ideas, and many academics are unaware of the latest excavation results. With no structure to realize the potential of commercially generated information, and no mechanism to disseminate this widely as new knowledge about the past, archaeological decision making is undermined. Many great advances have been made by commercial sector archaeology, but as recession looms large, and as both the UK and Irish governments continue to review their historic environment strategies, it is essential that the limitations of the commercial framework are acknowledged and challenged. Only then can a system designed to deliver quality management of archaeology for the customer (time-bound and within budget) be enabled to find new, secure knowledge of the past for the betterment of society as a whole.

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# Chapter 5

## An Archaeology of Many Steps

Marianne Lönn

### Theory Versus Material

Forty years ago theory changed archaeology. The New Archaeology started a discussion that has rendered us a multitude of theoretical aspects in an ever-changing flow. Archaeology has become theorized and has developed largely for the better. Unfortunately, in the theoretical debate one crucial part of the archaeological work has all too often been omitted – the fieldwork. Fieldwork developed in its own way, which has been rather un-theoretical. A consequence of this is the creation of two different archaeologies with different skills and values (see Hodder 2001). With a slight exaggeration one can say that some archaeologists have their career within the university where theoretical discussions take place and where a thorough insight in theory is needed. Others have their career within construction archaeology where a firm knowledge of the archaeological material and of excavation experience is necessary. It seems that the distance between theory and material has become an institutional problem. This can create educational problems (see Aitchison 2004 and Hamilakis 2004).

During my more than 30 years as an archaeologist I have worked nearly 20 years with construction archaeology but I have always tried to keep in contact with the university and its archaeo-theoretical debate. This has resulted in a lot of field-experience and, if not a great competence in theory, at least an interest in theoretical matters. It has also been greatly frustrating. At an excavation, standing with my feet literally on the ground looking at a dark spot or a dirty piece of pottery, I have difficulties in connecting this to for example Anthony Giddens' or Michel Foucault's sociological models for a society. The distance between the two units is far too great.

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In this situation an archaeologist is left with two alternatives, one is to leave theory to the university and never think about it again, the other is to try to diminish that troublesome distance or to find some way to get around it. I have chosen the latter.

Papers and discussions at EAA (European Association of Archaeologists) in Zadar, Croatia 2007, TAG (Theoretical Archaeological Group) in York 2007 and at the department of archaeology in Gothenburg have made me understand that I am not the only frustrated field-archaeologist. My opinion that there is a large distance between field-archaeologists and theoretical archaeologists, as well as between theory and material, has also been confirmed. Many times the distance is large enough to cause problems in discussions and co-operative work. Evidently, Middle-Range Theory has not yet found a way to bridge the ever growing distance. Maybe the theorizing of fieldwork will help to solve the problem. In this article I am going to describe some attempts to look at fieldwork more theoretically. I do this as an archaeologist of the field and I believe that the field-experience actually is the clue to understanding and theorizing fieldwork. I am going to look at the thoughts and historicity behind interpretations and also show how knowledge comes about and how it is communicated. I believe that archaeology will benefit greatly, if the two sides became more interrelated.

Since I work in Sweden, more specifically on the Swedish west coast and mostly in the county of Bohuslän, the examples I present are from different excavations in that area.

## Middle Range Theory Once Again

The New Archaeology, when it was introduced in the late 1960s, prescribed theoretical thinking and theoretical models for interpretation. This created the need to connect theory and material, since the demand for empiricism was still deeply rooted in archaeology. The problem was defined early on and the concept of Middle Range Theory was introduced in 1977 in Lewis Binford's introduction article in *For Theory Building* (see also review by Schiffer 1980).

There was in those days a considerable interest in developing and discussing theoretical ways to broaden the possibilities of archaeology. The journal *Advances in Archaeological Method and Theory* (edited by Michael B. Schiffer) reflects very well this situation. It began being published in 1978 and the last issue was in 1987. Many articles were published there which were important for the development of archaeology. *Advances in Archaeological method and Theory* was followed by *Archaeological Method and Theory* 1989–1993 and the launch of the *Journal of Archaeological Method and Theory* in 1994 both with the same editor, Michael B. Schiffer. There has also been a discussion about Middle Range Theory in itself, for example in the article *Middle-Range Theory in Archaeology: A Critical review of Origins and Applications* by Raab and Goodyear (1984) and Thomas (2004). I shall leave the literature of Middle Range theory at that and I will only touch upon works attempting to theorize fieldwork. In Sweden there is an ongoing discussion about

how to direct the enormous amount of new material each year into certain fields of investigation i.e. how to make it conform to knowledge in a broader sense. There are also discussions about the differences between university archaeology and construction archaeology; and about research in general within construction archaeology (for example Andersson et al. 1999; Ericsson 1999; Anglert 2001; Bolin et al. 2001; Högberg and Rudebeck 2001; Berggren and Burström 2002; Ersgård 2006; Lönn 2006).

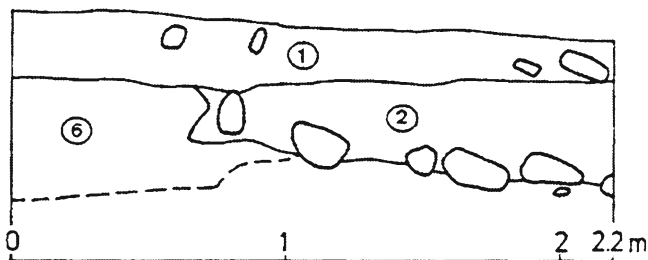
From the late 1960s and onwards fieldwork developed too, mostly as a part of construction archaeology. New methods were used, new kinds of analysis were brought in and digital measuring, plotting, drawing and analysing were introduced. Also, at least in Sweden, the field-archaeologists became better educated. Today there are many archaeologists with PhDs working in Swedish construction archaeology. This means that there is more research done, more expectations as to what results that come out of an excavation and more demands for theory.

## **Towards a Theory of Fieldwork**

The theory of fieldwork should not focus on descriptions of different methods or on construction manuals on how to excavate. Those things are of course important, but not our main task here. We need to concern ourselves with questions like: What is experience? How does the conception of something come about? Where does it start? How is knowledge communicated and to whom? What forms an interpretation? All these are difficult questions and it would take more than an article to answer them fully. Here I shall describe an idea of how one can work with them and perhaps also give some answers.

### *Creating Knowledge*

One lively discussion in Scandinavian archaeology has concerned the dwellings of Mesolithic people. At excavations of Mesolithic sites in western Scandinavia we rarely find anything but cultural layers and flint. We can very seldom identify structures such as hearths, post-holes or other remains of building constructions. In the 1980s there were therefore no known Mesolithic huts on the Swedish west coast. Despite a vast amount of settlement sites, identified and dated by the flint-material, well spread over the whole coastal area, the remains of their actual homes had not been found. No-one really knew what the huts would look like, especially not their remains in the ground. Many archaeologists had considered the possibility of finding them and had some ideas as to how they would be identified archaeologically. Maybe there was some mental barrier or tradition preventing them from changing the focus of excavations and really trying to find them. Anyway, the flint-material gave lots of information as to the whereabouts of the people, as well as their cultural



**Fig. 5.1** The elusive feature. The area marked 2 is the dark-coloured feature (From Schaller Åhrberg 1996:14)

basis and economic strategies; many archaeologists were content with that. Then in 1988 a strange feature was found at an excavation. Something had been dug down in the ground and in a section a distinct dark figure with a sharp edge in one side was visible (Fig. 5.1).

The archaeologist in charge, Eva Schaller Åhrberg, had not seen anything like it before and not knowing what it was, she asked around. Many archaeologists working in the area came to see it and discuss the find, but nobody had any idea how to interpret it.

A few years later, in 1994, an oval dark spot was detected in the midst of an area with Mesolithic flints. The find was interpreted as a Mesolithic hut by the archaeologist in charge, Bengt Nordqvist. This, of course, created a huge interest and many archaeologists in the area went to have a look at it. The interpretation caused an intense debate. Some accepted the interpretation and some did not. Unfortunately the excavation report was not published until 2005, which made the results difficult to use in further discussions (Nordqvist 2005). Among the participants at this excavation was Eva Schaller Åhrberg, mentioned above, Robert Hernek and Glenn Johansson, all of whom had parts to play in the process of creating knowledge about the huts of the western Swedish Mesolithic.

In 1997, with the experience of a possible Mesolithic hut, Robert Hernek and Glenn Johansson found an oval dark-coloured feature of 5 by 4 m and 0.25 m deep. The feature was dug down in the ground and its profiles at the sides looked just like the feature Eva Schaller Åhrberg found in 1988. There were also post-holes, hearths, cooking-pits, flint-material and five unanimous radiocarbon dates. The find was interpreted as the remnants of a hut, whose floor had been dug down in the ground and whose roof had been supported by posts within the structure. A hearth was also placed inside the hut. The features in and around it supported the hut-hypothesis and the material and the carbon dates could place it in the Mesolithic, 7500–7000 BC (Hernek and Johansson 1998). Of course colleagues went to have a look and this time very few, if any, doubted the interpretation (Fig. 5.2).

The following year yet another locality with possible hut-remains was found and Glenn Johansson was the one to identify two possible Mesolithic huts. This time the features did not look like the earlier finds, though, and the interpretation, built on coherent distribution patterns of phosphate values, stones and flint-material, was



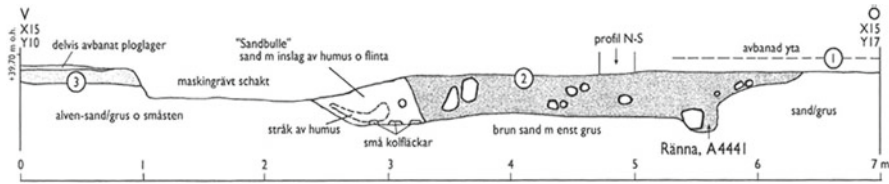


Fig. 5.2 Section of a Mesolithic hut, RAÄ 140, Forshälla socken, Bohuslän (From Bengtsson, Hernek and Johansson 1998:27)

much discussed among the colleagues. The weakness of the interpretation was the lack of any recognizable structure. It became even more discussed when carbon dates indicated the site was early Iron Age (Johansson 2003). Still, huts or no huts, it is interesting to note that earlier experiences of excavating Mesolithic huts made Glenn Johansson recognize the distribution patterns as possible remnants of huts.

And principally, what happened? What are the principles of cognition? I see the following steps:

1. *Cognition of a phenomenon.* We know that people have been in Bohuslän during Mesolithic times and we understand that they must have had a home, hut or house, somewhere. But we do not know what it looks like and we have never seen and comprehended such a home.
2. *A find that may be the remains of this phenomenon.* Someone realizes that a certain feature might just be the remains of a hut.
3. *A visual memory and a mental preparation for the next find.* The archaeologists that see and discuss this feature acquire a picture of what it might look like and in this way gain a visual memory of a possible hut.
4. *Identification of the phenomenon/ancient monument by the next find.* The next time this kind of feature turns up, an archaeologist who has seen it before or heard of it, will be prepared for such an interpretation and will adjust the excavating method and analysis to such an interpretation.
5. *Confirmation by additional finds.* Following finds will be compared with earlier ones. They will be accepted or not accepted in relation to the current knowledge. Some will also add to the knowledge.
6. *A known and accepted phenomenon/ancient monument.* When many archaeologists have accepted the interpretation, it will become credible and further on it might even become the truth.

In the example above I have deliberately used personal names to underline the fact that we are not yet dealing with general knowledge, but with very personal ones. In the beginning of the process we must almost totally rely on personal, visual, memories and personal experience. To take this even a bit further, we know that interpretations are subjective. This means that interpretations at an excavation are dependent on the excavating archaeologist's visual memories and general archaeological knowledge. We are indeed relying on personal experience.

## *When the Process Comes to a Halt*

Sometimes the chain of events, the process, is cut off before reaching the final stage of acceptance. This can happen at different stages and for different reasons.

### **Stone-Covered Hillocks**

In 1975 there was an excavation of a small cairn situated on the top of a small hillock. While taking away the turf and topsoil from the lower part of the hillock it was discovered that every crevice was filled with stones and that there were also stones laid around the base of the hillock as a brim. Among the stones were flint, pieces of pottery, soot and charcoal. The full size of it was about 70 by 35 m. It was the first find of its kind and of course it bewildered the excavating archaeologists as well as visiting colleagues (Andersson 1976) (Fig. 5.3).

A few interpretations were put forth. One was that the whole thing was to be seen as a grave; another that it had something to do with cultic ceremonies. To talk about cult in Scandinavian archaeology in 1975 was not entirely accepted since it was in the midst of the processual archaeological period, when interpretations were supposed to be functional. Thus, the cultic interpretation was not pursued.

So, the process of knowledge-building was cut off, mostly because of a lack of comparisons, this was a unique feature. The process was also cut off because of a theoretical trend that did not favour cultic interpretations. The next step in the chain of events did not occur until some 20 years later in 1998 when something of the same kind was found. It was a rather high, oval hillock with a broad brim of stones around its base. There were several graves and a house on the same site. The find was interpreted as a cultic site. Later on more stone-covered hillocks, mountain-sides and hill-tops have been found. They are different in size, appearance and location, but they all contain a hillock or a part of a mountain more or less covered with stones; they all have some connection to graves and most of them were built in the Iron Age.

In some cases it seems as if several graves, formed by stones to round or ship sized forms, have been unified by a continual covering of stones. In other cases the crevices of the hillock have been filled with stones sometimes with the addition of a stone brim at the base. A small amount of burnt bones and artefacts are usually found (Fig. 5.4).

Today, many archaeologists are intrigued by this phenomenon and take part in the discussion about them. Now we know what to look for and where to look for it, but we are still not certain as to the interpretation and we certainly do not understand its historical and social context. The only thing we know is that rocks, mountains and stones have a special part to play in the conceptual philosophy and belief of the time.

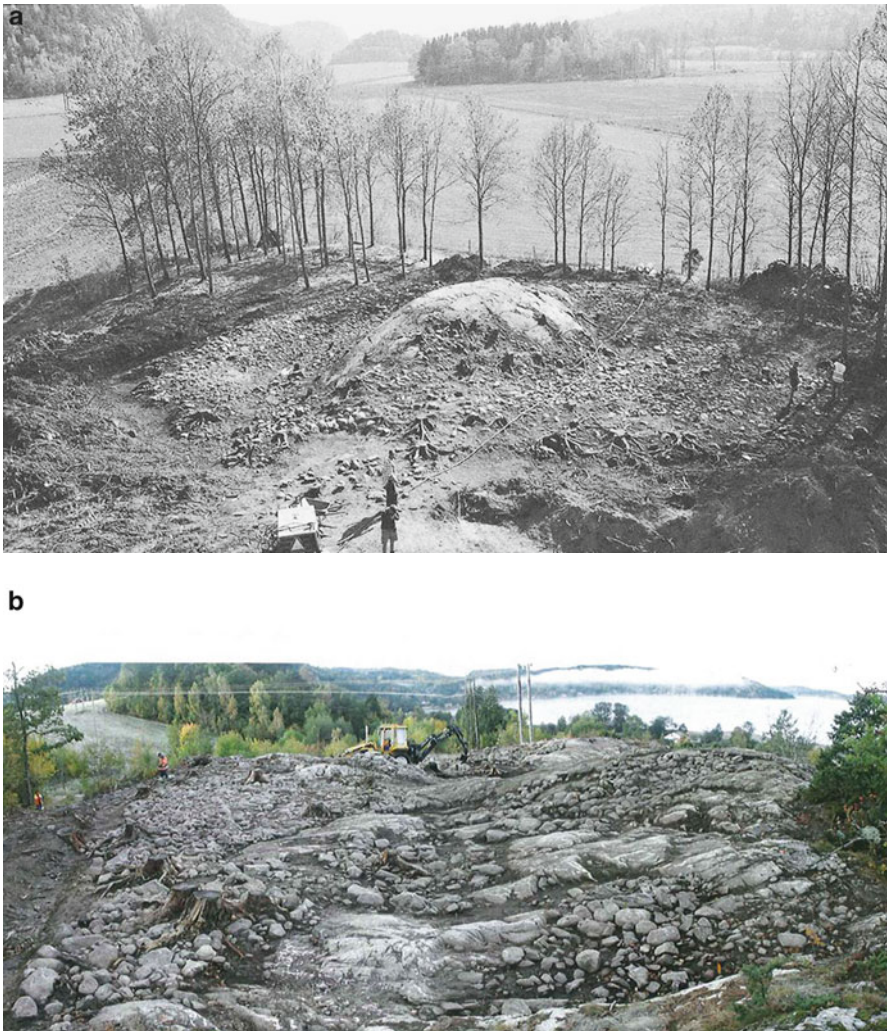


**Fig. 5.3** Stone-covered hillock (From Andersson 1976:139)

In the case of stone-covered hillocks there were several reasons for the halt in the process of knowledge-building when the first find was discovered. First of all, the find was a total surprise, there was no mental preparation for such a find and hence the discussion was very confused and the interpretation had to start from scratch. There were no known finds to compare it with and finally the theoretical views of the time favoured functional interpretations and such an explanation was difficult to find.

## **Slash-and-Burn Cultivation**

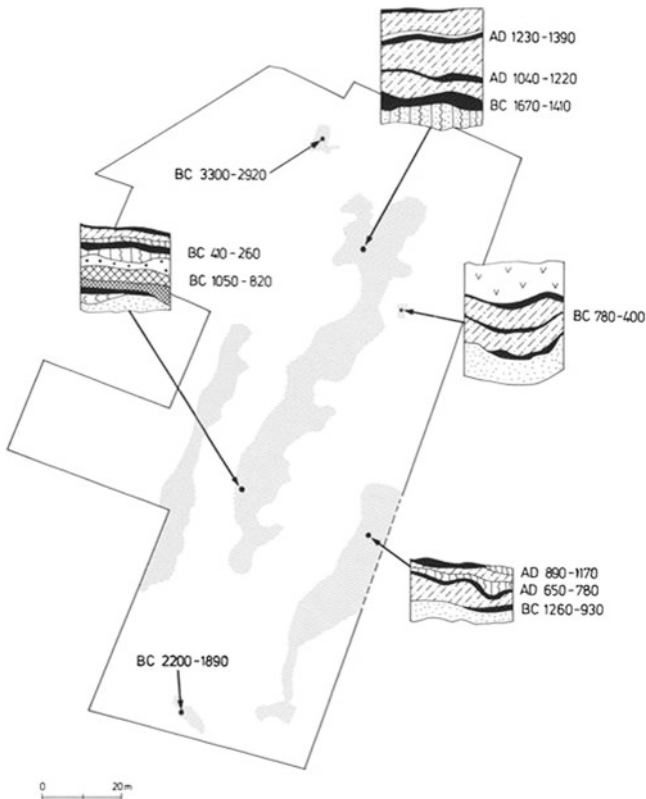
At an excavation in 1990, there was a very confusing find of dark coloured areas in a rather large field. When digging trenches through these areas it was detected that they contained layers with soot and charcoal alternating with layers of sand and soil. The sooty layers were dated to different periods of time. Some of the eldest at the bottom were Neolithic and some of the youngest at the top were Medieval. There were seeds from weeds and cereal in some of the layers; a house from around AD 700 was found near by; and pieces of pottery were spread over the area. It was interpreted as the remains of slash-and-burn cultivation from several occasions over a long period of time.



**Fig. 5.4** Examples of stone-covered hillocks, mountain-sides and mountain-tops (From (a) Munkenberg 2003:137; (b) Lindqvist and Toreld 2005:55)

This interpretation was possible only because the archaeologist in charge, Gundela Lindman, previously had done research on exactly the same phenomenon but from historic times. At the time of the excavation she had not seen any prehistoric examples, but still she had a mental preparation for it and a visual memory of what things like that could look like. That made it possible to think of such an interpretation and adjust the excavation strategy and methods to test the hypothesis (Fig. 5.5).

There was an enormous discussion about the interpretation, almost uproar. There is as yet no consensus among archaeologists in the area. Some find the interpretation



**Fig. 5.5** Sections of trenches and results of carbon dating (From Lindman 1993:64)

quite believable, others are absolutely against it. There have been no more finds of the same size, only some small remnants excavated by the same archaeologist. The process of knowledge has stopped. Maybe she was wrong; maybe she was right, as of now we do not know for sure. The find is still unique but maybe there will be a confirming one later on. Today there is a discussion going on among historical agriculturalists. Lindman's interpretation seems more accepted among them and she takes part in those discussions. Since the find was excavated more than 15 years ago, the memory is fading, which is sad. The chance to remember and relate to this find, when a new one comes along, is reduced, even if there is an excavation report.

## Communicating Knowledge

Communicating knowledge most often leads us to think about books, articles and reports. As seen from the example above, though, the communication of knowledge starts much earlier, it is not then written, it is a verbal communication.

There are the discussions at the excavation, about a feature which has been seen or maybe the showing of a picture. There is also the telling about earlier finds when something strange turns up at an excavation; or the call which is made to a colleague, who you know has seen something like the feature that you have just uncovered. There are also the talks around the coffee-table in the office. These pieces of information are personal, visual memories of individual archaeologists informing other archaeologists in the vicinity. To take the next step, there are small seminars and larger conferences, power-points shown at a lecture and so on. All this is verbal information and therefore temporary and the receivers are individual archaeologists or small groups.

It is important to understand that lots of knowledge, especially within contract archaeology, has not reached the written stage and thus only exists in the heads of some archaeologists. It is an invisible knowledge, a day to day learning of experience that, at best, is communicated from one field-archaeologist to another. I do believe that some of this knowledge will never reach that written stage. There is for example the experience of the landscape; the knowledge about the perception and use of it and the movements in it. There is also the knowledge about where to find a row of cooking-pits and to know what a cooking-pit looks like on the surface when you have taken away the top-soil. Knowledge like this differs from area to area and is often considered so simple and obvious that there is no need to write it down. It is only passed on from one archaeologist to another when being in that landscape.

Lasting, written, communication exists in the case of field-archaeology in the form of excavation reports and sometimes further analysis presented in articles or books. In the last decades at least in Sweden we have left out one step. That step is the publication of empirical studies; the detailed presentations of material of all sorts. Such publications have, in the eyes of a more theoretical archaeology, been considered of less value; less scientific. This is in a sense right, since there is no theory or further analysis attached to these publications. What is forgotten though, is that they represent an important step in the building of knowledge. When they do not exist, every archaeologist has to start from scratch; collect, arrange and analyse every material or type of artefact by her- or himself. That is an enormous waste of time, money and knowledge. In my eyes, the lack of publication of material during the last few decades has resulted in a diminishing general knowledge about material. From a theoretical point of view, this may be of less importance, but for field-archaeology it is a disaster.

Another step that we see too little of is the one that contains discussions about methods and analysis. Some discussions can be seen now and then, but we need more if we want to develop field-archaeology. Thus, the way I see it, there are at least three important steps to consider. The first step is the excavation report; the second the publication of material, discussions about methods and analysis and such; and the third is the publication of articles or books. Among these, the second step should be more emphasised.

## Interpretation and Theory

Seen from the point of view of fieldwork the road to comprehension of prehistoric societies runs from material to visual memories and experience; to communicated knowledge; to interpretation; to hypothesis and theories; to comprehensive theory and knowledge. Theory is always there in the minds of the archaeologists; from their educational background, their experience and their discussions and so on. Of course this influences the interpretations. So, sometimes intentionally and sometimes unintentionally, interpretation of material, dating, contexts and the like is affected or even determined by theory.

In analysing interpretations the theoretical background can usually be detected. Even when there seems to be no theory involved, there is such anyway. In the recognition of Mesolithic huts described above the theory seems to be absent, but I would describe it as an empirically and inductively identified structure. The knowledge about stone-covered hillocks also seems to have been empirically and inductively won, but there is theoretical thinking involved in the interpretation, or rather the lack of interpretation. In 1975, when the first structure was found, interpretations involving prehistoric conceptual philosophy and cult were not accepted; or at least not very highly thought of. Some decades later the values of archaeology have changed and conceptual philosophy of prehistoric times is regarded as very interesting. A quite different and rather wide range of interpretations is now possible. This is also a very interesting aspect of interpretation – the historicity. How much do contemporary values and the level of knowledge in archaeology in general affect interpretation? It is also interesting to see how the basis for an interpretation is forgotten after a while.

### *Bucket-Shaped Pottery*

Finds of a very special kind of ceramic pots have been made in Norway, mostly on the south and west coast; from Trondheim to Bohuslän (before 1658 a part of Norway). The clay is generally blended with soap-stone. The vessels also have a special form; they are shaped like a bucket and are very nicely decorated. This kind of ware is distinctive and they are called bucket-shaped vessels (*spannformade kärl*) (Fig. 5.6).

The core area for these vessels is the area around Stavanger in Norway but they exist in the southern half of Norway and the south-western part of Sweden. In the core area the eldest pots have been dated to the late Roman Iron Age, but generally they are dated to the Migration period, 600–800 AD. This dating is the accepted one for Bohuslän.

The core-area is considered to represent the place of origin from which the idea in a diffusionist way has spread to other parts of Scandinavia. This idea has been generally accepted even if there are some discussions about influences from other areas.



**Fig. 5.6** Bucket-formed vessel

Furthermore the dating was suggested already in the early twentieth century and was then built on typology. It does not seem to have been questioned since. Thus the dating of this kind of pottery depends on typology and a diffusionist way of looking at distribution of goods and thoughts (see for example Breivik 2006 (Magisteropgave, Trondheim); Bøe 1931; Magnus 1975).

In 2007, an almost totally demolished grave was excavated in the northern part of Bohuslän. One of the few things that had escaped destruction was a decorated, bucket-shaped vessel filled with burned bones. It was still standing intact in a hole in the middle of the grave. There was just one thing that did not fit the picture, its probable age. Carbon dates from the burned bones in the vessel gave an early Iron Age date. The pieces of pottery left in the grave, as well as carbon dates of different structures in the grave, supported this (Lönn 2009a). If we stay with the accepted dating and spreading of bucket-shaped vessels, this person, a woman in her twenties or thirties, was buried in a pot that was around 500 years younger than herself. We have discussed possible explanations such as false carbon dates, wrong identification of the pot and re-burial of the woman, but there is no evidence for any of this.

Then how do we explain these seemingly impossible results? In my view, it can be explained if we disregard the typological dating and also the theory behind the previous dating; the diffusionist view of spreading of goods and ideas. If these are



replaced with a view that takes social behaviour and social networks into consideration, for example exchange of gifts as a token of friendship or other relations, marital alliances and so on, there is another possibility. In such a scenario there need not be a core area that has the oldest vessels even if most of them are manufactured there. Instead the vessels are produced and used during the same period of time all over the area. With such an understanding, the woman in northern Bohuslän was part of a social network that stretches from Trondheim to southern Bohuslän. Thus, different theoretical views can create quite different interpretations and dating.

### ***Deductive and Inductive Ways of Thinking and Working***

Very often the way you work is, consciously or not, determined by what you once learnt. In the beginning of 1990s a colleague and I decided to try and find the small places that lie outside the prehistoric farmsteads but still are a part of the activities in the local, social structure. We thought of places where you would lay your boat; collect flint, food or medicine; keep animals in the summer; perform spiritual activities; hunt certain animals; make certain tools and so on. We were both brought up with the deductive way of thinking and hence we worked accordingly. One of us usually worked with Stone Age periods and the other with Bronze and Iron Age periods, so there were a great variety of places to look for. Another important aspect was that we were also expecting to find places that we would not have thought of; we expected to be surprised. We called the project *Odd places* (Lönn and Schaller Åhrberg 2004).

Of course we found interesting sites, of course we were surprised and of course we missed sites that archaeologists after us found. Interestingly enough once we had found something it did not feel odd anymore, at least not to ourselves and after a while neither to the people working with us. On the other hand there could be strange misunderstandings when we talked to archaeologists outside our little group.

The project was set up in a very deductive way but what we really tested was inductively growing knowledge. To me this experience mirrored the way we work in construction archaeology and in other kinds of fieldwork as well. The procedure is deductive, but there are always inductive elements such as surprises of different kinds. It is naturally very important to take advantage of these surprises, since they often represent the interesting new knowledge, the unplanned leap ahead. We must recognise and accept the great amount of important new knowledge that derives from surprise finds; in other words old-fashioned empiricism and inductive work.

### ***Empiricism and Surprises***

Not only must theoretically influenced interpretations sometimes be challenged, archeologically accepted truths must be too. In both cases a new interpretation may

have troublesome consequences. In 2006 two graves were excavated in Stenungsund in Bohuslän (Lönn 2009b). Both seemed to be almost totally demolished. One consisted of scattered stones and earth and would not have been detected had it not contained so many pieces of pottery. In a hole in the middle a pot with burned bones was still standing and was only slightly damaged. The other grave had a brim of nicely laid stones around most of it and there was a dark-coloured area with burnt bones in the middle, but no urn. This grave also contained many pieces of pottery. The two graves were very similar in age, AD 400–540 and AD 380–540.

The discussion during the excavation focused mainly on the great amount of grave-goods, mostly very fine pottery. Later, when we had the results of the analysis, our focus broadened. One especially interesting and odd thing was that the two persons in the graves seemed to have been buried almost exactly at the same time. The results of the carbon dates from bones of the respective graves were the same; but since there is 100–200 years of uncertainty, the burials could still follow each other within a very short period of time. What made it stranger was that pieces of pottery from the two graves clearly came from the same pot. Unless something had happened when the graves were demolished, the burials had to have been performed at exactly the same time (Arcini and Lönn 2008). A couple maybe, dead and buried at the same time?

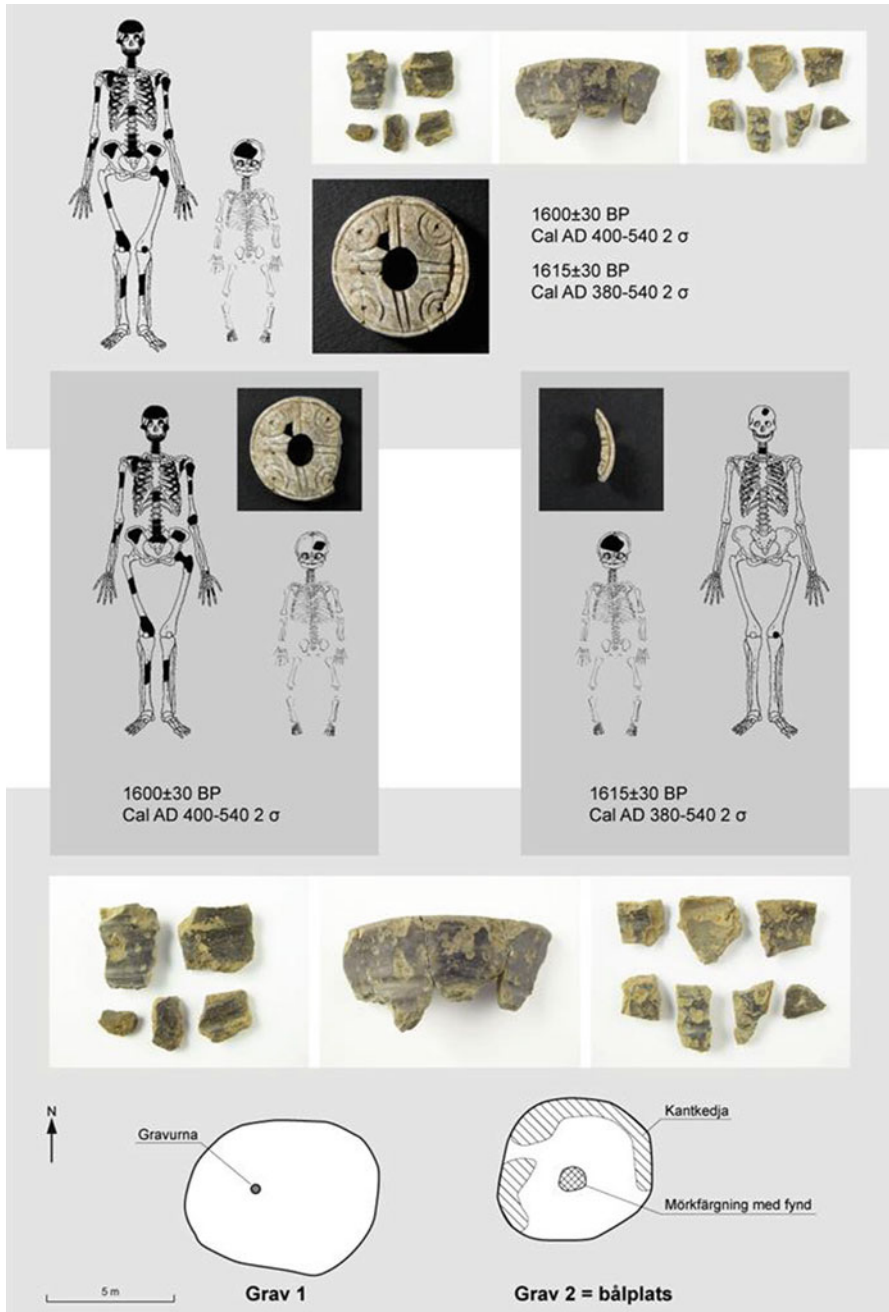
The osteologist found bones from two persons in both graves, a young woman and a baby, but there were different kinds of bones in the two places. It could in fact be bones from the same persons in both graves. We confronted our results and discussed the possibility of the graves not being two graves but one grave and one pyre site. The osteologist had seen a similar case before. She had written about it and I had read her articles, so we were not totally unprepared. New evidence was still to confirm that interpretation. A small marker or toy of bone was found among the burned bones in the pot from the first grave. Among the bones in the other grave a small piece of the same kind of object was found and the two pieces actually fitted together. There could be no more doubt. What we had thought of as the second grave was in fact the pyre site (Fig. 5.7).

Later I looked at several plans of excavated graves and found that in many of them there are dark-coloured spots that very well could be the pyre sites. In many cases the spots have been interpreted as graves, since they often contain small amounts of burned bones.

The consequences of this single find can be quite extensive. Do graves and pyre sites look the same? Why did people build a grave-like structure around the pyre site? And, if as much as half of the graves from the period between the birth of Christ and AD 600 are pyre sites, what does that do to demographic calculations from grave material or to any discussion involving graves and bones?

### ***Same Place: Same Time?***

An often used interpretation is that structures at the same place belong together, which of course is not always true. In the example with the graves above, there was a house under them and naturally we all discussed what it meant when a person was



**Fig. 5.7** A sketch of connecting objects at the grave and pyre site. At the bottom are the two graves and above those the finds and dates connected to each of them. At the top are all finds and dates put together (Arcini and Lönn 2008)

buried in his or her house. Carbon dates disclosed, though, that there was a span of around 500 years between the two structures (Lönn 2009b). The house had been long gone when the graves were built. This was very strange to us, because what had really led us to believe that a link between house and grave really existed was the fact that the urn in the grave was situated right by the hearth of the house. Sometimes chance plays tricks on us.

## An Archaeology of Many Steps

Of course there is a lot to do to create a theory of fieldwork and just as much to make theoretical field-archaeology, so to say, normal and generally performed. What I can conclude from my so far limited material and work, is that old-fashioned empiricism and inductive thinking are very important indeed for the growth of knowledge. So are years of experience of fieldwork and solid knowledge about the archaeological material. The whole chain of events from visual memories and onwards must all be there to create new knowledge of prehistoric societies. I see a great need to develop a theoretically inductive way of working. That could help us to see, comprehend and take advantage of surprises and transform them to knowledge.

In this context the method of reflexive field-archaeology has an important role to play (Berggren 2002; Hodder 1999, 2000). There is a great value in making discussions, decisions, choices and even mistakes visible and to present the thinking, perhaps the theory, behind an interpretation. It creates a highly needed transparency, which in turn forms the basis for understanding and improvement.

An old truth is that we must be conscious of our theoretical background. We need to improve our theoretical analysing of interpretations and develop our ability to use theory as a tool for interpretations. The historicism behind the interpretations must also be recognized. Even what we see as evident interpretations and truths must sometimes be challenged. Lastly, I must stress the absolute need to observe and recognize the first steps in the process towards a better comprehension about prehistoric societies; the verbal knowledge in field-archaeology. The visual memories and the experience of trained field-archaeologists must not be underestimated.

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# Chapter 6

## Stepping into Comfortable Old Shoes: ‘The Consequence of Archaeological Comfortability’

Cara Jones and Philip Richardson

### Introduction: Making Yourself Comfortable

This paper takes as its starting point a comment made by Baines and Brophy (2006a, b) concerning the ways in which archaeologists in the field render the unfamiliar deposits they encounter familiar or ‘comfortable’ through the very methods and practices they use. At the excavation of Battle Moss, Caithness (Baines and Brophy 2006b), the excavators admitted that they began with a pre-formed idea of what the archaeology was going to reveal. They were faced with a known site type – multiple parallel stone rows aligned on a Bronze Age cairn. However in practice, the archaeology did not conform to known typologies and Baines and Brophy talk of being forced to confront their own preconceptions; preconceptions which tended to be confirmed by their methodology until the evidence began to veer too far from the expected (Baines and Brophy 2006b:79–87). Originally, due to the lack of other excavations of these types of features, they drew on their previous excavation experience and background knowledge to develop a methodology. As is common in archaeology, the excavators at Battle Moss rendered the complex archaeological remains into a familiar concept i.e. a trench, which allowed them to relate to the previous trenches they had excavated throughout their careers. Yet, as most of us can appreciate, this reliance on the familiar, initially at least, turned out to be inadequate and the archaeological process became problematic or uncomfortable (ibid). However, they maintain that, the failure of their preconceptions, which led to this uncomfortable state, made them confront the site in different ways, it made them think outside of the box (ibid), or better yet, the trench. By confronting this un-comfortable state, rather than accepting it as part of the process, Baines and Brophy were able to explore the site in a variety of different, and in the end fruitful, ways.

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In a sense, their paper echoed concerns that we ourselves encountered while continuing our careers in commercial archaeology. The issues raised during the excavations at Battle Moss concern the wider epistemological basis of the construction of archaeological knowledge. These issues go deep into the fabric of British archaeological practice, especially the field work conducted under the rubric of developer-led archaeology. This paper then is concerned with the ways in which archaeologists in the field render the unfamiliar deposits we encounter, familiar or 'comfortable', through the very methods and practices we use. Thus activities in the past, very human things, become the knowable things of archaeology in the present – a ditch, a post-hole or a pit. The past becomes comfortable too (Baines and Brophy 2006a, b). To this end we will take a very familiar feature of British archaeology, the clay tile field drain, and attempt to further critically examine the issue of comfortability in the field.

## The Comfort Zone

The notion of 'comfortability' in archaeological practice can be seen as comparable with the concept of the 'comfort zone'. The comfort zone is a mental state, where the individual shapes their behaviour and environment in order to minimize risk and create a sense of security (O'Donohue and Kitchener 1996). Thus a series of operational boundaries, in this instance standard methodology applied to excavation, are set up and are the means where the unfamiliar is made familiar or comfortable. We would argue that the non-reflexive archaeologist establishes a comfort zone as a coping mechanism for the complexity encountered at the trowel's, or indeed machine bucket's, edge. Obviously a key part of the comfort zone is the individual's ability to 'step outside'. This 'stepping out of the comfort zone' usually occurs, as in Baines and Brophy's (2006a, b) case, when we are confronted with the novel or the unexpected and when we react in a different manner than we would normally (O'Donohue and Kitchener 1996). We will return to this point later as it is key to understanding the link between subject and object in our fieldwork a point drawn out elsewhere in this volume (Lönn, this volume).

First, however, it may be useful to look further at what it is to be 'comfortable'. Comfortable is defined in the Oxford English Dictionary as:

- Free from vexation or doubt (comfortable assumptions)
- Free from stress or tension (a comfortable routine)
- Applies to anything that encourages serenity, well-being, or complacency as well as physical ease (started feeling comfortable in our new surroundings)

All three of these definitions can be readily applied to archaeology, as one of the means by which we attempt to fit the evidence of the past into a world we can understand. Whether that be classifying a monument or making sense of a feature in the field. We are happy, then, when we feel comfortable, when our desires are satisfied, when things are going right. This is important, particular within commercial archaeology,



comfort makes things go faster and ultimately the practitioner and their client feel happy too.

Whereas Baines and Brophy (2006a, b) became uncomfortable when excavating an unfamiliar kind of site we suggest that it is within developer-led fieldwork where these tensions are most visible; the tensions we encounter, where we strive to bring the unfamiliar under control within budget restricted excavation. While writing this paper between 2009 and 2010, commercial archaeology in the UK has gone through a huge economic crisis and further cuts are likely to follow. Never have we seen such increasingly tight budgets, with projects required to be completed sooner rather than later. By the practitioner making themselves comfortable, they can, in theory, do the job more quickly and easy.

The unfamiliar patterns of the past that we attempt to distinguish may provide a picture of the world. Still, this picture must always be *our* picture, dependent on, and conditioned by, our very sociality and our past experience (Lönn this volume). Any archaeologist will be familiar with an archaeological depiction of a trench, its very uniformity makes each one of us instantly recognize and easily interpret the image before us. This uniformity of how to interpret and present such a feature is taught to us from the start, on our first excavations. But by the sheer force of the regularity of that which is familiar, each plan or section produced presents the past in the same form and creates a static past. How can it be possible, with all that is possible, that the same form is repeated again and again? For example recent work by British Waterways have explored the way in which different narratives can be constructed and conveyed when the traditional archaeological image is transcended or embellished. By using British Waterways's archives, local schools have researched the history of the area and created large mosaic panels to display their results (Henry 2011). With this kind of work in mind the question remains: how does the openness of the past and the future get closed down and reduced to such a standardised form in the present? Perhaps it is in order to make the past comfortable that 'we frequently promote (usually unwittingly) linearity, similarity, and regularity' (Insoll 2007:85).

We are acutely aware that it is when the remains of the past force us off kilter that archaeologists become uncomfortable. We know this and we have both experienced it. This discomfort is enabled by a sense of wonder. Rather than just seeing the familiar, we wonder at the irregularity of its form which forces us from our norms. The norms of our recording system enables forms, such as this trench plan, to be repeated, again and again, until they are 'forgotten' and simply become forms of life, and habitual practice. Manzo has argued that familiarity and habit are crucial elements of our embodied existence and the creation of our sense of place.

[...] regularity and routine are part of our way of being in the world, indeed we are not always conscious of our feelings for place. Moreover places that provide comfort and security tend to be places with which we are familiar, so we may be attached to them on an unconscious level.

(Manzo 2003:53)

Much of this is of course a product of the discipline and our desire to convince ourselves and our peers. This involves becoming a "part" of an institution.

Thus the discourse, the discipline, involves orientation devices that keep things in place. The effect of which could be described as a form of comfort. To be orientated, or to be at home in the world, is also to feel a certain comfort (Ahmed 2006:168). The word “comfort” suggests well-being and satisfaction, but it also suggests an ease and an uneasiness as we attempt to come to terms with the world around us. We learn to excavate and record archaeological features by rote – a site is created, transformed and interpreted by our methods. A situation that has elsewhere been described as ‘at-homeness’ the ‘[...] unnoticed, taken-for-granted situation of being comfortable in, and familiar with, the everyday world [the trench] in which one lives’ (Seamon 1979:70).

Often this is not as straightforward as we would like. These are the moments when we are confused, caught out, left adrift our methods have deserted us. When the past is “out of place,” it involves disorientation (Ahmed 2006:167–170). Of course this happens on all excavations but the point is that when confronted by a feature or a site that is ‘out of place’ we immediately fall back upon our standardised and comfortable methods to create the trench and the feature – and thus render the past knowable. When that dark ‘splodge’ is found beneath the machine bucket, thoughts flash through our minds; what could it be? A post-hole, a round house, or the remains of more recent farming practices like foot-and-mouth? As all this goes through our mind we immediately set about cleaning it back, planning, photographing and excavating in section or plan. This can be seen as a positive, and in many cases it is. These tensions lead to the creation of knowledge; the discomfort, as Ahmed says, albeit in a very different context, ‘allows things to move’ (Ahmed 2006:154). But the problem as we see it is that in many cases archaeologists are unwilling or unable to step out of their comfort zone. This can lead us to missing elements of the past and therefore we, as archaeologists, work within a methodology where we will always ‘find’ a ditch or a posthole, before we have even stuck the trowel in the ground. In this way ‘making the past comfortable’ can result in missing the actual engagement with the past.

## **Creature Comforts: Field Drains and Archaeology**

Field drains are a perfect example of the comfortability conundrum as they are instantly familiar, perhaps due to their abundance within the archaeological record in the UK. Due to their familiarity we think we know what they are, and despite being an archaeological feature they are routinely dismissed, un-interpreted and at worst unrecorded. Yet beyond this initial reaction field drains are able to offer huge amounts of information about land-use, agricultural ‘improvement’ and archaeological stratigraphy.

Field drains are commonly found in agricultural land throughout the British Isles. They are indicative of eighteenth and nineteenth century land improvements and an important social initiative, coupled with the rise of the agricultural revolution – the “expansion of land under cultivation and increase in yields per acre” (Williamson 1999:51).

Field drains transformed the British rural landscape, allowing areas of land to be cleared and cultivated or grazed for the first time. Early examples of drainage include bush drains – cut ditches filled with ‘faggots’ – offcuts from coppicing or hedge cutting (Williamson 1999:45). However archaeologists more often encounter ceramic drains within the archaeological record – sometimes early hand moulded horse-shoe shaped drains, but more often the later cylindrical clay drain pipes, produced by initiative machines presses (Douglas and Oglethorpe 1993:16). The mid-nineteenth century saw new approaches to the layout of drainage (Phillips 1999:53) and the introduction of machine pressed clay pipes which in turn increased productivity and output (Douglas and Oglethorpe 1993:16) making drainage cheaper and more affordable.

Sarah Tarlow and Susie West (Tarlow and West 1999; Tarlow 2007:10) have argued that much that is distinctive about later historical archaeology is masked by a superficial familiarity. Field drains are an excellent case in point. Not only are they ignored by many, much in the same way as a great deal of historical archaeology due to the wide spread belief that historical sources can tell us all we need to know (Tarlow 2007:29), but furthermore because at best field drains are seen as being of lower priority in a packed excavation trench and at worst they are seen as of little importance. Sarah Tarlow’s experience is of particular relevance:

As an inexperienced archaeology student I had a summer job on a project recording the archaeology of a beautiful valley in the south-west of England before it was flooded to create a new reservoir. We carefully planned and recorded the remains of walls, ditches and buildings. Once the topsoil was off, I remember being particularly struck by a grid of ditches evident across the whole area and dutifully began to add these to the plans I was drawing until the site supervisor noticed what I was doing and told me to leave off what were ‘only nineteenth-century field drains.

(Tarlow 2007: 59)

As Tarlow goes on to note, omitting such features was once standard practice but such omissions ignored a very interesting aspect of many sites (Tarlow 2007:60). Within developer-led archaeology field drains are routinely recorded as part of any archaeological intervention. However, field drains are largely absent from the interpretation of any given site. There has been little synthesis or other consideration of field drains in which to place site specific sequences making such considerations more difficult in an environment of intense time pressures such as developer-led archaeology. Yet there is a danger in this, it can become all too easy for the archaeologist in the field to fall into a comfort zone; drains are so familiar that they are instantly recognizable, we (think we) know what they are, what they tell us and can therefore be easily dismissed. We have met archaeologists across the UK who, when they note field drains at all, it is as a shorthand for the depth a trench will be excavated to. In this sense field drains are used to facilitate comfortability; when a site’s stratigraphy is difficult to read and it is unclear at what depth the first archaeological horizon or ‘natural’ appears, it can be very uncomfortable and disorientating. Yet, for some at least, the unearthing of a field drain can ease the situation and part of the processes that render the complex layers of soil in a definable area into a trench and hence comfortable.

As part of the research for this paper, we consulted local authority archaeologists within Scotland to ascertain their view on drainage, and more specifically clay pipe field drains – asking questions such as how often are field drains recorded and then added to the Historic Environment Record or Sites and Monuments Record? We also asked how they, as a curator, require drains to be ‘dealt with’ within development funded archaeology. One individual replied that when studying drainage, perhaps we need to think of them as indicative of material culture, and to ask questions such “whom, what, where and why?” While it could be argued that we, the authors, asking these very questions could have made the curator uncomfortable, thus forcing these structured answers, it is still an interesting response – the recognition of field drains as archaeological features in their own right.

Whether it is enforced or not, all those that replied believed that developer-led archaeology had not advanced our understanding of field drains, with one consultee noting that they are often recorded but never analyzed. Another consultee highlighted the varied nature of the information recorded or gathered on field drains within the Council’s register of sites and stated that most field archaeologists are unfamiliar with field drain typologies. With this lack of in-depth knowledge, it is hard to fully characterize them within the archaeological record. This consultation was useful as it further demonstrates how field drains are used in the interpretation of an archaeological site or landscape (in this case used as background information for the site) but yet they are still reduced to a clay pipe field drain considered to be of lesser or low importance. The assigned low or lesser importance to individual features can often result in the lack of stringent archaeological mitigation measures, being applied to the recovery or record of such a feature. With regard to field drains, we do not argue that they need to be considered in the same category as, for example, a prehistoric pit alignment, as the truth remains that they are abundant and are often not unique, but they still contain useful information if properly dated by knowledgeable archaeologists. The possible consequences of incorrectly dating a field drain are considerable. A horseshoe shaped hand pressed drain can 50–100 years earlier than machine pressed round drain, yet the individual archaeologist, could reduce those individual drains to ‘clay pipe field drain c. mid to late nineteenth century’ and miss an important nuance of that site.

Yet drains have social histories. It is a common experience in the UK to have worked on an excavation or evaluation and have been told tales of grandfathers or great grandfathers installing the drains in a particular wet summer. By placing field drains within a comfortable time horizon, are we then ignoring the social history of the field drain and as well as a phase of the individual landscape we are attempting to record? In a culture where Historic Landscape Assessment is now routinely used to aid how we interpret an archaeological landscape, or more importantly, how we put a value on that landscape, the development of field drainage is an important part of that discourse, not only to the actual landscape, but also to the social history it is attached to. The interpretation and dissemination of drainage and improvement can add to the cultural significance of that landscape.

Since starting this paper, we have both moved on from being ‘diggers out on site’. One of us now works as an outreach officer and the other has since worked as

a development control archaeologist for a Scottish council. We both found that even away from immediate confines of the trench, we have continued to encounter the consequences of comfortability. Even as curator, there is a reliance on the comfortable interpretation of a practitioner to clear planning conditions or to justify further archaeological work. This work often challenges curators to step out of their comfort zones – they are often questioned by planning officers to convince them (the planning authority) that further work is required even if there is little ‘hard evidence’ warranting this extra work to the developer. Even then the justification is often based on a known, known, where there is a large reliance on typologies and associated ‘importance’.

## Discussion: Are You Sitting Comfortably?

When we decided to use field drains to emphasize the archaeology of comfortability, and to consider how we deal with field drain archaeology, we realized that we needed to assess why we deal with the drains the way we do. Both of us have a background in developer-led archaeology, where standard approaches and methodologies are applied. Often it is a pressurized environment. We have to be comfortable and confident of our appraisal of the archaeological features in order to complete the work on time and get paid. Within this environment, field drains are frequently encountered when digging. They are noted during evaluations, watching briefs and excavations, but rarely are they considered in the same way as other archaeological features. Compared to, for example, a prehistoric pit, the humble drainpipe is commonly not considered ‘real’ archaeology. Sometimes they are considered an irritation, a disturbance of the real and more interesting event. On site, the linear nature of the feature will be noted, but they may not be fully excavated – they are assumed to be what they are. A typical trench report from an evaluation may read ‘Topsoil – 0.4 m depth, straight to natural. Three probable field drains running on a NE – SW alignment. No archaeology’.

It could be argued, then, that field drains should be recorded as a matter of course, as they can represent an important landscape development. However, important as point this may be we feel that there is a more fundamental issue that the example of field drains raises. What is at issue is the manner in which features deemed to be of low importance (such as field drains) are routinely dismissed or automatically fitted within a typology, perhaps to the detrimental result to the interpretation of that site. In this case, field drains are archaeology, yet they are relatively modern features and thus regarded as not important. Their abundance in the archaeological record makes them common, regular and easily recognisable. Their discovery can impact on the further understanding of the ‘site’ – providing a *terminus antiquum* “if the field drain cuts the ditch then ditch must be earlier than eighteenth or nineteenth century.” Field drains provide the archaeologist with reassurance – “it is ok, you totally understand what is going on!” but relying on them in such a way, do we get too settled in our comfort zone? As we all know, rarely is an excavation comfortable and straight forward.

We feel this is possible because we are too comfortable in our analysis of these features. We assume that it is what it is; a drain is a drain, and therefore no further interpretation or analysis is required. This assumption is the basis for being comfortable. With this interpretation we are so secure that sometimes we feel we do not even need to excavate it. It is invisible, not worthy of detailed explicit consideration, easily forgotten. This, we are suggesting, is a metaphor for wider archaeological field methodologies; methodologies that come about through the process of making the past comfortable. We do not doubt that field drains are often what we think they are going to be because we do not doubt our assumption. And we do not doubt our method because we strive to make the material remains of the past compatible with our methodology, practice, understanding and interpretation. One of the authors worked on the excavation of an 83 m long trench across a scheduled Roman Ditch, where this theory played out in practice.

We started out as three lone archaeologists with strict instructions not to 'f\*\*\* up' as the feature was Scheduled.<sup>1</sup> Three days in we still weren't sure if we had the ditch, deposits were merged, the cut of the ditch was still unclear. The field drains comforted us though – they were the first feature that we recognised archaeologically. They were a relief, as once we identified the drains, we were able to work out the other deposits, finally firmly placing the ditch within the stratigraphy and trench.

Due its scheduled nature, all features encountered within the trench were fully recorded. In this case the field drains were given a cut and fill number, fully excavated, planned and sections recorded. Plans and sections from that excavation demonstrated that what stands out are the field drains, yet due to their perceived lack of cultural significance, they were given the least regard.

It could be regarded as flippant to compare the ratio of field drains to roman ditches but the field drains are still important to the narrative of the site. In this example the field drains represented a later event to that of the roman ditch. By studying the stratigraphy and the placement of the field drain within it, the archaeologist can start to work where the Roman ditch fits into this 'known' time horizon. A Roman ditch was dug, it gradually silted up between 1700 and 1800 years and a field drain was then cut through the side of the ditch. The drains provided dating evidence and allowed the archaeologists to see what deposits had been cut through, providing reassurance on what was probably Roman. By relying on the drains to date certain deposits within the field, the archaeologists felt comfortable in their field interpretation of the fill deposits of the Roman ditch. The drains were recorded, but the emphasis was on their relationship with the Roman ditch. The drains were interpreted, but were interpreted in passing and not in their own right. Due to a value judgement, the ditch was given precedence.

In this instance and by following this set methodology we, as archaeologists, are ignoring that field drains are indicative of a significant change in landscape use, and their discovery and correct interpretation can add vast amounts of information on how the landscape 'containing' the scheduled Roman ditch was subsequently used. The focus is on the scheduled ditch (as stipulated by the Written Scheme of Investigation), the field drains recorded in reference to the ditch.

By comfortably 'fitting' features into known typologies, it eases the archaeologist into assessing, excavating, recording and interpreting the feature. However by being

comfortable, the archaeologist is also blinkered and prone to miss potentially important nuances within the trench. Current interests do not see field drains as worthy of note, but future archaeologists may curse us for not recording them fully. An extreme example of this is has been the huge loss of World War One and Two home front features such as air bases, pillboxes and control towers in the UK in the last 50 years. Once perceived as unimportant, their importance is now recognised and many of these features are now Scheduled or Listed, to protect them for the future.

Field drains are considered familiar, and therefore our methodologies can 'deal with them'. This allows us to comfortably conduct our role as the archaeologist. With field drains there is less risk of this strategy going wrong due to the uniform nature of the feature type. Perhaps it is because field drains do contain something (often a clay pipe) they are dismissed more readily. Perhaps if they contained nothing they would be elevated to a higher position of consideration. They would be creating an uncomfortable situation for the archaeologist – i.e. the unknown and, therefore, receive more attention. They would not be easily interpreted and thus discarded.

While this might seem like a long discourse on the virtues of field drains our point is a wider one – when we transfer this comfortability to a more complex site, significant problems can arise. For example a discovery of a possible prehistoric pit or a ditched enclosure, when uncovered and excavated are routinely placed within a known typology and automatically given a higher value, not only because they are often indicative of prehistoric features, but because prehistoric features are automatically given a higher value within our current paradigm. Compared with the field drains, these features cannot be assumed (usually they contain very little dating evidence) and therefore are often excavated far more thoroughly.

Whilst the importance of one (field drains) is left to the individual archaeologist, the importance of the other (a ditched enclosure) is never questioned. Fitting a ditched enclosure into a known typology, makes it familiar and perhaps easier to excavate, but this standard technique is also at risk from misinterpreting an archaeological feature, missing crucial information for reconstructing the past. Marianne Lönn (this volume) discusses similar constraints in her paper.

This 'typecasting' of an archaeological feature, makes the trench familiar and accessible and affects all aspects of the archaeological recovery. From the way we excavate it (if we put a slot here, the section will be clear to see); to the way we record it ("the profile of the cut appeared straight in section suggesting the feature was backfilled fairly soon after it had been formed or constructed"); to the post-excavation analysis ("one sherd of un-diagnostic Late Bronze Age pot was identified in the upper fill") to final report and interpretation of the site ("the ditched enclosure is most likely to be of X date due to the presence of the sherd, and the similarity between this ditch and several other X ditches of similar form and context"). This is obviously a simplified version of the process but it is a process which is repeated by archaeologists world-wide, and this standard methodology has made the trench (and the archaeological feature within it) accessible and familiar. We all do this in archaeology – in excavation, field survey and environmental analysis. This paper is not trying to say we do things wrong, but to question if we can do things differently, perhaps by making it less comfortable to deal with, by not automatically

imposing a typology on it, a value judgement will not automatically pass. We feel that comfortability breeds value judgements, value judgements which are used by the commercial archaeologist, the curator or even a student on a research excavation, to determine how much attention one feature gets. It is this implication, this assured value judgement, on which the serious consequence of comfortability becomes apparent. If we as archaeologists see something as ‘known’ and abundant, does it become of low importance? Often it does, and as a consequence less time and money spent on that feature. However, what if, on one occasion, in order to reduce the site into a known comfortable state, the archaeologist makes an incorrect value judgement? As a consequence important but now destroyed element of the trench could be lost.

How can we know start to confront the consequence of comfortability? We are not sure, but perhaps the first step is to be aware of the issue. This consideration of the ‘comfort’ issue has made us confront how we (the authors) record and assess archaeological features. Since starting this paper, one of the authors was out on a field survey and came across what seemed like a substantial concrete plinth, a modern feature considered to be of lesser or low importance and routinely dismissed from the record. But it seemed odd, this large plinth, so odd that the archaeologists decided to survey it in. Once surveyed and then assessed, it became clear it was giant arrow, associated with a WW2 practice bombing. By stepping out of the comfort zone and by not assuming that that feature was ‘just’ a concrete plinth, of lesser or low importance, not even worthy of recording, the arrow ‘received’ the correct level of attention and was added to the archaeological record. A value judgement was still passed, but perhaps it was a more considered one.

It is hard to suggest that we should do things differently at a time (when this paper went to press) with the world in economic crisis, and archaeology descending down the priority list. However we feel that we as archaeologists, by considering the consequence of comfortability out on site, now do archaeology differently, we now try not to discount a ‘possibility’ (of interpretation) or make an assumption before the ground has been broken. It is hard however to do archaeology without making value judgements, especially within a developer led context. These judgements are created through past experiences, through ‘comfortable’ knowledge. In all aspects of archaeology today, ‘comfortable’ value judgements drive forward archaeological interpretation and recovery. This is no surprise when our very methodologies, even our legislation encourages it. It is the surety [comfortability] that (within a commercial context) justifies developers to pay for it, or not to pay for it if it is deemed ‘not significant’.

## **Conclusion: Stepping into Comfortable Old Shoes: The Consequence of Comfortability**

As Baines and Brophy point out, by trying to hide and cover up our uneasiness, our un-comfortableness, with what the archaeology presents us with, we may be missing different ways of looking at things. This process, as we hope we have shown, starts



in the field, but also happens after the excavation. Post-excavation work can also provide constraints to interpretation. Specialist reports can provide answers to unsolved conundrums – they can provide a soothing tone to an uncomfortable state. Are there other pasts, we which cannot or will not understand?

As mentioned above, both authors have worked within developer-led archaeology and fully understand the regular pressures of trying to get a project finished on time. This paper is not about saying what we are doing is wrong – our methodologies today are obviously more rigorous than those employed 150 years ago and with the introduction of national planning policy in the UK in the mid-90s, the volume of archaeological data now being collected has increased. However, our yearning to be comfortable in what we are doing results in us perpetuating methodologies that render archaeology comfortable, leaving us at risk of missing elements of the past. Are we really that different from those antiquarians throwing animal bones out on the spoil heap?

However all is not lost. The tensions exposed through the consideration of comfortability, and related antonyms, allows us to reconsider our engagement with the archaeology. Just like Baines and Brophy (2006a,b) we all become uncomfortable when excavating or interpreting an unfamiliar kind of site and are confronted by tension when we strive to bring the unfamiliar under control. Where comfortability is recognized and reconsidered (such as when encountering a giant concrete plinth!), the constraints comfort can bring to interpretation can be overcome.

## Notes

1. This refers to a Scheduled Monument which is protected by law by one of the three regulatory bodies of the British mainland, Cadw (Wales), English Heritage and Historic Scotland. Excavations on a scheduled site requires specific permissions granted by these organizations.

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# Chapter 7

## Walking the Line Between Past and Present: 'Doing' Phenomenology on Historic Battlefields

John Carman and Patricia Carman

### Introduction

The purpose of the Bloody Meadows Project<sup>1</sup> (Carman and Carman 2006a, b, 2007, 2009) is to investigate historic battlefields of all periods, and we choose to do so from a broadly 'phenomenological' perspective. Our aim is specifically not to recreate what the battlefield was like on the day of battle or the events of that day, but rather to explore the historicity of particular kinds of places through the experience of being there.

Following Tilley (1994) and others, a phenomenological approach to the study of landscapes as taken by archaeologists has generally been limited to the monumental 'ritual' landscapes of European prehistory. The approach is, however, also of more general relevance to any encultured space, especially any marked as a particular kind of space. Tilley justifies taking a phenomenological approach to landscapes as follows:

[What] is clear [from the ethnographic record] is the symbolic, ancestral, and temporal significance of landscape [to peoples]. The landscape is continually being encultured, bringing things into meaning as part of a symbolic process by which human consciousness makes the physical reality of the natural environment into an intelligible and socialised form.

(Tilley 1994:67)

As he emphasises, the enculturation of landscape turns it from mere topography to a 'place': "Cultural markers [such as monuments or the memory of large-scale violence are used] to create a new sense of place.... An already encultured landscape becomes refashioned, its meanings now controlled by the imposition of [a new] cultural form" (Tilley 1994:208).

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The typical interpretive device in battlefield research is the battlefield plan (see e.g. English Heritage 1995), which is presented as an objective view from above, divorced from the action. But as Tilley also emphasises, place is not something that can be understood ‘objectively’:

Looking at the two-dimensional plane of the modern topographic map with sites [or artefact scatters] plotted on it, it is quite impossible to envisage the landscape in which these places are embedded. The representation fails, and cannot substitute for being there, being *in place*. [The] process of observation requires time and a feeling for the place.

(Tilley 1994:75)

The same is true of the traditional battlefield plan: it cannot substitute for direct experience nor for movement through the space. We therefore draw upon the ideas of prehistoric archaeologists who are developing ways of utilising the idea that the way of moving through particular kinds of spaces can be considered a form of ritual or performance (Carman 1999:242; Pearson 1998). For example, Barrett writes of prehistoric monuments in Wiltshire, England, that

for the distinctions [between people] to have operated... it was necessary for people to move between these [architectural] regions; to enter and leave each other’s presence, to observe passively or to act, to lead processions or to follow. The practice of social life is thus... performed.

(Barrett 1994:29)

Here, ritual activity is considered as a form of ‘acted out’ discourse (Barrett 1991:5; Thomas 1991:34), focusing on the physicality and (apparent) ‘objectivity’ of actions (Barrett 1991:4–6). Participants in rituals are guided through a series of specific meaningful actions, leading them to make the approved connections between them (Thomas 1991:34). Taking such ideas further, Thomas (1991) and Pearson (1998) argue that the focus of early Bronze Age ‘beaker’ burial ritual in Britain was on the body of the deceased. Objects which were put into the ground “constituted material signifiers whose role was to ensure that the intended reading of the dead person was made by the audience [at] the funeral” (Thomas 1991:34). Here, the space of the grave itself acted as the ‘stage’ of a theatrical performance (Pearson 1998:36–37). Mourners thus became active participants in the funerary ritual (Thomas 1991:39), players in the drama as well as spectators (Pearson 1998). The often slow and deliberate movements of bodies of troops across the space of a battlefield, frequently in defiance of common-sense, have obvious ritual connotations. The same is true of aspects such as drill, the proper use of equipment, standardised formations, and the focus on the capture of enemy standards and correctly worn regalia (Keeley 1996:62–63).

Putting these two styles of approach together, gaining a feeling for the place as a place and a focus on how one moves through it in performance, one can perhaps gain a specific sense of what a particular historic battlefield represents in terms of experience and meaning. The purpose of the Bloody Meadows project is thus not so much an attempt to recreate what an individual battlefield was like on the day of battle (or indeed the events of the battle). It is rather to establish a meaning for the historicity of the place in the present: hence our simultaneous concern both for

an understanding of the nature of war in the past and preservation and public interpretation in the present.

It is for these reasons that the Bloody Meadows Project looks very specifically at the *kind* of place where the battle was fought. The majority of archaeologists working on battlefields spend their time looking at the ground, trying to find the material left behind by the action. We instead spend time looking up and around us, at the shape of the space itself. A close focus on the shape of the space allows differences of choice across space and through time to become evident. In taking such an approach, and in being deliberately aware of both past and present in a particular place, we walk the line that lies between the past and the present, where neither dominates the other. Instead, they interact in interesting and challenging ways. It is not a search for an experience of being in the past, but rather an experience *in the present* which reflects and derives from the contribution of history to that place. In the case of a historic battlefield, it is not an experience of ancient slaughter, but an experience of a place in the present as read through its history as manifested in material form. This history inevitably includes the event of the battle that was fought there, but not exclusively.

## Investigating Battlefields as Historic Places

The primary data source used in the Bloody Meadows Project is the physical landscape of the place where warfare was practised. Drawing upon the work of previous scholars – who have identified the locations of many battlefields from the past – we focus upon the landscape itself to ask specific questions (Table 7.1). The answers can be ordered as set out in Table 7.2.

In Table 7.2, the *rules of war* cover such things as the degree of mutual agreement needed before fighting could commence, whether the two sides were required to see each other as ‘legitimate’ enemies or whether anyone could participate in a battle, some assessment of the level of violence employed, and how (if at all) the battlesite was remembered afterwards. They are a measure of how ‘formal’ battle was regarded and how distinctive from other forms of conflict at that time.

The *characteristics of the battlefield landscape* are addressed in order to identify features present in the battlespace and how they were used by combatants. This gives some insight into attitudes to the battlefield as a place. The query as to whether structured formations were present (such as ordered columns or lines of troops) gives a clue to how participants moved through the battlefield space: if the landscape is seen as architecture, so too can the forces engaged be seen as a kind of ‘mobile architecture’. The point is not merely to note those features present and used by combatants, as military historians might, but also and especially those features present but not used, and those present today but not on the day.

The two final sections attempt to summarise our expectations as filtered through an understanding of ‘good military practice’ derived from military writings (as in

**Table 7.1** Bloody Meadows Project: research questions

Of battlesites as historic places	Of battlesites as heritage
How clearly bounded is the battlefield space (does it have clear boundaries, such as impassable ground or a water obstacle)?	Was the battlefield subsequently marked by a monument or memorial in any way? If 'yes'
Is it located on high or low ground relative to the surrounding space?	Where is it in relation to the battlesite? What form does it take?
What kind of use (other than for war) was the site put to, if any?	Who or what does it commemorate? Who raised it?
Is it near or distant from settlement?	When was it raised?
Is it visible from settlement?	Is there any indication of the specific audience it is intended to address?
Does the ground contain particular types of landscape features – natural or built – which play a part in the battlefield action?	What does it say about the relations of commemorator or commemoratee to the battlesite?
What features present in the landscape (if any) played no part in the battlefield action?	Are links made with other sites or to other events? To what use(s) has the battlefield been put, and what is it used for today?

**Table 7.2** Parameters for studying battlefields

Rules of war	Battlefield architecture
Agreement to fight: Y/N	Features present
Mutual recognition as “legitimate” enemies: Y/N	Type of feature used
Level of violence: High, medium, low	Type of feature not used
Marking of battle-site	Use of terrain
	As cover
	To impede visibility
	To impede movement
Participants	Structured formations: Y/N
Functional aspects	
Dysfunctional aspects	

the concept of “inherent military probability” discussed by Keegan (1976:33–34)). It is, we believe, the *dysfunctional* behaviour (that is, the apparent mistakes or omissions) which can give a clue to cultural attitudes and expectations of the battlefield space which differ from our own. In applying this analysis to distinctive examples of warfare from various periods, the differences between periods become evident.

In approaching the landscapes that are our object, we use what we have called ‘the archaeologist’s eye’ – that is, the capacity of a trained landscape archaeologist to interpret space and to identify (especially manufactured) features in landscapes otherwise unfamiliar to them – to reach an understanding of the spaces of battle. By approaching such sites with a structured set of questions and by recording data in a standard format (Table 7.2) it becomes possible to recognise what such sites have in common and how they differ from one another. This in turn allows the identification of the types of location favoured as battlesites in particular periods

of history, and these can be related to other aspects of the battle as recorded by historians – including the type of participants, the nature of the conflict of which the battle is a part, and the flow of the action. Overall, it presents an opportunity to gain a direct insight into the ideological factors guiding warfare practice in that period and to compare them with those guiding warfare practice in a different period.

## **The Marking of Battlefields**

Many battlefields are marked by the erection of monuments of stone or concrete which are solid and enduring. Others are marked in other ways: by more ephemeral signs of memory, such as the names given to places and features, or local traditions which ascribe particular events to particular points in the landscape. Others again are marked by the actions of officialdom in recognising the site as of particular historical interest and importance: the traditional manner is to place an interpretive sign at a prominent viewpoint and perhaps to construct a circular walk or drive to visit the locations deemed important to an understanding of the events of the battle and its landscape context. Officialdom may also – as in the case of the *English Heritage Register* – mark the site out on a map, providing it with a convenient border and edge, allowing preservation and management within and less control without.

All such ways of marking battlesites – and others – indicate the way in which the site is perceived in the present; and to whom, and in what way, it is conceived to be important. The purpose of investigating these aspects of battlefields is to gain an insight into the contemporary meanings ascribed to such places. The purpose of combining such interests with research into the battlesite as a historic landscape in its own right is to relate the two: to find out if particular kinds of historic places are treated in one set of ways, while others are treated the same or differently, and to what extent, by whom and for what purpose.

### ***Battlefields as Cemeteries and Memorials***

The archaeological study of monuments to the dead and how war is commemorated is an area that has come to the fore in recent years (e.g. Borg 1991). Much of this has focussed on monuments to the wars of the last century – especially the First and Second World Wars (Winter 1995) and as part of the developing academic interest in collective social memory (e.g. Connerton 1989). According to some, the memorialisation of the dead of World War One was a process of de-personalisation at the service of a sense of national unity (Parker Pearson 1982): lists of named casualties gave way to monuments commemorating an anonymous ‘The Glorious Dead’ or simply ‘The Fallen’; and annual acts of remembrance denied the opportunity for a consideration of the experience or purpose of war (Bushaway 1992). On the other hand, it has been pointed out that the majority of war memorials constructed after World

War One “were initiatives which came from the people rather than the politicians.... [Their] erection was instigated by the bereaved public” (Tarlow 1999:162–163). They represented a response to the loss that had been suffered by large numbers of the population and who wished to find some way of marking and coming to terms with that loss. This emphasis on bereavement represents another strand to some recent archaeological work: a focus not only on the physical aspects of remains, but also on the emotional content of particular kinds of object (see also Tarlow 1999). In looking at monuments to the dead from this point of view, the question of who they were for comes particularly to the forefront and opens up a sensitivity to the meanings they carry.

### ***Battlefield Preservation***

The idea of preserving battlefields as important historic places is a relatively new one in Europe. The first *Register of Historic Battlefields* in Europe was produced in England, and has been subject to criticism for treating such places purely as historical phenomena, where the primary sources are written and where the location, its extent, and any physical evidence of conflict it may contain is of secondary concern (Foard 2001). These concerns have been partly addressed in recent Scottish historic environment policy (Historic Scotland 2009) where the archaeological potential of such sites is specifically addressed, and similar initiatives in Wales and Ireland are likely to follow suit. Nevertheless, in both the Scottish and English cases, the fact that a battlefield is included in the *Register* should be taken into account for development control purposes under relevant official guidance does not equate with full legal protection in the same manner as scheduled monuments and sites. In other parts of Europe, however, gaining official recognition for historic battlefields is a process barely begun.

This situation contrasts with the position in, for example, the USA, where battlefields considered worthy of note are taken into full legal protection and stewardship by responsible agencies under the aegis of the American Battlefield Protection Program of the Federal National Park Service (American Battlefield Protection Program 2009). A series of Federal laws relating to the preservation and protection of historic battlefields have been passed, primarily to provide funding programmes for suitable initiatives, and the most important battlesites of American history – especially those from the Revolutionary and Civil wars – have been taken into State care by the National Park Service under various designations.

### **Investigating Battlefields as Places in the Present**

The interest of the Bloody Meadows Project in the way battlefields are subsequently marked (whether soon after the battle or a considerable time later) is reflected particularly in the research questions (Table 7.1).



We are always fully conscious that marking a site is not the only measure of its importance or interest. Failure to mark a site can itself constitute a statement: sometimes this will be a representation of a lack of recognition of any importance or significance the site may carry for certain people, but other times a more positive omission with a purpose to it. By looking closely at such sites and the monuments and other marks they bear it is possible to come to an understanding of the meanings they carry in our own time, which can make a contribution to the study of collective memory (Connerton 1989; Foote 1997; Jones 2007).

These are also reflected in the purposes for which such sites are used. Battlefields from the past rarely offer much in the way of an obvious physical legacy. Where earthwork defences were constructed, or the fighting resulted in significant changes to the shape of the land, these traces may persist to become part of later uses. In those cases where archaeological investigation has been carried out, the archaeology has most often consisted of human remains buried at the site. More recent research has revealed the presence of scatters of material across the battlespace – most typically for battles of the firearms era, bullets and bullet casings (Haecker and Mauck 1997; Scott et al. 1989); for earlier periods, attachments to clothing which may have been torn off in the struggle (Sutherland 2001). Since such remains are generally invisible to the naked human eye, however, the landscape of such places has been seen as ‘empty’ of archaeology and therefore available for other uses. These uses may extend to the provision of park and amenity spaces, the historical significance of the location giving it an extra attractiveness to visitors. At Northampton, for instance, the space of the battlefield has been converted into the municipal golf course; at Quebec in Canada the site of the conflict of 1759 has been used as a site of recreation since the beginning of the twentieth century. Hence our reason for asking about the subsequent uses of the site up to the present. From this we can ascertain the various uses over time to which the space has been put – other than, or at least as well as, for war making – and from this gain some insight into the meanings the level of significance the place has acquired over time.

## Choosing Sites for Research

The focus of the Bloody Meadows Project is upon the older and perhaps less well known sites of violence in the past. We deliberately stop short of the twentieth century since a wide variety of research is already being conducted into the warfare of our own age (Saunders 2001; Schofield et al. 2002; Schofield 2005) and modern battlefields tend to be both very large and very well promoted. In addition, twentieth century warfare has disconcertingly extended from the surface of our globe into other realms: into the air; under the sea; into the most inhospitable regions of the world, such as mountain ranges, jungles, deserts, the arctic and antarctic; and even into outer space. It has also gone beyond the physical into more conceptual regions: into the relations of government to people (as opposed to being limited to the concerns of a specific ‘warrior’ caste); into the realm of science and technology; and, with the rise of the computer, into the so-called ‘infosphere’ and electronically-generated cyberspace (Carman 2002). The battles of our age can be said to have no

limits or boundaries: they frequently cannot be seen or measured, nor physically controlled. Unlike the warfare of previous ages, they do not occupy a particular location but are at once nowhere and everywhere. Their understanding thus lies beyond the methodology of this particular project, and we leave them to others with more appropriate styles of approach.

In general, the purpose of battle has been held to be the achievement of some kind of decision. However, as Weigley (1991) has pointed out, the battles of the era from the seventeenth to nineteenth centuries were for the most part *indecisive*. What we tend to remember are those battles that can be held to be in some sense to have resulted in a clear decision: for instance, by forcing a change of strategy upon one side in a conflict; by closing off a military or political option during the course of a conflict; or by bringing about the final defeat of one nation by another and thus an end – however temporarily sometimes – to a conflict. But the majority of battles do not achieve such decisiveness: instead they lead to more violence elsewhere at a later time. These battles, which are more readily and more easily forgotten, represent the majority of battles fought and the more typical form of battle in any period of history. What their study has to tell us is less about the outcome of wars and the political and social changes they engender than what war was generally like in that period and how the people involved perceived and understood the role of war in their lives. By focussing on such less spectacular and less historically significant events we gain a different kind of insight into war in the past than from much military history.

## **Standing in Empty Spaces**

Historic battlefields are locations where events once took place. They are now places marked by those events and accordingly of interest to students of those events. To study them is to stand in a place today, dreaming of an event of yesterday, an event that has passed and is gone. All one can do is stand and look, and that – put simply and bluntly – is the methodology of the Bloody Meadows Project. But there is more to looking than inactivity, and to look effectively one must also take note and respond to the images that present themselves. That too is part of the methodology of the Bloody Meadows Project.

For us, taking this approach means walking through the space with a keen eye to the different periods of history – and different human uses of the space – represented by buildings, monuments, street plans, different kinds of land-use, and different shapes of ground. The result is a kind of ‘time travel’ – not a one-way trip into a singular and particular past and back, but a real journey through various times, where different pasts and an immediate present are met in juxtaposition. Places have histories that are evident in the experiences of them, and it is in experiencing them as places that the histories become evident. The place has meaning because it has a history and that history is manifested in the material evidences of its past which testify to interesting and different pasts. These material things create the drama of the place which is the experience of its history in the present.

It is this historicity that such a ‘phenomenological’ approach to historic battlefields can produce. In taking such an approach, and in being deliberately aware of both past and present in a particular place, the line that lies between the past and the present is walked, where neither dominates the other. Instead, they interact in interesting and challenging ways, as illustrated in the following three examples.

### *St Albans, Hertfordshire, UK, 1455*

For the first battle at St Albans in 1455 King Henry VI gathered his forces in the centre of the town, where the wide main market street, as today, was suitable for the mustering of an army. The opposing army launched an attack that travelled up the narrow streets towards the centre of the town. Barricades were thrown down and the defenders retreated towards the town centre (Carman and Carman 2006b:97–100). Some of the buildings present today were those standing on the day of battle, and passing up these streets today, you still enter the town centre quite suddenly, going from quiet residential side-streets into the bustling market area. Nearby, a new shopping precinct overlies what were household garden plots on the day of the battle. Its internal arrangement reflects the narrow alleys that criss-crossed the area in the fifteenth century: efforts to negotiate one’s way around and out of this confusing space perhaps reflects the soldiers’ efforts to climb over fences and through hedges. In both streets and shopping precinct, the effect is somewhat similar to that likely to have been experienced on the day of battle (Fig. 7.1).



**Fig. 7.1** Northampton Street, St Albans, Hertfordshire, England. Site of a battle in 1455

### ***Roundway Down, Wiltshire, UK, 1643***

The landscape of Roundway Down is typical of its region in the southern part of Britain: rolling chalk downland with mostly gentle slopes although cut by steeper scarps. Roundway Hill itself is a rough isosceles triangle in shape with two long sides to north and south and the higher and broader eastern end immediately above the battlefield. To the south lies the town of Devizes, masked by a lower rise of ground, and linked by a route that climbs the steep southern slope of Roundway Hill. The land was mostly open grazing in the seventeenth century: the ploughed ground that makes up the rest of today's landscape is much more recent in origin. Roundway Down is today peaceful countryside: agricultural, tamed, gentle and empty. To see the battlefield you must walk around it and gaze at it from some distance away, for there is no right of way through it.

For 2 km above the eastern end of Roundway Hill the ground rises gently, but suddenly it falls almost sheer for 100 m: down this near vertical slope fleeing cavalry tumbled and fell, horse and rider, unable to stop or rein in. Walkers today going slowly on foot also come across it with frightening suddenness: one moment the ground is flat, the next it falls away into bottomlessness, hidden by trees. What it was like for fast-moving riders – the panicked screaming of horses and riders; the attempt to pull up only to be pushed on by those coming from behind; the fear, confusion and noise – can at least be guessed at when you are there. The bottom of the slope still bears the memory of the event: it is today called the Bloody Ditch (Fig. 7.2).



**Fig. 7.2** 'Bloody Ditch', Roundway Down, Wiltshire, England. Site of a battle in 1643

### *Corunna/Elviña, Galicia, Spain 1809*

The battle of Corunna – or as it is known locally, Elviña – was fought by a retreating British army before taking ship from Spain in 1809. The contending armies were formed on two parallel ridges about six kilometres south of the city, and most of the fighting took place on the slopes of the higher and steeper southern ridge on which the French army stood. The most fierce fighting took place in and around the village of Elviña, which changed hands several times. The vernacular stone buildings of the village still hug the steep slopes of the hill and the original core of the settlement remains much as it must have been on the day. From within the settlement, due to foreshortened lines of sight and impeding buildings, a sense of the surrounding landscape is difficult to grasp: little can be seen except the village itself. The modern Elviña church lies across the valley, providing a view of the main area where fighting took place. From here it is possible to gain a good view across the flat ground of the valley between the two main ridges and the arable fields occupying it. The small size of the fields and the vernacular buildings set amongst them indicate little change in this landscape since 1809; although on the hills above the encroachments are very clear of the expanding city and especially the new University (Fig. 7.3) which has been built specifically here because of the significance of the site in local historical memory.



**Fig. 7.3** The modern University at Elviña, Galicia, Spain. Site of the battle of Corunna 1809

## **Answering Some Criticisms of Phenomenology**

The three preceding examples are intended to emphasise that ours is not a search for an experience of being in the past, but rather an experience in the present which simultaneously reflects and derives from the contribution of history to a particular place. In the case of a historic battlefield, it is not an experience of ancient slaughter, but an experience of a particular place in the present as read through its history as manifested in material form. This history inevitably includes the event of the battle that was fought there – and indeed is what attracts us to explore that place – but not exclusively. It is that experience of being in the place that is captured.

We have been accused in one review of our work of going to places so we can ‘dream’ the past (Foard 2006). This is a common charge against phenomenologically-derived studies in archaeology. Trigger (2006:474–475) has referred to it as a ‘contemplative’ or ‘intuitive’ style of archaeology reliant on assumptions about a common ‘human nature’ and a shared bodily experience that crosses cultural boundaries. He points out that anthropology has “empirically demonstrated that cultural differences are sufficiently great as to make it unlikely that [phenomenological approaches] could control for ethnocentrism and produce reliable results” (Trigger 2006:474): he also accuses phenomenological approaches of relying upon subjective feeling (Trigger 2006:477).

By contrast, we believe that our work demonstrates the utility of an approach to landscapes based upon phenomenology: moreover, by applying this approach to historic landscapes we show the usefulness of the approach beyond the study of prehistory. Our approach is based entirely upon the notion that those attitudes towards and the expectations of landscape in the past were different from those held by people in the twenty-first century: if they were not different we would have nothing to say. We believe our approach seeks out and identifies those differences by using an explicitly modern Western mode of investigation of space and comparing it with the use of that space made by people in the past. It is from noting the manner in which space, objects and landscape features are used or any failure to use them as we would today that these differences emerge.

Where objects that were present in the past and would be available for use in the present – especially for military purposes, such as facilitating or impeding movement, for concealment or for protection – but were not used for these purposes, it can be inferred that the objects were not seen as useful. This in turn indicates a measure of difference between the past and the present. We also compare the uses of space in one historical period from those of another, revealing other differences in attitude and expectation.

### ***Trees and Buildings***

Woodland can offer a place to hide troops, may be an area to avoid or simply provide a source of raw material. In the medieval battlefields we have studied, the woodland

areas were avoided by troops and if present at all provided a boundary to the battlefield space. The trees themselves were sometimes a source for the material used to construct barricades protecting the defenders' position. The specifics of particular circumstances seem to determine the role of woodland in battle: as an inconvenience or an asset, as a landscape feature or as merely a number of individual trees. The manner in which woodland is treated by soldiers in different periods may indicate how such features are perceived more generally in that period: our work suggests that trees are more likely to be seen as woodland landscape features (that is, as woods or forests) in more recent periods, and more as sources of material (that is, merely a collection of individual trees) in earlier times. Either: but not both at the same time. There is scope for more research here.

Churches and chapels are a significant and common feature in any European landscape; accordingly their presence in the battlefield space may not be remarkable, and also as what is very often the largest stone structure in their area they may inevitably attract attention. Frequently, however, they were ancillary to the battlefield action, and specifically avoided by combatants during the fighting. Monasteries too frequently stand just off the space of medieval battles. The fighting avoided these places, but they provided rescue for the wounded and medical aid once the fighting was over. We think it significant that of our sample of medieval battlefields, almost all are known to be close to or involve churches and monasteries while less than half more modern sites do. Fighting penetrates only one such structure in the medieval period while a majority in the modern period are in the centre of the fighting. This suggests a change of attitude towards such places over time: that while churches and church foundations are not to be fought in or over in the medieval period, their presence nearby is desired or expected; while in later times they form merely another part of the battlefield space and no longer command special respect. Non-church buildings are relatively rare in the medieval battlefield landscape unless the battle takes place through urban space. The incorporation of settlements into battlefields in later periods increases the number of buildings present and such buildings are more likely to be used as part of the fighting, indicating another change of attitude to landscape features.

## Conclusion

We believe that by choosing to examine landscapes that were used for a very particular kind of purpose in the past makes the identification and examination of differences in attitude and expectation as revealed by differences in use more reliable, and that they therefore reveal real differences between various periods of history and those periods and our own. The differences in expectation and understanding of landscapes thus derived can then be taken up by others who are interested in understanding the use and attitudes towards space of people in the past. Elsewhere (Carman and Carman 2006a, b, 2007, 2009) we have given details of our results and more detailed consideration of what we think we have and have not achieved. Here we wish to emphasise that we believe those results to be meaningful,

and that taking our phenomenologically-derived approach to particular places of experience in the past demonstrates the value of such approaches. However, to do so is neither simple nor straightforward: it is a case of constant awareness of one's situatedness in the present while attempting to compare that with a known past.

Ours is not an effort to 'dream' a past, but to compare the past and the present – and different pasts with each other and with the present – in a meaningful way that opens up possibilities for understanding the difference of the past from the present. As Tilley puts it, it is about “being there, being *in place*” (Tilley 1994:75) in the present but being simultaneously aware of that place's past. Accordingly, our research is very largely not about the past at all, but about studying the past as a set of contemporary practices (Shanks and Tilley 1987a, b; Edgeworth 2006). We firmly locate ourselves in the present – and use knowledge of the past as a counterpoint to expose the peculiarities of the modern experience of space and place.

## Notes

1. The Bloody Meadows Project is co-directed by the authors and was instigated in 1998. It derives from their joint interests in landscapes as particular kinds of entity, in war as a subject of archaeological enquiry, and as an opportunity to use archaeology to contribute to significant debates of our time. The project studies battlefields from all historic periods and is not limited by geographical region.

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## Chapter 8

# The Struggle Within: Challenging the Subject/Object Relationship on a Shoestring

Members of the Ardnamurchan Transitions Project\*

*Reaching out for something you've got to feel while clutching to what you had thought was real.*

The above quote is taken from the Metallica song *The Struggle Within*, taken from their self-titled album. The song concerns a person who is suffering from a self-defeating personality, highlighting the difficulty of facing one's own demons (Lindholm 2007:68). In many ways this quote exemplifies both the potentials and pitfalls of the interpretative strategy being developed and implemented by the Ardnamurchan Transitions Project (subsequently ATP). This 'struggle within' is similar to Kierkegaard's claim that subjective truth is the most important truth (1992:343). Here, Kierkegaard is not rejecting objectivity, but making the point that attempting to live objectively makes a parody of lived experience and that people's true selves become mere shadows (Lindholm 2007:68; cf. Haraway 1991). For us Kierkegaard's claims are mirrored in the archaeological process, in the tensions between our need to fully record the archaeological deposits encountered, whilst simultaneously challenging our taken-for-granted assumptions concerning the nature of fieldwork. This conflict presents us with our own existential 'struggle within', one paralleled in the discussions of Kierkegaard and the music of Metallica.

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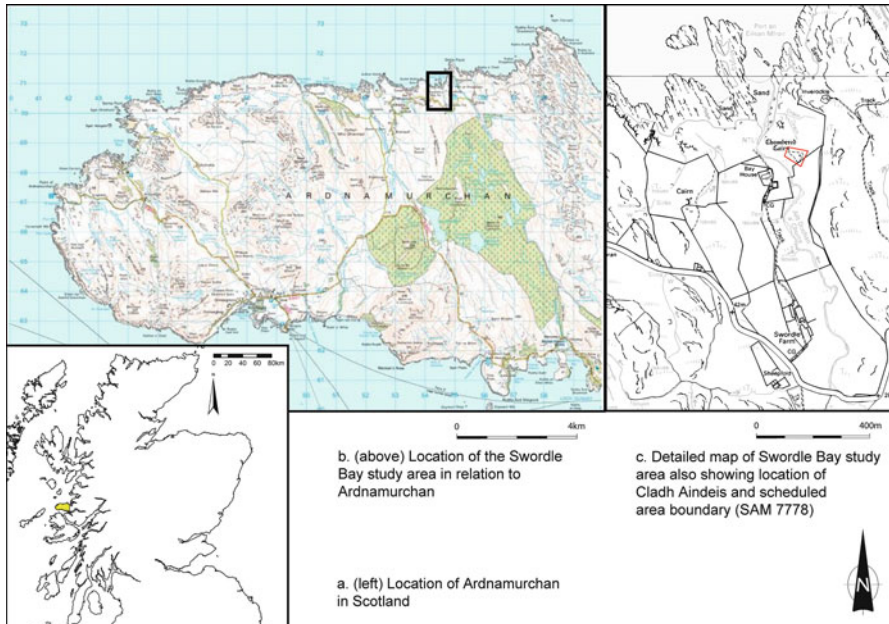
\*The ATP has emerged from the collective efforts of a wide range of students and researchers, too many to credit as joint authors. However, we list the most significant contributors to this paper here in alphabetical order: Hannah Cobb, Alistair Curtis, H  l  na Gray, Oliver Harris, Gemma Midlane, Paul Murtagh, Phil Richardson and Eleanor Rowley-Conwy.

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## Introduction

The practice of archaeology is firmly rooted within a particular way of seeing the world, one that divides everything into tightly bounded categories (Lucas 2001). Some things are viewed as natural, some as cultural, and this distinction lies at the heart of how archaeologists value the material they encounter (see also Cobb et al. this volume). Of course such a distinction is hardly unique to archaeological field practice and, as has been repeatedly noted, is in fact a central distinction of modern western thought (e.g. Latour 1993; Thomas 2004). Furthermore, it is just one of a series of distinctions inherent in Modernity which arise from Cartesian rationalism that also draw contrasts between male and female, ideal and material and subject and object. These distinctions also relate each of the former terms together and privilege them. Latour (1993) has described in detail how these acts of ‘purification’, as he calls them, disguises the proliferation of hybrids, of things that are both nature *and* culture, both subject *and* object, beneath the veneer of the ‘modern constitution’. In many ways archaeological practice is thus a modernist practice *par excellence* and authors like Edgeworth (2006) and Yarrow (2003) demonstrate just how these hybrids proliferate in our attempts to force the things we excavate into neatly bounded categories.<sup>1</sup>

Here we concentrate on the way archaeology dedicates itself to making divisions between subject and object, as this has been central to how archaeological practice has been developed. This distinction is usually made not between different parts of the ‘archaeological record’ but rather between the archaeologists (subjects) and the sites and materials (objects) (Yarrow 2003, 2008). As the material is viewed as radically separate from the processes that brought it into being (Barrett 2000 [1988]), it can be *objectively* seen to be out there, awaiting excavation. Our ability to successfully access this past rests not upon theory, on how we *think*, but rather on methodology – on what we *do* (or so the argument goes). The development of a successful methodology, it has been proposed, will allow us to record the past without bias and thus aid the second part of an archaeological approach: interpretation. This perspective, we suggest, is deeply flawed. Not only does it ignore the fact that interpretation goes *all-the-way-down* (Hodder 1997, 1999), it also imposes a set of understandings on to the material that separate it from the archaeologist excavating it. Our emphasis on purification denies ourselves access to the hybrids we in fact produce, the way a site produces the archaeologist as much as the archaeologist produces the site (Edgeworth 2006; Yarrow 2003). Nevertheless, given the central role of the subject/object dichotomy in the development of archaeological methodologies is it possible to imagine fieldwork, or even the profession, without this reliance on dialectics? The standards of single-context recording, objectified and distanced drawing and photography, and subsequent Data Structure Reports all act to (re)impose this distinction. Despite all the many changes to how archaeologists approach interpretation (highlighted during the theoretical upheavals of the 1980s and 1990s), fieldwork has followed its own historical trajectory (see Carver, and Thorpe this volume). Indeed, beyond the noteworthy attempts to provide new directions both in commercial and academic spheres (e.g. Andrews et al. 2000; Chadwick 1998, 2003; Hodder 2000)



**Fig. 8.1** Location of Ardnamurchan, the ATP study area and the site of Cladh Aindreis © Crown Copyright/database right 2010. An Ordnance Survey/EDINA supplied service

the classic distinction between subject and object remains rooted in archaeological method. What has been underplayed, as Thomas Yarrow (2003, 2008) has pointed out, is the manner in which this distinction emerges *from*, rather than prefigures, the work that we do.

In this chapter, we aim to explore the basis and results of a wide range of strategies employed during the on-going excavation of the multi-period landscape of Swordle Bay, on the Ardnamurchan Peninsula in western Scotland (Fig. 8.1). ATP is a survey and excavation project designed to investigate the transitions in lifeways on the peninsula through time, from the first Mesolithic and Neolithic occupants, to the most recent historical upheavals of the Highland Clearances (Cobb and Richardson 2007). The results of a series of surveys and excavations emerging from the project will be published in a series of reports in due course. However beyond this, the project seeks to question the subject/object dichotomy by challenging members of the team to fully appreciate the entirely interpretative nature of archaeological fieldwork. This requires us to face up to *our* struggle within, as we do so within an epistemology and set of power structures designed to keep that system in place but that also enabled us to become archaeologists in the first place. These power structures include the fact that the focus of our work from 2006 to 2010, the Neolithic chambered cairn of Cladh Aindreis, was a Scheduled Monument. Consequently Historic Scotland, the governmental organisation in charge of Scottish

heritage on behalf of the Scottish Parliament, protects and manages the site by law. We thus had to employ strategies that are able to satisfy the requirements and standards of Historic Scotland in order both to obtain and retain consent to excavate on the site. We were not, even if we wanted to be, in a position just to throw the rulebook out and start again. It is essential to note, however that these power structures, as we will discuss below, are not only constraining, but also enabling (Giddens 1984).

Despite this struggle, and building upon earlier projects, the ATP posits a radical archaeology, which places the observer in the foreground (Gren 2001:212), highlighting the reflective, plural nature of fieldwork (Cobb and Richardson 2009). This is essential because a crucial aspect of subverting modernist dichotomies lies in recognising the ways in which power structures on-site also construct certain people as active subjects (usually the directors) and others as obedient objects (diggers and students). It is not only the relationships with the archaeology, but also with the archaeologists, that we feel needs radical re-evaluation (cf. Lucas 2001:11–14). This chapter will show how these aims have thrown up more questions than were perhaps expected. As we begin to challenge our methods and categories, the potential for different pasts becomes apparent, yet the consequences of *Being Modern*, of working within the modern constitution, lurk in the background and need to be fully explored.

## **Struggling Within: The Ardnamurchan Transitions Project**

This chapter aims to explore how we may challenge the clear subject/object divide that traditional archaeological methods are founded upon. In doing so we reveal our own struggle within to undercut (following Webmoor and Witmore 2008) the multitude of rigid divisions that we draw between teaching archaeology and researching archaeology; between theory and practice; between academic archaeology and commercial archaeology; between skilled and unskilled; between subject and object; between being Modern and being-in-the-world. Yet in reality these rigid divisions are harder to define, much more fluid and less secure. The boundaries we create for ourselves are often simply arbitrary divisions which represent a characteristic of current archaeological *discourse*, rather than any true reflection of how we *do* archaeology in reality. Consequently in this chapter we suggest that we must strive to develop ways of undermining these artificial boundaries that divide us as archaeologists; something easier said than done.

We are attempting to address these issues in the ATP. At the heart of this project is an explicit recognition of the contingency of our work and the central importance of maintaining a constant, reflexive dialogue (Cobb and Richardson 2009). Reflexivity refers here to the way in which our results are never final, and are instead constantly being re-examined and reworked in the light of further work and thought, together with comments and suggestions from all members of the team (cf. Chadwick 2003:101–102). In this chapter, rather than discussing the results of the on-going excavations themselves, we will outline the strategies we have so far employed to

put such a reflexive outlook into practice, and to incorporate this reflexivity into the 'official record' of the site. This aims in part to capture the emergence of our bounded categories, the acts of purification that Latour identifies, and in so doing to similarly reveal the hybrids that lurk unconsidered and uncontested underneath. In this regard we are inspired by Yarrow's (2003) analysis of the emergence of subjects and objects in archaeological excavation, and indeed his analysis of the use of context sheets (Yarrow 2008).<sup>2</sup> What we seek to do, however, is not to recognise this separately, as an ethnographic point, but rather to capture this as it happens as *part of what we do as archaeologists*.

### *Aims of the Project*

Before turning to such strategies, however, it is worth considering the aims of the project, and in particular our intention to explore new ways of practicing archaeology. Specifically, the ATP aims to move away from the format of a traditional training excavation, and instead explore the potential of embodied archaeologies, where the experiences and encounters of *all* participants, human as well as non-human, in the excavation process are placed in the foreground (Gray et al. in prep).

Of course we are not alone in our aim to implement such this call for radical changes in both the teaching and wider undertaking of field practice (e.g. Berggren and Hodder 2003; Hodder 2000). In reality, however, many of the projects that have attempted to meet this aim have been able to put such reflexivity into practice due to the large-scale, well funded, nature of their work (Chadwick 2003; Cobb and Richardson 2009). In turn it is clear that such large projects are able to emphasise aspects such as digital methodologies (for recording reflexive observations), which unintentionally 'perpetuat[e] traditional hierarchies of power and representation' (Chadwick 2003:97). This manifests itself through the varied levels of control and access to both the resources and areas of the site. It illustrates clearly that *any* reflexive technique, from video recording to hand-written reflexive site diaries, can only be productive when excavators are empowered and recognised as decision makers and not merely data retrievers (Chadwick 2003; Edgeworth 2006). Such an observation highlights the extent to which we must recognise the value of core field skills as inextricable from reflexive theoretical approaches.

It is in building upon this kind of recognition, and the work of a series of recent projects that have endeavoured to tackle questions of reflexive field practice (e.g. Hodder 2000), that we have sought in our own project to explicitly integrate our reflexive practice concerns within a framework dedicated to providing rigorous, empowering, theoretically situated training for undergraduates (cf. Berggren and Hodder 2003). In contrast to other projects that have considered these issues we have both a very limited budget and consequently restricted resources, however we feel that it is simply not satisfactory to use the issue of budgets as a get-out clause for unsatisfactory training or interpretive methodologies. In fact we have explicitly aimed to develop affordable, realistic field practices that can be employed for little

cost but that ultimately seek to interrogate how we create the divisions that we have outlined here and to break these down. Integrated within this is the aim to develop a student-led ethos to teaching and learning, ensuring a bottom up approach to the project by giving students the kind of responsibility that both empowers them and enriches the continually reflexive exercise of excavation. This is necessary not only in order to make the students ‘good’ archaeologists, but also for the project to do ‘good’ archaeology. However, whilst such aims read well on paper, to put them in practice has so far required a holistic approach to how we excavate and our relationships both on and off site.

## **ATP in the Field**

Traditional methods of recording involve producing a written, drawn and photographic record. Over the last 20 years especially, the principal written record produced in British archaeology is the single unit ‘context’ sheet or context form (see MoLAS 1994; Roskams 2001; also see Thorpe this volume). A context may be a layer, floor, the fill of a feature, or any similar deposit that we might consider to be an archaeological event. The context form is also the primary record used during the post-excavation process for the writing of reports and publications. This is because they allow the archaeologist to record the physical attributes of an individual context (such as colour, compaction and depth) to describe the context in written prose, and to indicate the immediate physical and stratigraphic relationships that the context is believed to have had. This method is straightforward, and numerous manuals and training programs deal with these issues (e.g. Drewitt 1999; Roskams 2001). Following a growing number of commentators, however, we believe that this system is far from unproblematic (e.g. Chadwick 1998, 2003; Hodder 1999; Lucas 2001). The context form, by its very structure, forces a separation between what we may think of as being factual, and what can be considered interpretation; a dichotomy equivalent to the division discussed above between subject and object. The physical attributes of a context are considered as the facts that can be read directly from the material record whilst any interpretation, seen as less secure, is kept separate (Lucas 2001). The facts are preserved, therefore, whilst the interpretation is liable to change. That both emerge together is denied from these perspectives, the entanglements of subjects and objects are pulled apart; the results are purified.

Thus even when the contingent nature of excavation is acknowledged, the record we produce becomes fossilised and fixed (cf. Patrick 1985). This leads to a false objectification of the past, whereby the physical, embodied nature of excavation becomes suppressed, denying the reflexive and fluid nature of our engagements (Barrett 2001). On the other hand, we would argue that the processes of digging and recording archaeological deposits can never be separated from the act of interpreting a feature, context or site (Hodder 1997, 1999). They are interlinked, inseparable activities which have been falsely separated by the very recording strategies



we employ in order to understand the past (but see Thorpe this volume). Recording is not, then, a series of inevitable linear steps on a metaphorical objective ladder. It is our contention that the reflexive nature of the archaeologists' engagement with the past, in the present, is inadequately accounted for in current British context recording.

Following this growing critique these issues were of paramount concern with the conception of the project. Indeed our concern reflected the fact that the entire project staff have or do work in commercial archaeology in Britain. In these arenas a feeling of frustration with standard archaeological method was encountered by the staff on a daily basis. Given this, ATP provided an excellent forum to address these concerns by modifying the kinds of record we produced within the bounds of excavating a scheduled monument and all that entails (SHEP 2009).

### *Recording at ATP*

Let us then briefly look at the ATP context form. We are by no means the first to suggest that the context form, a standard part of British contract and research archaeology, needs to be modified (e.g. Chadwick 1998). Our context form is designed to meet the questions and needs of our own project however (see Fig. 8.2). It still relies on the recording of the physical nature of the context. Nevertheless, this is considered part of the interpretative process, prompting the excavator to consider how the context formed, what this might mean for human activity in the past, and how the context might fit into a wider feature or site interpretation. The back of the form (Fig. 8.3) also provides space to write down initial interpretation of what the context might be or represent, the interpretation of the context after half-sectioning, and what the interpretation was after full excavation. This aims to make the context sheet accurate, not only in regard to traditional Cartesian measurements but also to how our understandings emerge. This is an accuracy that 'rests upon the traceability of movements between the material world and what we say about it' (Witmore 2007:551), rather than a simple one-to-one relationship between 'facts on the ground' and their representation in the record (cf. Latour 1999: Chap. 2). Once a feature or area had been completed, excavators filled in an intervention form<sup>3</sup> (Fig. 8.4), which prompted further interpretation of what a particular feature or area might mean in relation to the rest of the site, and to human action in the past. Here, excavators also had to complete a matrix which depicted the stratigraphic, physical, and temporal relationships of each context within a feature. Thus a detailed picture of the context, the feature and the reflexive nature of the process of excavation is built up, which operates between the scales.

In addition to this, everyone on site filled in a participation form at the end of the day (see discussion below and Figs. 8.5 and 8.6), describing activities undertaken, the tools used, what each person felt their contribution to the project had been, and what they had learned or gained from the day's work. Of course it is feasible to argue (and we would not necessarily disagree) that this already happens

PROJECT	SITE	CONTEXT RECORD FORM Ardnamurchan Transitions Project	TRENCH	INITIAL + DATE	CONTEXT
EXCAVATOR	SUPERVISOR	DEPOSIT	ADD	SECTION	PLAN
FIELD INTERPRETATION			PROVISIONAL DATE		
CONTEXT DESCRIPTION		CONTINUES	FINDS		
COLOUR		CLARITY OF HORIZON	SAMPLES		
COMPOSITION		Stone size + shape	% of context		
COMPACTION		Stone % + sorting	Reason		
LENGTH		Charcoal %	Sieved?		
WIDTH/DIAM.		Roots	PHOTO (Slide)		
DEPTH/HEIGHT		Other inclusions	PHOTO (Digital)		
CONTEXT FORMATION (Taphonomy)		CONTEXT FORMATION (Anthropogenic)	HOW WAS THE CONTEXT FORMED? (intentional? The process/procedure)		
<b>Stratigraphical relationships</b>		<b>Physical relationships</b>	<b>WHAT DOES IT RELATE TO?</b> (what does this all mean? Look up from the deposit and consider why here, why now)		
		Overlies			
		Underlies			
		Fill of			
This context		Cuts			
		Cut by			
		Continues			
		Same as			
COORDINATES		Within			
		Abuts			
GRID REF		Abuted by			
WHY THIS KIND OF DEPOSIT (context narrative- what kinds of practice does it relate to? look out from the deposit and consider the story)					
CONFIDENCE IN EXCAVATION 1 ↔ 5			CONFIDENCE IN INTERPRETATION 1 ↔ 5		

Fig. 8.2 The ATP context sheet (front page)

PRE-EX: initial thoughts and chat. (complete following pre-ex plan).
HALF SECTION: consideration at this stage. (complete following section drawing).
POST-EX: concluding thoughts. (complete following post-ex plan).

Fig. 8.3 The ATP context sheet (back page)

PROJECT		Ardnamurchan Transitions project 'INTERVENTION' FORM		IG	
Contexts this relates to:				SG	
SITE	TRENCH	DATE	SUPERVISOR	EXCAVATOR	CONDITIONS
Feature Type:			Entity Type:		
<b>Intervention (excavation/sampling) Strategy Description:</b>					
<b>Portion excavated:</b>					
<b>Feature Description:</b>					
<b>Degree of preservation:</b> excellent                      good                      poor					
<b>Cause and amount of deterioration:</b>					
<b>Description of how feature was constructed and used:</b>					
<b>Description of modification/remodelling:</b> yes      no <b>Sealed:</b> yes      no					
<b>Associated features (and wider associations):</b>					
<b>Use History (interpretive vignette):</b>					
<b>Section and Plan numbers:</b>					
<b>Confidence in Narrative:</b> high                      high                      average                      low                      very low					
SKETCH SEQUENCE/PROCESS OVERLEAF					

Fig. 8.4 The ATP intervention form

PROJECT		Ardnamurchan Transitions project			Name: [REDACTED]	
PARTICIPANT FORM		PARTICIPANT FORM			Date: 3/7/07	
SITE	TRENCH	SUPERVISOR	EXCAVATION	OTHER	CONDITIONS	
AGE 2	2	DOLLE			Hot Hot Hot	
Feature investigated:			Entity investigated:			
<p><b>Intervention- what tasks did you perform today and who with and under what conditions?</b>          took SW corner down to sandy ledge - hot res, was the condition.</p> <p><b>What research question(s) were you attempting to answer?</b>          where the possible linear feature ends. (depth wise)</p> <p><b>Methods used and why?</b>          troweling because it is the bee's knees in the world of excavation</p> <p><b>What tools did you use and why?</b>          trowels and hand shovels - the Mr and Mrs smith of Archaeology</p> <p><b>Description of what you investigated:</b>          it was wet n' muddy, but the layers went from a dark, chocolate rich sand to a very dew, light sand.</p> <p><b>On the basis of the information recorded above, what activities do you think took place in the area you were working in?</b>          everyone was trying to find sand and the edge of the linear feature</p> <p><b>On the basis of the information recorded above, what do you feel you have contributed to the project and your own training needs?</b>          found a possible base and edge to the feature, and realised what we thought was an edge or proven previous days was not.</p> <p><small>Please continue overleaf if necessary- drawings welcome</small></p>						

Fig. 8.5 A participant form illustrating running student reflections on the nature of the ditch discussed above – the section most relevant to our argument is highlighted in red

in conventional field practice. Regardless of the system used, archaeologists always attempt interpretation, often involving plural dialogue, and are aware of the contingent nature of their work. Yet where our approach differs is in our attempt to build the reflexive nature of archaeology into the recording system. This approach to recording therefore means that reflexivity is not separated from the excavation in the form of removed site diaries, which Chadwick (2003) has rightly noted as a problem with other attempts to capture the contingency of interpretation. Of course

PROJECT		Ardsamarchan Transition project		Name: [REDACTED]	
ATP 07		PARTICIPANT FORM		Date: 01/07/07	
SITE	TRENCH	SUPERVISOR	EXCAVATION	OTHER	CONDITIONS
2	TP 6	HELENA			SUNNY/WET
Feature investigated:			Entity investigated:		
<p>Intervention- what tasks did you perform today and who with and under what conditions?                  Cleared the cobble layer from the bottom of the test pit down to the sand underneath. I did this with Ian in the sun and the pouring rain!</p> <p>What research question(s) were you attempting to answer?                  We were attempting to uncover the natural sand layer underneath the cobbles of an old storm beach</p> <p>Methods used and why?                  Troweling after removing cobbles because there wasn't much to take off.</p> <p>What tools did you use and why?                  Trowel, handshovel and bucket... to get rid of the soil.</p> <p>Description of what you investigated:                  We investigated an old storm beach but then discovered it may be the edge of the enclosure in front of the tomb -the trench is now being extended!</p> <p>On the basis of the information recorded above, what activities do you think took place in the area you were working in?                  I think at least part of the area would have been used as a surface for building fires/other things/activities that may have taken place in front of the tomb.</p> <p>On the basis of the information recorded above, what do you feel you have contributed to the project and your own training needs?                  Ian and I have discovered by accident (YAY) the edge of the enclosure. Genius. I now also know a lot more about geology due to my talk with the nice geology lecture man! (Hoop)</p>					

Fig. 8.6 A participant form illustrating a positive reflection upon the student’s contribution–the section most relevant to our argument is highlighted

the subject/object dichotomy has not vanished from this recording strategy, but is rather subverted, as the contingent nature of objective ‘facts’ is consistently and explicitly realised. This in effect captures both the way archaeological recording purifies the world into subjects and objects, and creates room for the hybrids, or perhaps better mixtures, which proliferate in our practice, to co-exist within the recording system, something Yarrow (2008:135) has called for.

## Facing the Struggle in Practice

In theory, if you forgive the pun, this alternative recording strategy seems straightforward. However, what we had hoped to achieve was much more difficult than first anticipated, provoking within us the kind of struggle that we began the paper with. In order to set out how these forms worked in practice, we will now turn to examine two examples and the successes and failures that our attempts to challenge the subject/object dichotomy met with during our 2007 season. This will begin by a reflexive study of what should be a relatively straightforward procedure: excavating a ditch. We will then examine how our approaches worked with regard to teaching and learning, and how we were able to draw on the students help to capture the contingent nature of interpretation, even when our own strategies let us down.

### *A Specific Example: Excavating a Ditch*

Our first example refers to the excavation of a single feature within Trench 1 at the Neolithic chambered cairn of Cladh Aindreis, which was the focus of our work in the first five seasons. This revealed precisely the tenacious ability of traditional archaeological strategies, utilised within the profession to turn up in practices typically undertaken on archaeological sites, including this one.

Trench 1 was situated in front and slightly to the left of the main chamber cairn. In the rear of the trench in season two – 2007 – under a layer of colluvium we discovered a ditch. Initially, as would be expected, the ditch was recognised in plan and we proceeded to excavate it in three sections with baulks left in between. One student dug each of these slots. A number of fills was discovered as we moved through the ditch. The bottom of the ditch was rich in charcoal. As students dug through the various fills they were assigned context numbers and each fill was sampled. Finds were recorded as coming from the context in which they had been discovered. These mostly consisted of flint debitage from the bottom layer of the ditch.

Once the ditch was excavated – with the exception of the three baulks – we began to record the various sections and fill in the context sheets. It was here that we began to realise we had made a mistake. The context sheets called for an initial interpretation to be recorded *before* excavation. The purpose of the box is clear, it makes explicit that understanding and interpretation is both contingent and part of the process of excavation, not something that gets tacked on at the end. We had known this – yet when we discovered the ditch we had not remembered to ensure the students immediately began to fill in the sheets. This did not mean interpretation had not begun of course. We can trace the ways in which the debate about the ditch developed, a classic three stage process familiar from all rites of passage (Van Gennep 1960). Separation from the narrative of the site: was it a ditch? No you do not get ditches by monuments like this, at most it is a natural hollow. Liminality: perhaps it is a ditch, we will consider it. Re-incorporation: ok it is a ditch, and we have secure finds from the base of it and all this evidence for burning. This pastiche of the discussion

reveals the way in which the notion of the objective recording of something discovered utterly hides the ways in which the feature, indeed the site, is created through debate and discussion. It was in this debate, as much as in the movement of trowels, that the ditch became part of the story of the site. And thanks to our careful preparations we had context sheets on which to preserve these ideas and this debate. Yet we had forgotten to use them. The recording took place once the sections had been dug, a classic example of purification, of imposing the subject/object dichotomy on to a fluid, dynamic and discursive process.

### *The Tenacity of Dichotomies*

Why was this? In writing this chapter we came to identify two reasons. Firstly it is tied into memory. Excavation is partially an act of bodily memory (Connerton 1989; Harris 2006), it is a way of dwelling on a site where actions are often guided not by explicit thought but by habit and expectation (Ingold 2000). The ditch was thrown up as the object of debate because it was not what we expected (cf. Casey 2000:146). But digging a ditch is a relatively straightforward thing to do, we had dug ditches before, we knew what to look for and expect: the application of previous knowledge, taught and observed, learnt and experienced, created the ditch. The students excavated it, but they did so in the way we would have done with the companies we work, or used to work, for. The excavation of fills, the assigning of context numbers, went as expected. In doing so it re-enforced and confirmed our memories. The ditch in many ways became the objectified expression of our memory.

The second reason relates both to the nature of the records we produce, and the power structures at play. Yarrow (2006) has pointed out how the objectivity of a site comes from the people who interpret it and the credibility they are seen as having. As a group of archaeologists in the early stages of our careers and from a wide range of archaeological career paths we have very little academic capital in this regard. We need to convince our peers of our findings. The pressure to do this was intensified by the fact we were digging a scheduled site, even though the ditch was outside the scheduled area. The records that we produced thus had to provide the information that would support the arguments that we needed to make, which would convince Historic Scotland and others of our arguments. The power structures of archaeology require sites to provide recordings that meet criteria created within the subject/object world. The sheets themselves were thus caught up in the power relations that surround us, they were part of the material culture that we as archaeologists produced, even when filled in by students. This maintained, rather than undermined, the role of the records as objectified 'facts' about the site, and increased the reluctance, unmentioned but present, to record before we knew what was going on. This is not to be critical of Historic Scotland, these power structures not only are necessary ethically (in that they offer legal protection to threatened monuments) they are also enabling. It was through our ability to work within these structures that we were able to excavate on the site, and to develop a satisfactory outcome. Nevertheless, the demands of

digging a scheduled site, or more precisely, our reaction to those demands, helped ensure the maintenance of the production of the subject/object dichotomy.

Does this experience mean we feel negatively about this aspect of our attempts to challenge the object/subject dichotomy on site? Fundamentally we failed to escape this most pervasive aspect of modernity. How could we, when both our permission to dig the site, and our nascent careers rested on our ability to provide an objectified account of the site to satisfy the power relations which we had *chosen* to submit ourselves to? Yet we feel very positive about it. Although the way we excavated on this occasion followed our unthinking patterns of memory it was seeing the sheets themselves that reminded us that we should have done it differently – our own material culture acting back (cf. Yarrow 2008:132). It is unlikely that, in the future, we will make the same mistakes because the sheets not only reminded us at the time, they also led to this chapter; they increased the reflexivity of the excavation. At the same time they provided and recorded all the information required to satisfy the demands of the most rigorous positivist. Furthermore, as we discuss in the final section of the paper, other procedures that we had introduced helped return some of the contingency of interpretation.

### *Teaching, Learning and the Struggle Within*

If we were only partly successful in employing our own methodologies in the excavation of a single ditch, what of our attempts to empower student learning and to increase reflexivity through the full participation of all members of the project? In this final section of this paper we will conclude with a brief consideration of the pedagogical outcomes of our work at the site and how the students themselves actually felt about their part in this project.

We argue that this is crucial to assess, because on training excavations there will always be a continual dialectical relationship between the responsibility to train and teach students and the ‘struggle within’ that we experience over the practices we engage with in the field, and their role in the production of knowledge. Thus, as we have outlined here, for the ATP, our excavation ethos is about actively valuing the excavator, and placing them at the forefront of the interpretation processes. This in turn demands a very different kind of power dynamic to the traditional hierarchical structure from dig director at the top, through supervisors, to diggers at the bottom. Yet, because those working on the project are predominantly students we found ourselves faced with a range of issues which once again made our aspirations to challenge such structures much harder to apply in practice.

For example, the notion of the empowered excavator can only emerge from the excavator themselves. Empowerment cannot be taught or enforced. Yet, inevitably, training excavations in Britain typically have a proportion of students who do not want to be there but have to attend an excavation fulfil a required fieldwork component of their course. Consequently such students are often not interested in engaging in the archaeological process at all, whether it is one which seeks to empower them



or not. Another issue is simply that on a training excavation students will inevitably be actively learning through instruction. Such instruction will always create the kind of power relationships and structures that our project explicitly tries to undercut. Consequently we are faced with some fundamental struggles in this regard.

To address these issues, then, we tried to do a variety of things. Foundational to our training provision is a firmly held belief that students should learn by engaging practically with every aspect of the excavation process. As a result, we tried to go beyond making sure students just ‘got a go’ at doing everything. In our 2007 season each of the 16 students, no matter what their level of prior experience, were given responsibility for their actions and interpretations. In practical terms this meant that students took certain areas of the main trench and test pits, and followed them through from opening the area to backfilling it.

As we have already discussed, another key aspect of this project has been to develop a recording strategy that goes beyond the traditional, objectified interpretation of a site generated only by the dig directors. Instead the kind of reflexive, multivocal recording methods that are outlined above were instrumental not only in enabling students to play an integral role in interpreting the site, but by doing so this also enabled the active challenge of traditional hierarchical power structures. This process was particularly emphasised in the Participant Form which everyone on the excavation completed at the end of each day. The role of these forms was to enable people to reflect on a range of things, from the fluid and changing nature of interpretation to their own contributions to the project and their own personal training and development.

Inevitably there were some problems with these forms. Perhaps the most predominant was students often writing what they thought we would like to hear. This perpetuated exactly the kind of problematic power structures we wanted to undercut. Another problem was that we intend to make the completed participant forms freely available online as part of the site archive, and this obviously meant that people were reticent to express their real experiences in some cases. However despite these occasional problems, the forms were nonetheless highly successful. For example Fig. 8.5 is a form completed by a student who was involved in excavating the ditch we have discussed above. What this demonstrates is that whilst the context sheets might not have captured early interpretations of the ditch until late in the excavation process, some of the fluid and changing interpretations of the site are recorded here. This second level of recording, focusing on the students, helped to capture the contingency of interpretation. However, perhaps of even greater importance in this is example is that not only does this demonstrate the reflexivity we strive for being reached in practice, but critically it shows that the students were integral to this reflexivity, and to generating these interpretations, and they were aware of this:

*What do you feel you have contributed to the project and your own training needs? “Found a possible base and edge to the feature and realised what we thought was an edge on previous days was not” (Rob, 28/06/07).*

This demonstrates one of the key strengths of these forms; that they enabled students and staff to reflect on what they had done and their own contributions to the wider picture.

Moreover the explicit reflection upon this by the students was core to their own empowerment, and caused them to feel immediately positive about their contribution. This is demonstrated well in Fig. 8.6 and particularly in her final response:

*What do you feel you have contributed to the project and your own training needs? “Ian and I have discovered by accident (Yay) the edge of the enclosure. GENIUS. I now also know a lot more about geology due to my talk with the nice geology lecture man! (Whoop)” (Natalie, 01/07/07).*

## Conclusion

The comments from Natalie, above, provide a positive note on which to bring the chapter to a close. Of course we are aware that this has been a very brief introduction to a range of ways that we, as a project, have attempted to address our own ‘struggle within’. This is a struggle that faces up to challenging dichotomies that our practices themselves impose, and empowering students democratically through unequal power relations. Clearly many of the aspects that we have reviewed have some problems, and this illustrates the difficulty of trying to employ this kind of ethos of plurality and reflexivity in practice. We are also aware we have not yet begun to outline how we will deal with issues of reflexivity, multivocality and participation as the project moves towards final publication. Nonetheless, what we hope we have illustrated is how, in practical terms, on a medium sized but low budget project, we can get close to forms of reflexive recording and empowering excavation whilst at the same time still fully recording the archaeological deposits to meet conventional standards, and fulfilling traditional training requirements. As we have illustrated here, this is still very much a work in progress and there are inevitably aspects of the project that still need work. Crucially, however, it seems that even in its early stages the kind of ethos we have attempted to put into action at Cladh Aindreis may provide a way to radically challenge the problems of traditional archaeological practice by undermining the subject/object dichotomy, by provoking members of the team to fully appreciate the entirely interpretative nature of archaeological fieldwork, and by enabling us all to face up to the struggle within.

## Notes

1. Latour’s thought is growing increasingly influential across the social sciences. For a more detailed discussion of its implications for archaeology see the work of those archaeologists writing from an explicitly symmetrical perspective (e.g. Olsen 2003; Shanks 2007; Webmoor 2007; Webmoor and Witmore 2008; Witmore 2007).

2. Though this latter paper (Yarrow 2008) was published after we had developed the interpretive strategies outlined here, and indeed written the majority of the paper.
3. This particular sheet was inspired by those used in the Framework project. Although none of us had actually seen what one of the Framework sheets looked like, we were able to positively adapt the ethos of these larger projects into our own strategies.

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# Chapter 9

## Through Many Eyes: A Non-hierarchical Approach to Interpreting a Site in New Brunswick, New Jersey

Rebecca Yamin

### Introduction

In 2006 I asked key members of my field team to write narrative vignettes based on the archaeological investigation we had conducted on eight historic lots in New Brunswick, New Jersey (Fig. 9.1). The excavation was done in compliance with Sect. 106 of the National Historic Preservation Act and the client was an engineering firm under contract to the New Jersey Department of Transportation.<sup>1</sup> In other words, it was your standard CRM (Cultural Resource Management) project done within the usual constraints of time and money as well as explicit excavation and reporting requirements. Four of the lots faced Albany Street, the main street in New Brunswick and the focus of its earliest development; the other four lots faced Water Street, which ran parallel to the Raritan River and eventually to the Delaware and Raritan Canal which was built between the street and the river in the 1830s (Fig. 9.2). A complicated traffic interchange now covers the site.

We did not find the eighteenth-century deposits we had hoped for. The Indian Queen Tavern where Benjamin Franklin, Edward Rutledge, and John Adams stopped on their way to a meeting with Lord Howe to prevent the Revolutionary War was within the site boundaries and there were two more early taverns on the block.<sup>2</sup> Most of the archaeological deposits, however, dated to the nineteenth century. The earliest stratum in the stone-lined privy behind the Indian Queen, for instance, yielded fragments of blue and black transfer-printed teasetts and lots of tumblers and the most recent stratum in the same privy yielded white granite teawares as well as tumblers, apparently the vessel of choice in the tavern that operated on the property up into the twentieth century (Yamin et al. 2006: 11–34). The contents of

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**Fig. 9.1** Overhead view of route 18/27 interchange site, New Brunswick, New Jersey

the privy on the property next door – the site of one of the other early taverns – related instead to the long-term resident Van Dyke family, members of which lived there from 1791 to 1868. Artifacts from a privy on that property, too, dated mainly to the nineteenth century.

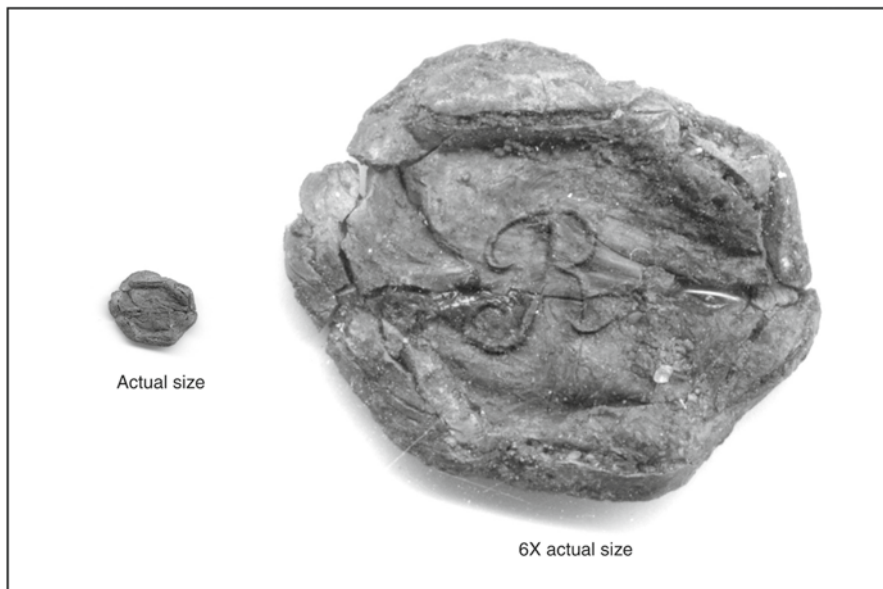
The most unusual find on the project was not found in the ground. A handwritten diary kept by 17-year-old Rachel Van Dyke between May of 1810 and July of 1811 is in the Special Collections at Rutgers University and Tod Benedict, one of the field directors for the archaeological project, ran across it while doing standard map research after the excavation was completed. The diary, which had recently been transcribed and published by historians Lucia McMahon and Deborah Schriver (2000), is an extraordinary document. Rachel brings to life the very block we got to excavate almost 200 years later. When she began the diary she had already graduated from Miss Hay's Boarding School and was just leaving the Female Academy in New Brunswick. Her education, however, was hardly over. She continued the serious study of Virgil in Latin; she studied chemistry under the tutelage of her brother, who was studying to be a doctor with Benjamin Rush in Philadelphia. Inspired by a friendship with her former Latin teacher, Ebenezer Grosvenor, whom she calls Mr. G., she read widely, struggling self-consciously to avoid novels which she considered a waste of time. Rachel and Mr. G exchanged journals in what was, according to McMahon's essay at the end of the published diary, a conventional part of courtship and when Mr. G. went home to Connecticut to resume the study of law, they



Fig. 9.2 Sanborn insurance survey, 1886, showing route 18/27 interchange site bounded by Albany street on the south, Water street on the east, Washington street on the north, and peace street on the west

exchanged monthly letters. The wax seal with an “R” that we found at the bottom of the family privy (Fig. 9.3) may well have been identical to the seal she used on those letters.

The Van Dyke household basically consisted of Rachel’s mother and father, neither of whom she talks about very much, her sister, Lydia, and several apparently African-American (probably enslaved) servants. The servants mentioned in the



**Fig. 9.3** Wax seal with an impressed “R” that Rachel Van Dyke probably used on her letters. Recovered from the bottom stratum in feature F on the route 18/27 interchange site

journal include Pompey, Sylvia, Edward, Jenny, and Adam. Relatives including her grandfather (mother’s father), aunt (mother’s sister), and cousin were fairly regular visitors, however, and Betsy, the cousin, appears to have been living in the household and studying under Rachel’s direction for a good part of the year. Rachel’s brothers – Augustus, the medical student, James, a future entrepreneur who was working in New York City during the year of the journal, and John, mentally ill and living in Monmouth County – were away from home. Rachel looked forward to Augustus’s visits. He was clearly her favorite and, like Mr. G., he had a strong influence on her education. Rachel liked to sit in the window of Augustus’s room “from which there [was] a fine prospect of the river and the opposite bank” and a view of “little boats gliding over the smooth surface of the water...” (McMahon and Schriver 2000: 28).

Details more pertinent to archaeology in the journal include a description of the “small” yard where Rachel and her sister planted things. On July 4th she mentions “our little calf baaing” and in June of 1811 there were baby chickens which she held in her hand “to stroke their silken feathers” (McMahon and Schriver 2000: 292). Mentioned outbuildings include a stable with horses (June 10, 1810), and we know the family had a sleigh because they go riding in it. The Van Dykes owned a farm about a mile and a half out of town. Peaches (Rachel’s favorite food) were grown at the farm and also strawberries. Frederick Van Dyke supported his family with a “dry goods business” run out of the house (though never mentioned by Rachel) and the farm. Dry goods probably meant grain in this case as Rachel goes shopping for



the kinds of things we now think of as dry goods: to the milliners for a hat, for instance, and at another time for the needed fixings for a new hat, new “frocks”, flower pots, and to the shoemaker’s.

There are tidbits about food – a breakfast of “mush and milk” on May 28, 1810 and just coffee on November 22. The family appears to have eaten before eight although Rachel sometimes got there too late and ate alone. Dinner was over by three and tea was served in the evening. Guests came to tea, which always included cake of some kind. Rachel was often entrusted with baking the cake and sometimes with overseeing the household in her mother’s absence. A particularly relished cake was plum “nearly full of citron,” and pumpkin pies are mentioned more than once. There are a few references to supper although it does not seem to have been a formal meal. One entry describes eating a particularly satisfying supper of peaches. Rachel does not say anything about the dishes on which meals were served, how the table was set, or even much about the food, but it must have been characteristically “Dutch” because the “round of beef” served at Ross Hall where a friend lived made her “secretly smile and remember that I was eating at an Englishman’s table” (McMahon and Schriver 2000: 103).

Rachel’s story made us, or at least me, want to continue the narrative, to use the archaeological investigation to tell the rest of the story of the block. I had used something I call “narrative vignettes” on the Five Points project in New York City to bring that neighborhood to life and I thought we could do the same in New Brunswick, but I did not want to do it alone (Yamin 1998, 2000, 2001). The process of writing vignettes is a process of discovering what you know and, importantly, what you still do not know and I wanted team members besides me to have the experience. I also thought perspectives other than my own would add to the interpretation of the site, an idea that met with some resistance.

Cultural Resource Management (CRM) as practiced in the United States is, like pretty much every other business, inherently hierarchical. The people at the top do the decision making and the people at the bottom do the manual labor. The people in the middle, the ones who run the field projects and generally write up the excavations, are not expected to do much more than describe what was found and, of course, how it was found, the familiar methods and results sections of a CRM report. Interpretation is generally done by the principal investigator (the P.I.) with input from field personnel to the extent possible and from impressions gained on field visits in combination with the results of the historical research and whatever theoretical perspective the P.I. deems appropriate. Section 106 curiously requires the formulation of research questions before full scale excavation (what we refer to as Phase III or data recovery) which constrains interpretation to some degree although there is room to drop questions for which there are no data and add questions that are more appropriate once the data have been analyzed.

There is comfort in this system. The managers worry about the money and keeping the client happy. The P.I.s claim the interpretive role (and get to give the papers at professional meetings and, hopefully, publish), making it seem alright to not be working in academia, and the field directors do not have to think very hard, at least after they have solved the logistical problems involved in actually doing the excavation.

The excavators are not expected to contribute to the final product. They fill out prepared field forms that include space for comments and interpretation which may or may not be taken into account by the person who writes up the excavation. This is in spite of the fact that it is the excavators who are the closest to the data during its recovery. They are the ones who are overheard on a site speculating what something might or might not mean – “It might be a French drain; no it’s probably just a slump in the yard.” “It looks like the edge of a garden bed or maybe it’s the top of a privy.” “The number of bottles sure look like we have a tavern or do you think it’s the community bottle dump?” Such speculation is, of course, part and parcel of figuring a site out. Ian Hodder has called it a necessity. “Having a story in mind,” he wrote in *The Archaeological Process*, “is necessary to help us think while we dig, think of alternatives” (Hodder 1999: 55). Those thoughts, however, more often than not get lost between the field and the office. They are unremembered almost as soon as they are uttered and whether right or wrong, their absence deprives the final report of the process of finding the past. A basic tenet of an interpretive approach is that the past is not there to be dug up like a natural resource, it has to be constructed and a lot of the constructing goes on in the field (Shanks and Hodder 1995). To get to the past through archaeology is “another way of knowing” and it is that other way of knowing that I am interested in capturing in the report.

## Another Way of Knowing

Long before he published *The Archaeological Process* Hodder bemoaned the state of archaeological reporting. In an article entitled, Writing archaeology: Site Reports in Context (Hodder 1989) he compares a letter report that appeared in Vol. 2 of *Archaeologia* in 1773 with an excerpt from a typical report written in the 1970s. The first is “fixed in time and place” (Hodder 1989: 269), it uses the “first person pronoun” (269), and it “involves named actors and their intentions” (268) while the second is written as if the finds are “self-evident”, beyond history and beyond the influence of the investigator (271). Much has been written about introducing a more reflexive approach to interpreting archaeology and it need not be repeated here, but Hodder makes several points in his 1989 article that are not often part of the argument. One is that

the site report could be written as a complex interweaving of sequences of events in the past [what happened on the site] and sequences of events in the present [what happened on the excavation]. Most excavations have their dramas, their problems unsolved. The text would permit uncertainty and unresolved doubts and would narrate a truer picture of what had passed. Dialogue would draw on the relationships between co-directors, between supervisors, between diggers, between specialists, so as to provide a flavor of debate and argument about the site.

(Hodder 1989: 273)

He claims to have instituted a framework on the Catalhöyük project that would actually allow this approach to interpretation (Hodder 2000), but how successful it has turned out to be is unclear.

Hodder's second unappreciated point is that those old contingent reports were exciting. There was

a tension as one follows the process of discovery. This excitement is enhanced by the use of personal pronouns and actor oriented accounts....The understanding of the site is tied to the process of excavation itself. Hypotheses are rejected or adopted as the excavation continues. The meaning of the site is linked to the circumstances of recovery.

(Hodder 1989: 270)

Implicit in this statement is that reports we generally produce are not exciting. In fact, they "are dull, excessively long, detailed and expensive and read by no one except the delirious specialist" (Hodder 1989: 273) and, of course, in the case of CRM in the United States, the poor reviewer assigned to the task at the State Historic Preservation Office (SHPO).

Our reports are, indeed, notoriously dull, but even more serious is the fact that they so rarely convey anything worth knowing. Although we feel we are learning something important when we are in the field we manage to eliminate whatever it was during the analysis. It is as if that "other way of knowing" that we experience cannot be translated. There is undeniably a need to include the archaeological data in a report and because, in the case of urban projects, at least, there is so much data there is no way to avoid the kind of quantitative (tables, profiles, plan views, etc.) presentation that is considered boring. There is also a need, however, to interpret the data in a way that does more than respond to pre-set research questions that have little interest beyond the profession, e.g. how do ceramic choices reflect (and create) socioeconomic status, or buying power, or trade networks? How do clay pipes express political allegiances, or ethnic boundaries, or class conflict? Those are the kinds of questions that the P.I., who is presumably familiar with the literature, will address, but that is not the most interesting thing a historic site produces. It produces another way of knowing the past and there is every reason to believe that any and all of the participants in an excavation are equally qualified to interpret the data that pertains to the piece of the past they have uncovered.

Just as Hodder would have co-directors, supervisors, and diggers weigh in on the interpretation of a site, I would have anyone who worked on a site that was willing to incorporate what they thought the various artifact deposits combined with the historical record meant into narrative vignettes. My colleagues in New Brunswick were not enthusiastic. I suspect they were uncomfortable with such a seemingly un-scientific approach (counting up glass fragments seems more scientific, I guess), but pressed into action they surprised themselves. Just as Henry Glassie says at the beginning of *Passing the Time at Ballymenone* (1982) and I have many times experienced, writing is a way of discovering what you know and that is just what happened to Tod and Alex and Juliette. They interwove the artifacts and bare historical facts (census data, directory and map information) plus a little secondary research into vignettes that tell the story of the block we excavated after Rachel Van

Dyke lived there. The authors brought to the writing an intimate knowledge of the features they had personally excavated and, in the case of Juliette (the laboratory director), the artifact assemblage she liked best. I organized the vignettes chronologically and included them as the last chapter of the report, entitled "Once Upon a Time in New Brunswick." A representative (and chronological) sample follows.

### ***With Flooding in Mind, by Alex Bartlett***

The year was 1797. William Forman had owned the lot at 12 Water Street and the old house that stood on it for some time now, and he finally had the time and money to do something with them. He hated the house. It was old, weathered, and flimsy and he needed something better for his growing family. Six children were born to William and his wife Eleanor (nee Pool) in the 1790s. The old place would have to come down. He demolished the house and built a much larger, solid structure, of the finest wood and local shale. Because he had seen the Raritan overflow its banks, William designed an elaborate drainage system in the cellar of his new structure. He canted its floor down to the east to allow any water emptying into the cellar to return to the river by way of a pipe. The floor itself would have to drain as well and he took care of this problem by laying a floor of river cobbles meticulously embedded in sand, except at the south end which he covered with brick. That would be the work room, where he could wash his tools and have the water drain off. William was proud of himself; the new structure would serve his family well. Little did he know that 50 years later the very same house would be bursting with the workers who eventually displaced merchants like Forman and Taylor on the block. The workers found employment in the factories that lined the Delaware and Raritan Canal almost as soon as it was completed in 1833 and William Forman's substantial house was subdivided into apartments to house their burgeoning families.

### ***The Spinsters on Albany Street, by Tod Benedict***

It was July 4, 1851, a Friday, and Lydia Van Dyke, now 64 years old, was sitting in her parlor in the late afternoon. She looked out the window and watched with amusement as one of the hired boys next door at the Bell Hotel tried to corral three chickens that had escaped from their pen out back. The birds probably sensed their impending fate as part of that evening's meal for the dozen or so regulars as well as the extra guests who were staying at the Bell for the holiday weekend. It was Independence Day, her brother James's favorite holiday. Nearly 8 years had passed since his death, but she still missed him terribly. Until his sudden demise at the relatively young age of 59, attributed to "palsy of the brain," the never married siblings had been the only residents of the 5 Albany Street home for two decades, since their mother died in 1823. Occasionally a niece or nephew came to stay with

them, but otherwise it had been just the two of them and the help, of course. Their other siblings, Augustus, a doctor in Philadelphia, and Rachel, now married to Henry Jackson and living in Castleton on Staten Island, visited at least once a year, but their stays passed much too quickly and before you knew it the house was empty again.

Mary Ervin, Lydia's cook, entered the parlor from the kitchen, where she had been working on dinner, and announced that it was time to eat. The past few days had been sweltering, and Lydia had asked if she would fix something cool and refreshing – “Don't turn on the stove,” she said. The 40-something Irish immigrant, whom Lydia had hired nearly 5 years previous, obliged by putting together a platter of fresh fruit and shellfish she had bought the day before at the Hiram Market, the farmers' market just a short walk away. The two women proceeded to the dining room, where the table was set with the blue-transfer-printed plates that had been in the Van Dyke family for decades, probably fragments of a set Lydia's parents received when they were married. The sliced watermelon and peaches, grapes and cherries, all locally grown, looked beautiful on the platter Lydia had added to the larder not so long ago. A small salad of lettuce and tomatoes from the backyard garden was lightly sprinkled with French-imported John Durand olive oil and the shellfish – a dozen or so oysters and clams on the half shell—were neatly displayed on the old Chinese charger. How good it was to see that beautiful piece of china that was usually hidden away in the cupboard. A hand painted tureen overflowed with an assortment of nuts – peanuts, walnuts, almonds, pecans, and hazelnuts. This was more than enough for the two women, especially on such a hot and humid day, but they savored the meal after which they adjourned to the early evening shade of the front porch with their partially finished drinks.

### ***Making Do, by Alex Bartlett***

James Couenhoven was really feeling it. The 1850s were not being kind to the keepers of the Bell Hotel, and the Panic of 1857 just added insult to injury. Couenhoven wanted to make the tavern – the old Indian Queen – into a showplace; he wanted the nicest glassware and the best alcohol, but with the financial climate of the day, he just could not do it, no matter how hard he stretched every penny. Those plain and boring tumblers would have to last a little while longer. Worn and chipped with wear, they were hardly pleasing to look at. Then again, most of his patrons probably did not even care. They were perfectly happy to imbibe using whatever glasses were there. It was the same story with the dishes. Old and time-worn, they still functioned as they were supposed to. James wished that the help would be a little more careful though. He was getting tired of always replacing the blue decorated dishes – especially the platters. He was having enough money troubles. Besides, every time he replaced one, it never matched the others exactly. But it would have to do.

James did not want the tavern to appeal only to customers who were drinking away their sorrows, so he kept a supply of soda and mineral water on hand. With S.W. Bells' place just up the road at 94 Albany Street, getting a good stock of beverages

was easy enough. Every once in awhile, one of his patrons would order up some champagne. Though times were not horrible, the taciturn hotel keeper hardly felt like celebrating – he left that to those sitting at the bar.

Not that James had much to complain about. It was, after all, a reasonably successful business, steeped in tradition – John Adams and Benjamin Franklin once shared a room at the Indian Queen, you know – and located on the best corner in town. Still, he wanted things to be better, and hoped the 1860s would bring better times. Little did he know that the country was on the brink of war.

### *Beast Grease by Tod Benedict*

Benjamin V. Ackerman had just finished overseeing the day's dipping at his 8 Water Street tallow chandlery, known as B.V. Ackerman & Co., and was walking toward his home next door at No. 10. He rented two lots from the Pools, one for the chandlery and the other for his home. William Lawrence, Harriet Pool's brother, co-owned the lot, but he lived in New York and left the business of landlording to his sister and brother-in-law who were just across the river. A stiff breeze was blowing from the south, and the slightly unpleasant smell of tallow, colloquially called "beast grease," was in the air, although neither Ackerman nor his family noticed it anymore. At least there were not any animals. Since slaughtering within city limits and the resultant problem of carcass disposal was not looked upon kindly by most New Brunswickers, Ackerman did not keep the sheep or oxen required for making his own tallow. Instead he had a shipment delivered every Monday. His business was doing well in 1850 and was valued at about \$3,000, a figure that included equipment, raw material, and product on hand.

As he approached his doorstep, the three youngest Ackerman children, Abigail, James, and Benjamin, who were playing with dolls and marbles in the backyard, ran to greet him while his eldest daughter, Sarah, already 10, and his wife, Ann, who were helping their servant hang laundry on the clothesline, smiled warmly. The oldest of the children, David, was busy with his homework in the first floor parlor and the baby was asleep upstairs. It was quite a houseful. David sometimes helped out at the chandlery after school, but not today and besides, there was always Mr. Cory.

The Cory family lived above the chandlery. Robert Cory worked part-time, depending on demand, and David Ackerman used Cory as an excuse to get out of working whenever he could. Robert and Mary Cory were the proud parents of a 6-year-old and a 3-month-old baby. They were African American as was the Ackermans' servant, Jane Hoagland, and they all got together whenever they could. The two households generally got along, regardless of the not-infrequent acts of violent racism that occurred across the country at the time, to which New Brunswick was not immune. However, the Corys' tenure as residents of 8 Water Street would not be long lived – they had left for parts unknown by the mid-1850s and were replaced on the second floor by liveryman William Gable and laborer John McGlean.

***Letter to Charles Steward Stout by Juliette Gerhardt***

14–16 Water Street  
 Charles Steward Stout, Farmer  
 Lake, Illinois  
 September 5, 1860

Dear Charlie,

How is Papa doing? I am certain he is much better off out there as I know it would pain him to see how changed New Brunswick is. We took the carriage out to where our little house used to stand on Water Street and all that's left now is Papa's wharf. It was sad to see an ironworks on the spot where our house stood and to remember what a fine view we had of the Raritan. Maybe you remember how excited we used to get when one of Papa's cargo ships pulled up to the wharf, and how we snuck out to watch it unload. I remember the first time we saw and ate a coconut. Moses and I used to like hiding under the porch, listening to Papa and the Captain as they exchanged the particulars of trade over a pipe and how, between sips of rum, the volume of their conversation rose and fell as if pirates were at our very doorstep. As scared as we sometimes got hearing those stories, we preferred that secret spot to Mama's fancy tea parties. But this reminds me, Charlie, do you remember that time we were having tea with the wife of one of Papa's business associates and you came whipping around like a great gust of wind upsetting a tray of Mama's favorite blue cups? Poor Mama. We would have laughed if you had not spoiled things in front of company. Well there's a story you must tell Papa and see what he recalls of those days.

Give Papa and the children a hug.

Affectionately,  
 Mary

After writing this letter to my brother, I went about the business of unpacking and settling into our new little house, ruminating about the past and those days on the Raritan. I was making my way through a crate of our belongings when I unwrapped a ceramic figurine of a man dressed in the old style that was one of Mama's treasures. It used to sit on the mantle paired with a woman holding a basket, but what became of that I'll never know.

***Boardinghouse Blues by Tod Benedict***

Looking none too happy, Pierce Roach and James H. Fine walked out the main door of the New Brunswick Rubber Company on Peace Street and slammed the door behind them. The two men were looking for work again, having been unemployed since most of a year ago. Almost a year without jobs – since the Novelty Rubber Company closed its doors for good in early 1886 – some thanks after more than 6 years of service! Not that long ago the factory, located directly north of the hosiery mill above the “head” of Water Street, was the country's largest manufacturer of rubber buttons, roughly 50 million per year. It also manufactured smoking pipes, checks, canes, and knitting pins, but suddenly business just dried up. Sadly, they were told, not even part-time work was currently available at New Brunswick

Rubber and there was nothing at the New Jersey Rubber company either – they had stopped by their office on the other side of Albany Street just yesterday.

The two men cut across the New Brunswick Rubber Company yard to Peach Street and headed southward toward Albany. They thought about stopping for a drink at the Whitehall Hotel, but they knew it was too early. It was dangerous to drown your sorrows in alcohol and they were too close to home to get away with it. At No. 5 Albany Street the men were greeted by Roach's wife, Eliza, who was waiting on the front porch. Pierce gave her a peck on the cheek and James walked up the stairs to the apartment he shared with his brother, sister, and brother-in-law. James Ferguson, a fisherman, and his family also lived there which made the apartment a bit cramped, but at least there were lots of people to share the rent.

Ferguson and Joseph Fine, James's brother, worked a fishing boat together. Once in a while they went out into the bay just past Perth Amboy, but usually they stayed in the river where the primary catch was striped and largemouth bass, flounder, weakfish, sheepshead, catfish, kingfish, and carp. Sometimes they got herring, yellow perch, or even eels, although there were fewer and fewer now that the factories along the river were spewing their waste into the water. They finally docked the boat along the north side of the Albany Street bridge as the sun was setting, another dawn-to-dusk day on the water. By the time their catch was unloaded and transferred to the waiting ice-filled wagons, to be taken to the local market for sale the next morning, nearly all vestiges of light in the western sky had faded. The two men trudged up the block and a half along Albany Street to the house, seemingly a much longer walk than it had been at six that morning. Fergusons's two younger children, James and Bertha, were already in bed on this school night, leaving only his wife and brother-in-law to greet them as they reached the top of the staircase. Dinner tonight, as most nights, would be a simple affair, baked fish, this time flounder brought home the day before, potatoes, and salad from the backyard garden, drizzled with olive oil.

The Roach family downstairs had eaten much earlier and considerably more formally. Having made a trip that morning to the grocery and the butcher's shop on the west side of Neilson Street, Eliza Roach put a decent meal on the table. It was served on the set of plain white dishes she had cobbled together and she even used the Chinese porcelain platter salvaged from the attic when they first moved into the house. It probably once belonged to the Van Dyke family, but Eliza would not have known that. It just looked pretty and no one else claimed it.

Seeing the discouragement on her husband's face, Eliza did not bother him about helping the children brush their teeth before bed with Calder's Dentine tooth powder or comb the lice out of little Elizabeth's hair. Oh dear, she wondered, when would there be work for Pierce and the other men who had been let go from the button factory? After the children were safely tucked away, Eliza sat down to read Mark Twain's *Adventures of Huckleberry Finn* by gaslight. She was lucky to be able to borrow from the New Brunswick library. Twain's latest offering had been published in February 1885 and here she was already reading it.



## Through Many Eyes

There is a kind of poignancy in the image of Eliza Roach reading Mark Twain by gaslight in the very same house where Rachel Van Dyke had read Virgil 75 years before. In fact the whole sequence of narratives, and I have left some out, paint a poignant picture of New Brunswick as it changed from one kind of community to another, from a small city based on river trade to an urban center that first supported small industries conveniently located next to the canal, and finally to a thriving factory town until the factories outgrew the transportation network and Johnson and Johnson (J & J), the multi-national healthcare corporation, bought up the land. My colleagues – the authors of the narrative vignettes – found this story in the archaeology. It is their interpretation of what the site meant. With urging they were willing to think about how people used the things we had found, how they re-worked old houses into new ones, how they made a living, and how they made do. The vignettes humanize a process of urban growth and urban decline, a process not unique to New Brunswick. And, as elsewhere, the process ended with a road, first built in the 1970s and re-worked in 2005, the reason for the archaeological project.

This is not the only story that could be told about New Brunswick, or the only interpretation of what was found. I addressed the research questions: Was there anything in the artifact assemblage that suggested the Dutch background of many New Brunswick families? Were artifact assemblages associated with public places distinct from assemblages from private houses? Did there appear to be differences between the kinds of things people owned in “urban” New Brunswick and the things they owned at the small trading community across the river known as Raritan Landing? My interpretations appeared along with my colleagues’ in the final chapter of the report. We presented the narratives in several venues, always with two podiums so readers could alternate without interrupting the flow of the story. Audiences loved it, old New Brunswick residents with tears in their eyes, and the client – the engineering company – embraced (and even read) the report. Only the New Jersey Department of Transportation (NJDOT) was nervous, just about the final narrative, written by me. My narrative talked about the transformation of New Brunswick, from an aging industrial town with character to a sterile corporate home – base for J & J, a story that involved the burning down of most of the old buildings and the intentional dismantling of a National Register Historic District (Yamin and Masso 1996). Rachel Van Dyke would surely not recognize the town where she was born although the First Reformed Dutch Church that was under construction during the year she kept her diary is one of the two historic structures in the NR district that survived.

The Five Points narrative vignettes, based on archaeological finds from the Courthouse Block in Lower Manhattan, served to present a very different picture of a neighborhood that had been characterized as a notorious slum in the yellow journalism of the nineteenth century (e.g., Foster 1990) and perpetuated in the twentieth and twenty-first centuries (Asbury 1928; Scorsese 2002). The New Brunswick narratives are different. There are no myths to explode about New Brunswick, but



**Fig. 9.4** Barware from the Bell Tavern (formerly the Indian Queen) including an assortment of tumblers, pitchers, and liquor bottles (feature A, AS II)

there is a past that has been completely obliterated. With the exception of one church (Christ Church) and the reconstructed Indian Queen Tavern, which was moved board by board from the project block to a county museum on the other side of the Raritan River, there is virtually nothing left of the eighteenth- or nineteenth-century town. Rachel Van Dyke's journal and the vignettes developed from the archaeological investigation of one block, provide at least a glimpse into the city's lived past. The artifacts, boring though their descriptions may be, also provide an authentic tie to the past and it is important that they too be part of the record. In a yet to be published paper Jim Symonds has written that abandoned artifacts found in Sheffield "provide a sensual echo from the industrial past" (Symonds 2009: 45). All of it matters (Fig. 9.4).

Dance teachers, especially very good dance teachers, describe new steps, or combinations of steps, in several different ways. They seem to intuitively know that some students will understand one set of words while others will need an entirely different set. They also know that demonstrating the new step or steps is not enough – at least for most students. Words are necessary and different words will work for different people. Archaeologists should be so smart. We say we need different reports for different audiences, but what we really need is one report that expresses our finds in different ways. Rosemary Joyce (2002: 132) puts it this way: "And above all we need to tell multiple stories about the same material, in multiple media and formats, from the conventional article to the imaginative narrative." She might not agree that all the stories should be in one report, but I think that is just where they should be. Different people will understand in different ways, but all should be

made aware that there are different ways to know about the past and that all our efforts – our interpretations – are just that, interpretations.

The hierarchical way that most archaeological projects are organized often leaves out the insights of the people who are closest to the data. We have learned to incorporate the perspectives of descendant communities and other interested parties, but we have been less good at providing opportunities for everyone involved in the excavation to contribute to the final product. It is not easy to break out of a hierarchical framework; it is built into the capitalist culture in which we live. I believe it is necessary though, if we are to realize the most we can from the fragmentary remains of the past that are our stock and trade.

## Notes

1. Tom King (1998) has provided the discipline with numerous books explaining the laws that guide the practice of cultural resource management in the United States. Among the most useful is *Cultural Resource Laws and Practice, An Introductory Guide*.
2. In his book about John Adams, David McCullough (2001: 155) describes the room Adams and Franklin shared in the Indian Queen as "...a chamber little larger than the bed, without a chimney and with only one small window." According to McCullough, Franklin and Adams argued over whether the window should be open or closed, Franklin, of course, preferring fresh air. Adams, it is said, fell asleep to the sound of Franklin expounding "upon air and cold and respiration and perspiration" and the window remained open.

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# Chapter 10

## The Archaeology of the Aesthetic

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2008 marked the 300th anniversary of Annapolis' Royal Charter. Annapolis was founded in 1649 by Puritans and other religious radicals pushed out of Anglican Virginia and welcomed in religiously-tolerant Maryland. In 1708 under Queen Anne of Great Britain a city charter was issued to Annapolis. This was the first lasting democratic process in Maryland, or in any of the southern colonies. It also laid bare an important reality of the eighteenth century world: many people were not free or enfranchised (Annapolis Charter 300-Annapolis Alive! 2008). We commemorated this anniversary by celebrating the quest for liberty, which has characterized

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Annapolis since its inception. It was home to religious dissidents, political dissidents during the American Revolution, many free African Americans before Emancipation who fought very hard for the freedom of the enslaved, as well as African Americans after the Civil War who fought for the civil rights that they were continually denied.

Asked to participate in the year-long celebration of the charter, Archaeology in Annapolis' contribution was two-part: public excavations within the historic district of Annapolis and exhibitions of materials excavated from the city over the last 30 years. These exhibits, entitled "Seeking Liberty: Annapolis, an Imagined Community," (hereafter referred to as "SL") were to show the past, its immediacy, and its connection to today. However, these exhibitions were not to be a retrospective on discoveries, rather, our intent was to explore the notion of liberty in the context of Annapolis, a place where liberty was sought and produced. At the same time, these exhibits were to invite people and groups to seek more freedom where they felt it was necessary. This essay will focus on how the theme of liberty was displayed through archaeological materials, and how these materials, and associated texts can show Annapolis as a place where lives were improved by demanding greater freedoms, and where people can influence current Annapolitans to continue to demand these freedoms. Central to our understanding of the purpose of "SL" is a close reading of [Terry Eagleton's \(1990\) \*The Ideology of the Aesthetic\*](#), which allows us to use social theory to speak to our efforts to build, in public, archaeological data that show how interrelated the parts of the city have been and that, although stratified and sometimes exploitative, Annapolis is a city where its groups seek to be mutually helpful by being equal to all others. This essay will follow a basic structure: introduction to the Annapolis Charter 300 celebration, an overview of the part that Archaeology in Annapolis played within this celebration, a look at the thematic material of the "SL" exhibits, and, finally, a discussion of the social theory that allows archaeological materials to be actively involved in the production of liberty in a city like Annapolis.

The idea of a year-long celebration marking the tercentenary of the charter of 1708 was that of the mayor of Annapolis, Ellen Moyer. A strong supporter of Archaeology in Annapolis almost since the project's founding in 1981, Ellen Moyer had been an alderman representing Eastport, Annapolis' earliest suburb, for many years before becoming Annapolis' first female mayor in 2001. Mayor Moyer designed the anniversary to include all parts of the city with popular, international, and scholarly events. The Mayor set up a volunteer committee to lead the celebration, but other than authorizing three grants to the City for components of the 'Annapolis Alive!' she kept hands off the fund raising and management. The idea was a celebration of Annapolis' independence and the creation of a truly participatory democracy, so that tourism could be enhanced and the history and importance of Annapolis would be highlighted (Annapolis Charter 300-Annapolis Alive! [2008](#)). Annapolis was to be celebrated, not only through new works of art, music, literature, and festivities, but through family and neighborhood commemoration.

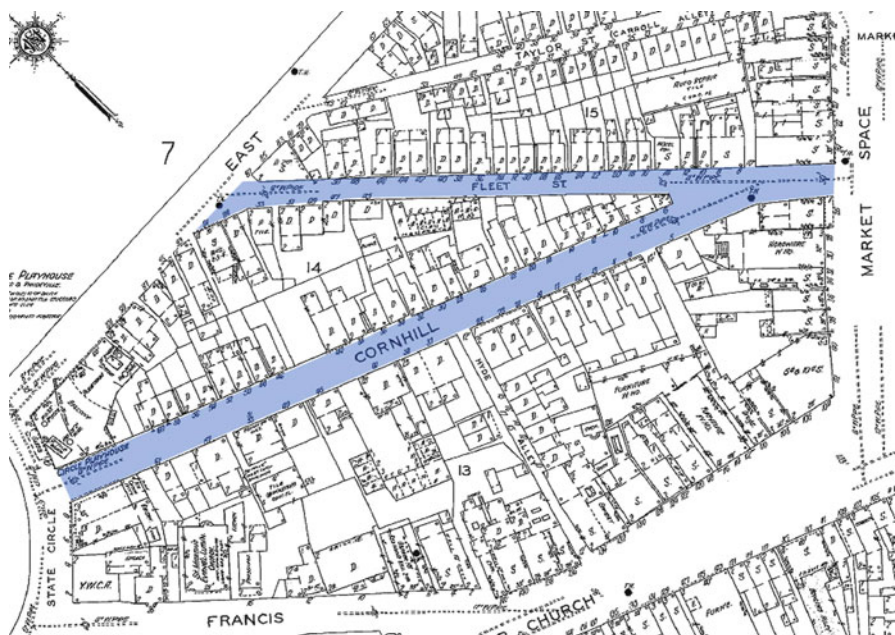
The grounding for the use of archaeology as a significant part of the city's anniversary celebration came from a trip the Mayor made to Jamestown. The Jamestown [2007 Celebration \(America's 400th Anniversary Legacy Site 2009\)](#) marked the 400th anniversary of the founding of Jamestown, the first established English colony

in Virginia. This year and a half long celebration's goals were to bring the international spotlight to Virginia, increase business opportunities throughout the state, and create an educational environment for students, both in Virginia and around the country (Flippo 2004). Virginia's use of Jamestown's justly famous excavations, undertaken by William Kelso, set the foundation for its 400th anniversary event (Preservation Virginia 2009), and influenced Annapolis' mayor to consider archaeology as a cornerstone for 'Annapolis Alive!'

From this experience, the Mayor, her committees, and city administrators decided on two parallel archaeological exhibitions to be the heart of their efforts to show the city's past and to demonstrate the liveliness and importance of its archaeology. The archaeology was to be useful, direct, and inclusive of the city's many populations, most obviously its long-subordinated African American neighborhoods. The first environment for using archaeology was a major excavation on Fleet and Cornhill Streets, in the heart of the historic city. The purpose of these excavations was to have a full-scale archaeological project available to the public for touring as a physical example of how archaeologists work and to show the immediacy of the archaeological materials under the sidewalks and streets of Annapolis. The second environment for celebrating the tercentenary was a number of exhibits showcasing the very best archaeological collections recovered in Annapolis since 1981. These exhibits, designed around a core exhibit at the Banneker-Douglass Museum, were intended to display materials excavated from within the historic center of Annapolis at the buildings from which the materials were recovered. The central exhibit, "SL," was intended to serve a number of purposes: as a primer on all of the sites on exhibit, to show as much as possible of the city's archaeological heritage, including many of the best collections of materials, and, most importantly to our discussions here, to explore the idea of liberty in the context of Annapolis and to show how the people in the city have pushed continuously for ever greater freedoms for themselves.

Initially framed as part of a large public works project for the city of Annapolis, the excavations on Fleet and Cornhill Streets became an integral component to Archaeology in Annapolis' part in celebrating the city. At first, the city wanted our excavations to be part of the Annapolis Charter 300 celebration, but archaeologists had many more questions, including, if there was any archaeology, how intact it would be, and if it would be worth protecting. Excavations were opened in April of 2008 and between then and mid-June, more than 3,500 tourists visited the sites. Fleet and Cornhill Streets are small, short, residential streets that run downhill each in the same direction, from the Maryland State House to just above the city's harbor, called City Dock, where they form the base of a 'Y' (Fig. 10.1). Although the harbor is relatively shallow and small, it is well sheltered and midway up the western shore of the Chesapeake Bay, making this an integral feature to Annapolis' early success. Fleet Street is by far the older, dating to as early as 1684 and certainly to the early eighteenth century. On this basis alone, archaeologists were providing a first-hand look to tourists and Annapolitans of what the city was like during the early part of the eighteenth century when it became Maryland's capital.

Not only did Fleet Street provide a look into 1708 Annapolis, but also to a richly diverse occupation over the last three centuries which made the neighborhood



**Fig. 10.1** Fleet and Cornhill Streets. Excavations were open to the public during 2008. Excavations uncovered a log road, sometimes called “corduroy” road, dating from the 1680s, intact, and submerged below the water table. There was an altar or bundle, dating to the early eighteenth century, derived from West African spirit practices, left in the open in a gutter of the upper part of the same road

a precious archaeological resource in its own right. Fleet Street was possibly almost a century older than Cornhill Street, Cornhill not being developed as an urban neighborhood until the 1770s, and mostly after the American Revolution. Even so, Cornhill holds ten of the city’s 50 eighteenth century houses. On Fleet Street, one of the houses had been a mint after the Revolution and another a stable, demonstrating that there were home-based cottage industries in the neighborhood. Fleet Street also houses one of the few remaining African American neighborhoods left in the center of the city. Local knowledge guided us to the fact that African Americans had owned businesses and houses on Fleet and Cornhill Streets as early as the late nineteenth century, and a number of African American families still live on the street (Horace Byrd, personal communication 2008). We also know that both Cornhill and Fleet Streets were home to the first Jewish community of Annapolis (Congregation Kneseth Israel 2009). Russian Jews had owned up to ten houses on these two streets starting in the 1870s, including one that was used as a synagogue during the early twentieth century.

Public excavations can, according to Parker Potter, Jr. (1994:167), “help re-enfranchise people with control over their own consumption of history”. Potter (1994) goes on to say that in order to re-enfranchise people, a tour guide must



demystify archaeology, place the archaeologist and tourist on the same footing, discuss the social context of the excavations, be explicit about process and not hide agency, and “present archaeology as a contemporary social activity that creates a version of the past (1994:168).” In the case of the excavations on Fleet and Cornhill Streets, archaeologists were providing the context for the materials that tourists would see on display in the “SL” exhibit. By having open excavations, tourists could see the process through which materials are recovered, and make the connections with objects that they see in the ground and the artifacts that appear somewhat wiped clean of their context and made into ‘art’ in the museum.

“SL” was created primarily to celebrate the history of Annapolis, but also to celebrate the best and most interesting collections that have been uncovered archaeologically from Annapolis. However, the exhibits were not to be a retrospective on discoveries. The first phrase in the exhibit’s title, “Seeking Liberty: Annapolis, an Imagined Community” is a gerund, a verb acting as a command and, therefore, an invitation to visitors and viewers. This invitation was to see Annapolis as a place where liberty was both sought and produced, to see the struggles, and the cultures that have created the city as it is today: and an invitation to look forward, and to continue pushing for freedoms, to make Annapolis the place they want for the future. We featured the American Revolution, women’s quest for professional recognition and equality, and African American opposition to racism as well as the quest to preserve religious freedom, both West African and Islamic religious traditions. The specific examples were, to us, less important than the forward-looking command to see Annapolis as a place where lives were improved by demanding greater political independence, equality among the sexes, voting rights, equal access to historical materials and interpretations, and elimination of racist treatment in the past. Annapolis was a place to continue these quests. That was the city’s theme and identity; and because this quest had defined Annapolis for 300 years, it was important that it be identified, celebrated, and lived out in the future.

Within “SL” there were five sites exhibited, which had all been excavated by Archaeology in Annapolis. These included the Jonas Green House and print shop, the Governor Calvert House Hotel, Reynolds’ Tavern, the Brice House, and the Maynard-Burgess House. All five of these sites unveiled important information about disfranchised populations in Annapolis. The Jonas Green House and print shop showed the life of a professional woman in the eighteenth century; at the Brice House, Reynolds’ Tavern, and the Governor Calvert House, religious practices of enslaved Africans; at the Jonas Green print shop, political dissent during the American Revolution; and at the Maynard-Burgess House, how African American families, both before and after Emancipation, negotiated their way through a world of racism. The then-Director of the Banneker-Douglass Museum, Wendi Perry, immediately saw the potential of this exhibit and gave it a home at the museum. It fits in directly with the mission of the museum, which is to collect, document, preserve, exhibit, and interpret objects and materials important to the African American history and culture of the State of Maryland (The Banneker-Douglass Museum 2009a, b). To this



**Fig. 10.2** Archaeological materials excavated in Reynolds' Tavern and exhibited in the foyer of the building now. This is one of five locations in Annapolis with such exhibits. The two lower shelves contain caches or bundles invoking West African spirits through rituals. The bundles were found at the base of the basement steps of the Tavern. All the materials date to the early nineteenth century

initial exhibit at the Banneker-Douglass Museum, satellite exhibits, focusing on similar themes, were to be placed in each of the buildings from which the materials were excavated.

Subsequently, we worked with the owners or managers of the Jonas Green House, Reynolds' Tavern, and the Governor Calvert House Hotel to place cases of artifacts and interpretive panels in their businesses. These exhibits were placed in frequently used public spaces in each building, and will remain there indefinitely. Exhibit panels were also created for the open spaces and backyard of the Maynard-Burgess House, which is owned by the city of Annapolis and directly across the street from City Hall (Fig. 10.2). These satellite exhibits were meant to enhance each of the businesses that they were placed in, as well as to make more explicit that the archaeology of Annapolis belonged to the people of Annapolis.

By 2008, on site exhibits of archaeological remains were no longer a new idea. Although Archaeology in Annapolis had initiated the idea of displaying artifacts in the Calvert House Hotel in the early 1980s, and had mounted small exhibits around the city, as well as one larger exhibit at the Banneker-Douglass Museum of our excavations in the area of their new wing, we had never been able to mount an exhibit that involved as many artifacts with an ambitious, coherent interpretation drawn from our research.

Historic preservation has left a permanent mark on Annapolis, having created the classic, current look of the city. That look was protected by law, promoted in guidebooks, and cemented in the popular consciousness. Even though some of this appearance was the result of archaeological work, such as the restored William Paca Garden (Historic Annapolis Foundation 2007; Shellenhamer 2004), a visitor to the town would rarely know it. With these new exhibits in popular locations, there were reminders that the town had produced archaeology that commented on its past.

The Banneker-Douglass Museum education department also implemented two technology-based forms of interpretation – a cell phone tour and a blog. The purpose of using such technology in this exhibition was to allow wide access to interpretation of the exhibition and its artifacts as well as to open up a dialog among visitors to sites and online. Both cell phone tours and blogs were new to historic interpretation in the Annapolis and Anne Arundel County at the time of this exhibition, never having been utilized by historic and/or cultural institutions in the area. The use of these two technologies was followed with interest among the historic community eventually leading to the establishment of a technology working group for historic sites in the area. This group was organized by the Four Rivers Heritage Area. The group examined the use of technology at historical and cultural venues throughout the region as well as offering tutorials on developing and implementing Web 2.0 technologies.

When looking at various ways to interpret “SL,” it was evident that with the inclusion of a central exhibition at the Banneker-Douglass Museum and several satellite locations, a new approach was necessary. Implementing technology-based touring allowed for a broader audience and permitted the exhibition to expand beyond the physical walls of the museum. Through the use of a cell phone-based tour, the exhibition was able to easily incorporate all of the locations included in the exhibition in a unified voice. Rather than relying solely on text panels and the archaeological objects at each individual location, visitors were able to use their personal cell phone to walk to each of the locations to contextualize their exhibition experience by viewing each of the sites interpreted in “SL”. At each site, visitors were able to listen to pre-recorded interpretations based on the specific location and the objects found there.

The cell phone tour proved to be an interesting form of interpretation for visitors, with over 1,000 calls logged into the tour during the run of the exhibition. The cell phone tour gave visitors the opportunity to receive personalized guided tours of “SL” by allowing for the selection of locations to visit. It also provided greater ease in touring the exhibition because it allowed tours outside of museum hours. In selecting a cell-phone-based tour option rather than an mp3 player, or wand-based

tour, the museum was able to create further accessibility. The tour was offered free to the public save for the number of minutes they used listening to the tour. All of this allowed “SL” to reach a wider audience.

The implementation of a blog for “SL” allowed the exhibition to open and maintain a constant flow of communication between the exhibition’s audience. The role of the blog was to allow for the exhibition team to talk about various topics in “SL,” developments in the Fleet and Cornhill Streets excavation project, and discuss related issues. The blog was created and utilized in the weeks prior to the exhibition opening to provide a “sneak peek” at the exhibit and talk about what goes into developing and designing an archaeological exhibition. This provided a similar behind-the-scenes look at archaeology that the excavations on Fleet and Cornhill Streets did. By demystifying the archaeological (and museum) process and creating a social context for the exhibition, it allowed viewers to come at “SL” from a different angle.

The response to the “SL” blog was global with over 3,500 visitations from over 40 countries through the world and nearly every state in the United States. Visitors have come from all backgrounds ranging from area residents to members of the archaeological community. Connecting to the blog through links on the “SL,” Archaeology in Annapolis, and Banneker-Douglass Museum websites as well as through internet searches on the exhibition, visitors were able to experience an ongoing examination of the exhibition and actively participate in the interpretation of the exhibition. Additionally, visitors were able to leave comments and ask questions through the blog, which the exhibit team was able to answer, thereby providing a direct experience and for additional discussions beyond those crafted by the exhibition team.

At the conclusion of the exhibition, there was some debate over the future of the blog. Because the blog was based on an exhibition that was no longer on display, the exhibition team was unsure how to proceed and decided that blog visitors could decide its fate. The response to a posting asking readers what they would like to see happen to the blog was that it should be turned into a blog for the Banneker-Douglass Museum with the “SL” entries remaining in the archives section and occasional entries relating to the now-closed exhibition appearing in the blog (the “SL” blog is: <http://bannekerdouglassmuseum.blogspot.com/>). Even after the blog’s conversion, a high level of interest in the “SL” exhibition entries remains. Searches and visitor activity on “SL” topics appear daily on the blog monitoring software thus validating the decision to maintain the blog and continue the conversation.

Another way that Archaeology in Annapolis sought to increase the breadth of “SL” was through a web-based tour of the complete exhibit. This would provide greater access to the exhibit and its themes, as well as extending the exhibit’s 16 month life indefinitely. Completed by Jessica Mundt, then a graduate student at the University of Maryland, College Park, this webpage provides a set of pages featuring each of the five sites, all of the text panels from the exhibit at the Banneker-Douglass Museum, discussion of the main themes of the exhibit, and photographs of all of the artifacts (the website for “SL” is: [http://www.bsos.umd.edu/anth/aia/seeking\\_liberty/home.htm](http://www.bsos.umd.edu/anth/aia/seeking_liberty/home.htm)). We mention the several ways of reaching many publics

because we also decided against publishing a paper catalog of the exhibit. In particular, the blog allowed for some discussion and further interpretation. The blog could be a vehicle for individualistic expression of the results of having access to so much unique material that represents so many long invisible pasts of the many groups who live in Annapolis.

Before delving into the questions of why we built the exhibits and what they were supposed to achieve, we would like to open up two of our themes: (1) political dissent during the American Revolution, and (2) the enfranchisement of more people from the social fringes of Annapolis. This second theme was carried out by exhibiting women and African Americans in Annapolis' history. Through looking at unconventional roles of women in eighteenth century Annapolis and African and African Americans struggle to preserve religious practices and cultural integrity, we hoped to make explicit that Annapolis housed many more traditions in its past than previously, and publically, acknowledged and which, therefore, should be a part of the city's present and future.

First, we would like to discuss political dissent and the American Revolution. We displayed, as a central object of the exhibit, one of Archaeology in Annapolis' most important artifacts: a piece of printers' type used to print a skull and crossbones, called a death's head. This small block of type was found in our excavations of the Jonas Green print shop in the early to mid-1980s (Fig. 10.3). It was used to print newspapers in 1765 to protest the Stamp Act and accompanied the banner headline on the *Maryland Gazette* ([MG], 10 October 1765), "The Maryland Gazette, EXPIRING: In Uncertain Hopes of a Resurrection to Life Again" (Fig. 10.4). The skull and crossbones image was placed in the lower right-hand corner of the page, in place of the mandated stamp. The Stamp Act is universally recognized as a crucial event in the American separation from Britain and an early stage in the revolutionary fight for American Independence. The *Maryland Gazette*, and its publisher, Jonas Green, played defining roles in rationalizing the Revolution.

Also tied with the *Maryland Gazette* was Anne Catherine Green, wife of Jonas, who took over the printing business and continued running the *Gazette* during the Revolution and early federal periods. During the eighteenth century, women were not recognized as having personal or professional independence. Despite this, Anne Catherine became the printer to the state, head-of-household, and a well-known community leader. She is the best known female Annapolitan of the eighteenth century. Her role in printing has been studied in doctoral research (Little 1987; Leone 2005:51–52). Anne Catherine has become an icon in Annapolis in the last 20 years, mostly because descendants of the Greens, who are the current owners of the Jonas Green House, have incorporated her into the tours of their property and placed her on par with Jonas Green who had previously received far greater attention, but whose accomplishments were probably no greater than his wife's (Capt. Randall Brown, personal communication 2008). This set of efforts has promoted the city to celebrate a set of women leaders in a way that is new and now includes: Anne Catherine Green, Harriett Tubman who is from Maryland, although not from Annapolis, and St. Claire Wright who founded and led the city's famous and successful historic preservation movement.



**Fig. 10.3** The “Deaths Head Type”. This small block of type was found in our excavations of the Jonas Green print shop in the early to mid-1980s



**Fig. 10.4** “The Maryland Gazette, EXPIRING: In uncertain Hopes of a Resurrection to Life again.” The skull and crossbones image was placed in the lower right-hand corner of the page, seen in the mid left of this image of a display of Jonas Green’s imprints. The headline itself is not visible in our photograph

A second investigation looked at the integrity and continuity of African and African American culture in Annapolis, especially the survival and flowering of West African religious traditions, including some evidence of Islam. While Archaeology in Annapolis has done work on as many as ten sites owned by or related to African Americans, our most visible and important work was on the survival of West African religious traditions in Annapolis. Known as Hoodoo or conjure, these religious practices are an amalgam of Christianity and many separate West African practices from Nigeria to Congo that almost always feature bags or bundles of objects that are used to make the spirits of the dead work for a current purpose: healing, protection, or punishment. Objects common to bundles are pins, nails, four hole buttons, rounded stones, white powder, red objects, and rings. These bundles or bags are referred to by archaeologists as caches, but are more appropriately called *mojos*, *hands*, *fixin's*, *root work*, or *tobys*. They descend from the Mande, Yoruba, KiCongo, and Congo, among other groups, and became an amalgam of ideas in North America throughout the practice of slavery and continued to flourish in the later nineteenth century and are still used in some forms in the American South today (Leone and Fry 1999; Leone et al. 2001).

We had found caches or bundles in many of the most famous buildings in the city. We could find them because we had made an effort to isolate the characteristics of their contents and placement after discovering our first one around 1990, and we subsequently looked for them systematically whenever we worked. We had three goals when looking for caches: we wanted to show that African traditions were not extinguished with slavery; that there was African American culture, different and whole not just a version of European American culture; and that Christianity, both Catholicism and Protestantism, was not the only religion in practice in Annapolis.

“SL” framed its exhibit around two central objects. The first was the “Death’s Head printers’ type”. The second was a piece of carved tortoise shell that was uncovered at the base of a lightning rod amongst a small cache of items outside of the Brice House. Through reading the work of Thompson (1983) of Yale who is a scholar of West African religious traditions, it was determined that within these traditions spirits are associated with beams of light and reflective objects. In this case, the lightning rod provided a pathway for the spirit, while a crystal held the spirit. In a similar vein, in the Afro-Cuban Lucumi traditions, the tortoise is sacred to Shango, the orisha (spirit or deity) of lightning. “The placement of the shell at the base of the lightning rod at Brice House at least suggests the practice of offerings to Shango at lightning-attracting trees such as the araba in Yorubaland and the ceiba and royal palm in Cuba (and Miami). Anything “touched by lightning” in this way would carry Shango’s power or “ashe”...” (Joseph M. Murphy, personal communication 2008). This one item, like the “Death’s Head printers’ type”, evokes the spirit of “SL” by speaking to ideas of liberty and personal freedom achieved in religion by African Americans in an important and compelling way.

“SL” also featured the large set of caches we had discovered from the Brice House that made two of the house’s work rooms into a single cosmogram, which is a design of the path followed by the living and the dead and through which spirits work. These caches were displayed in a case that was built as a miniaturized to-scale replica of the Brice House work rooms (Fig. 10.5); each cache was placed



**Fig. 10.5** Close up of the biggest and central cache from Brice House. All the caches were displayed in a case that was built as a miniaturized to-scale replica of the Brice House work rooms. Each cache was placed in the portion of the case analogous to its placement in the room where it was found

in the portion of the case analogous to its placement in the room where it was found. This included one cache with about 500 items placed in the doorway, or crossroads, between the two rooms.

Ever since we began discovering bundles in Annapolis, scholars and others familiar with West Africa wondered whether there was an Islamic element in them. Islam was already an important religion in West Africa in the eighteenth century and so we know that some people who were enslaved were Muslims (GhaneaBassiri 2010). In the early 1980s, a small, gilded Hand of Fatimah or hamsa (Fig. 10.6), used to ward off the evil eye, was found during the Calvert House excavations, which we felt had to be connected to Islam. This find made us think that as archaeologists we had not explored the greater presence of Muslims in Annapolis. We also had never asked before whether any Muslims live in the city today. Two commonly found archaeological elements, which are caches routinely found in northwest corners of rooms, usually in basement or first floor work rooms, and the many blue beads in the bundles, could possibly be signs of Islamic traditions in Annapolis. Blue beads have a traditional use in Muslim cultures as protection against the evil eye, and were often cached in corners of rooms or sewn into carpets for protection (Schimmel 1992:124). This possible interpretation for the caches found in archaeological contexts in Annapolis has been, to this point, unanalyzed.





**Fig. 10.6** A small hamsa used to ward off the evil eye

All of these ideas, including highlighting a woman who was an entrepreneur in eighteenth century Annapolis, and looking at multiple West African religious traditions, allowed Archaeology in Annapolis to use “SL” as a way to explore issues of personal and professional liberty in disfranchised populations in the city. It also allowed us to urge action by more people from the social fringes of Annapolis, that is, making the histories of groups that have been historically disfranchised, or are currently disfranchised, to claim greater visibility through the political and social discourse in Annapolis.

After this description of the exhibits and the several ways of opening up the themes we ask: why did we build the exhibits and what were they supposed to achieve? To answer, we use Terry Eagleton’s (1990) *The Ideology of the Aesthetic* and rely on his synthesis of critical theorists. He uses Marx, Freud, Lukacs, Althusser, and Habermas, but builds on them to produce his own clear view, which speaks for most of us authors. We present a summary of the main parts of his arguments that underpins our efforts to build, in public, archaeological data that shows the fundamental place of social and individual equality in Annapolis, and that may promote greater access to democracy.

Eagleton begins with Freud, who placed the origins of morality in the biological need among humans to care for each other when weak or helpless. This is also the beginning of our rationale for treating and showing equality among the people of the past and present. Equality is the aesthetic. Eagleton says:

Such compassionate feeling, however, has to struggle and in the course of our personal and historical development against a whole range of threatening factors [...]: the harsh conditions imposed by the need to labour, and the conflict and domination which arise when the

appropriation of a surplus from the fruits of that labour lays down the conditions for class-society. Our shared material conditions bind us ineluctably together, and in doing so open up the possibilities of friendship and love; it is not necessary to argue in a nuclear age that friendship and biological survival go hand in hand...But the history we must evolve, by virtue of our biological structure, also divides us and thrusts us into enmity with one another. Communication, understanding, a certain reciprocity are essential for our material survival, but can always be deployed for the purposes of oppression and exploitation. The language which releases us from the monotony of a purely biological existence also weakens the intraspecific inhibitors which constrain our mutual destructiveness.

(Eagleton 1990: 410–411)

Part of Eagleton's paradigm recognizes that as we separate from nature, we separate from each other. This creates barriers like class, and race, and gendered differences which must be broken down if we are to survive.

Work, sexuality, and sociality all bring with them the possibility of gratification [...] so in the course of social development the processes of pleasure and fantasy come to separate themselves out to a certain extent from the fulfilling of material wants, in the phenomenon we know as culture. Once the economic surplus permits, a minority can be released from labour, sexual reproduction and political regulation. 'Value', in this sense, comes to distinguish itself from 'fact', and finally comes to deny its roots in material practice altogether. [...] The political struggle which arises at this point is between those who wish to direct the forces of production to the end of [...] becoming a gratifying end in itself, and those who, having much to lose from this prospect, resist it by violence and manipulation. In the service of this manipulation, certain aspects of culture can be exploited so as to redefine the concepts of power, law, freedom and subjectivity in ways which contribute to the maintenance of the given social system. A conflict accordingly sets in between two opposing notions of the aesthetic [all art], one figuring as an image of emancipation, and the other as ratifying domination.

(Eagleton 1990:410–411)

This quote is about the fundamental place of social equality, as well as individual equality and how this is reflected in aesthetics. The notion of opposing ideas of the aesthetic is how we framed our understandings of "SL" and the lens through which we viewed Annapolis as we built the exhibit. The first notion of the aesthetic, "figuring as an image of emancipation" (Eagleton 1990:411) is how we see the "SL" exhibit itself, with its purpose in showing Annapolis as a place where liberty has been sought and produced. The second, aesthetic as "ratifying domination" (Eagleton 1990:411), can be seen in the original Calvert and Brice Houses, and in Reynolds' Tavern where much of this material was found and which is about slavery. The juxtaposition of the two, our efforts to bring to the fore marginalized populations in Annapolis through "SL" and our use of materials that can be seen as part of a culture and way of life that 'ratify domination' is the conflict at the heart of our thinking and understanding of Annapolis. Eagleton goes on to suggest that if both sides of this conflict do not have free self-realization, then we will not only destroy ourselves as a society but as a species as well. Eagleton says:

The idea of a human nature does not suggest that we should realize any capacity which is natural, but that the highest values we *can* realize spring from part of our nature, and are not arbitrary choices or constructs. They are not natural in the sense of not being obvious or easy to come by, but in the sense that they are bound up with what we mutually are.

(Eagleton 1990:412)

Our exhibit is one of objects. We used about 600 artifacts, all of which were made by employing one aesthetic or another. All were presented as art. All the commercial artifacts were made according to some style. Because these artifacts were either chosen or reused for their style or purpose, and because each exists within some aesthetic, they would, when seen and understood by the groups we want to encourage, be able to enhance freedom.

Although we have been using the term ‘aesthetic’ to define and describe how we view “SL” and our discussions of Annapolis, Eagleton uses the term “aesthetic” to mean art, literature, and things which we normally call beautiful, but which embody the general while being specific.

The aesthetic is preoccupied among other things with the relation between particular and universal but which is also a matter of importance in ethics and politics. A materialist ethics is ‘aesthetic’ in that it begins with concrete particularity, taking its starting-point from the actual needs and desires of individual human beings [...while the...] fundamental political question is that of demanding an equal right with others to discover what one might become, not of assuming some already fully-fashioned identity which is merely repressed. All ‘oppositional’ identities are in part the function of oppression, as well as of resistance to that oppression; and in this sense what one might become cannot be simply read off from what one is now. The privilege of the oppressor is his privilege to decide what he shall be; it is this right which the oppressed must demand too, which must be universalized [...] it is [...] every individual’s equal right to have his or her difference respected, and to participate in the common process whereby that can be achieved.

(Eagleton 1990:414–415)

The idea that the oppressors have the right to decide what he shall be, but that this right should be universalized is the point of “SL”. The question then becomes how can this be done? How can we use our exhibit to demand the right of self-determination? For the answer to this question Eagleton turns to Habermas.

Eagleton uses Habermas to argue that art contains the stubbornness, resilience, and idiosyncrasy of daily life that plays against the domineering demands of class economics and politics (1990:402).

Habermas’ faith is that language, however distorted and manipulative, always has consensus or understanding as its inner telos [purpose]. We speak to others in order to be understood even if the content of our enunciations is imperious or offensive; and if this were not the case we would not bother to speak at all.... It is therefore possible to project from this condition the contours of an ideal communicative situation, implicitly anticipated in every actual act of dialogue, in which discourse would be a symmetrical distribution of chances. [Therefore...] we can recover the political values of autonomy, mutuality, equality, freedom and responsibility [...].

(Eagleton 1990:403–404)

And furthermore:

[...] the very act of speech or dialogue [...] cannot help bearing with it a tacit commitment to reason, truth and value, establishing a reciprocity, however inequitable, within which it is open to us to glimpse the possibility of full human mutuality. [Habermas believes] that a species that depends for its survival on the structures of linguistic communication and cooperative [...] action, must of necessity rely on reason. Reason thus has its roots in our social and biological condition [...] and in this sense truth remains something to be anticipated [...].

(Eagleton 1990:404–405)

Through this act of communication, “SL” has the potential to establish reciprocity between those who already have liberty and self-determination and those who are continuing to fight for it. However, we do not define the contents of “SL” as language, but we see it as art, meaning an aesthetic, and we see it as things and as material. It is filled with objects, and these things are about the material existence of daily life.

Habermas (1987) sees “life world” as cultures that are outside the rationalizing, bureaucratic logic of capitalism as it developed. Habermas (1987:355) sees life world and system in conflict and capitalism’s logic as invading life worlds.

What he wishes to speak up for is ‘the structure of rationality which is immanent in everyday communicative practice, and which brings the stubbornness of life-forms into play against the functional demands of autonomized economic and administrative systems.’ In the broadest sense of the term, he thus writes as a political ‘aesthete,’ defending the lived against the logical [...]. Indeed art itself is for Habermas one crucial place where the jeopardized resources of moral and affective life may be crystallized...and...may be re-established [in] the public sphere.

(Eagleton 1990: 401–402)

Lifeworld can be a radical political resource with its inner logic acting “to chart a kind of alternative rationality at work within everyday bodily experience [...up against] the operation of abstract reason”.

(Eagleton 1990: 403)

We hope that our descriptions of how to use printers type, hoodoo bundles, and West African spirit practices, the effects of racism, and Islamic charms begins to show an alternative rationality to the logic of the class structure of capitalism.

We displayed 600 largely mass produced things and with them their use and manufacture and with these we:

Move into late capitalism, into an apparently wholly reified, rationalized, and administered regime. You can’t bring it to its knees with organic craftsmanship, so you have to try instead the silent scream, the scream which in Munch’s famous painting rips open the blank face of the solitary figure and reverberates endlessly. The aesthetic becomes the guerrilla tactics of secret subversion, of silent resistance, of stubborn refusal. Art will pulverize traditional form and meaning, because the laws of syntax and grammar are the laws of the police. It [art] will dance on the grave of narrative, semantics and representation...and might just allow us a glimpse of what it might conceivably be like to be free.

(Eagleton 1990:369–372)

Eagleton sees freedom as a utopian wish inside which is an image of harmony which can be hijacked easily. While art is freeing, it is a sublimation which can be undermined and may not be able to release the power of political change. Art can be appropriated.

[...] the question of appropriation has to do with politics [...]; it is a question of who is winning at any particular time. If *they* win, continue to govern, then it is no doubt time that there is nothing which they cannot in principle defuse and contain. If you win, they will not be able to appropriate a thing because you will have appropriated them. The one thing which the bourgeoisie cannot incorporate is its own political defeat. The [...] avant garde tries to avoid such absorption by not producing an object. No artifacts, just gestures,

happenings, manifestations, disruption [...]. The [...] avant garde understands that the question of integration stands or falls with the destiny of a mass political movement.

(Eagleton 1990:369–372)

Our social theory depends on the assumption that it is best to preserve and enhance democratic society through fostering the emergence of the logic or aesthetics of oppressed groups and their alternative logics. Eagleton stresses the individuals as well, and our work begins to make room for that necessity. Further, Eagleton uses Habermas and language to build his case. Our case is built through the parallel world of material things, the world of archaeological recovery, which must operate with the world of words to go along side it. All humans make and use things and it is clear that we could neither think nor speak without things. Things, however, do not speak and, although universal, cannot operate without language. Different from language, things are likely to hold more meanings and can add or lose meanings. Key here is the revolutionary works of Binford (1983), who stressed the ability to recover these meanings in archaeological things. This means that past peoples and oppressed groups have visibility and an occasion to provide opinions, critique, and opportunity for themselves. We see this enhancement of equality as the point of “SL.” Though the objects on display have a power, importance, and meaning on their own, without text (language) communication would be muddled and the dialectic between art as emancipation and ratifying dominion would be left unevoked. Only some of our exhibit texts showed glimpses of lost lifeworlds like hoodoo/conjecture, local racist practices, charms in Islam, and woman’s professional life. Our publications extend our textual senses of these worlds better.

The artifacts in the “SL” exhibit, and many others excavated by Archaeology in Annapolis have been the basis for many books and scientific articles on Annapolis, as well as for the long run of science news pieces in newspapers, magazines, television, and radio. The artifacts form a coherent collection, excavated, cataloged, and curated uniformly for almost 30 years. Like any good scientific collection, they contain the evidence for surprising, counterintuitive discoveries that are either unique finds or form statistically valid and unpredictable new knowledge. Because these discoveries contradict the given interpretation of the history of Annapolis, they have provoked controversy, relief, and oppression. It is just these reactions that we have sought to understand and display in “SL.” Through all of the projects that Archaeology in Annapolis contributed to the Annapolis Charter 300 celebration, a strong push for illuminating struggles for liberty, freedom, and self-determination was our purpose. When better to examine the quest for liberty and social and personal equality than during the celebration of the anniversary of the beginning of Annapolis’ representative democratic government, which created as many rights for some as it disfranchised others.

We end by saying that we prefer the strengths we know which are competent excavation and precise, plainly expressed scholarship. We know this makes a difference because people in Annapolis from minority groups say these matter.

I learned about African based religious traditions that I was unfamiliar with...Caches!

It was all new in a sense that I’ve never been here, but the most interesting displays were all about hoodoo.

I didn’t know so much was going on here. That there was free slaves.

I did not know about the finds of African religious items.

A more parallel of not-so-past life with today (maybe more hopeful in its simplicity but best ingredients intact). Also a small amount of African religion also brought to America.

The African charms – not everything was taken away.

Archaeological importance and purpose.

I felt proud. It's important for us to share our culture with others.

I knew Harriet Tubman as a woman was involved but these many other woman surprised.

I hadn't been thinking about the African community this way. It was very informational.

The African Americans who had been dispersed in the city became more segregated. As a whole and specifically related to religion. I wonder what parallels Africans brought to America saw in, Judeo Christian religion with their own. I saw obvious paths for connection (not as a totality – the god with knife on head with Jesus/as description at crossroad). I wonder what has been lost in [indecipherable] silence between arrival and integration.

I see how archaeology can show us what is literally “below the surface” when it comes to history and the stories we've learned.

Debated validity of various conclusions/influences.

I was with a historian who knew a lot. I pointed out the bundle & X-rays across the hall.

With two sisters – we talked about where we grew up, artifacts. I explained Brazil connections.

The exhibition on some of the things from Africa really let me know they were not as primitive as I was led to believe. (The Banneker-Douglass Museum 2009b)

Eagleton's analysis is important to us because he articulates most recently and best why we do what we do. We also understand that there is now no way to track the results of our work's success except through the enfranchisement some feel because of our work, and the opposition of others.

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# Chapter 11

## Commentary

Sarah May

Fieldwork is at the heart of archaeological endeavour. Periodically archaeological theorists remember this and consider the questions of how and why we do fieldwork (e.g. Andrews et al. 2000; Chadwick 1998, 2003; Hodder 1997, 2000). And fieldwork changes, occasionally in response to these thoughts, but more commonly as result of changes in the structure of the discipline, funding, technology. These papers provide a welcome exploration of where these two things meet: theoretical consideration and the experience of changing practice.

Most of the papers were completed before the publication of Planning Policy Statement 5 (PPS5) (DCLG 2010) and the volume is being finalised as this is replaced with National Planning Policy Framework (NPPF) (DCLG 2011). Both of these documents offer an opportunity to bring significance to the heart of all field practice in the UK. These queries about reflexive process gain new force in this context.

### My Perspective

One of the reasons I was asked to write this discussion paper is that I have spent the last 10 years reconsidering the fieldwork practice of English Heritage's Archaeological Projects team. One of the drivers for this work was a recognition that the information infrastructure supporting our work was broken. It was a mixture of paper and digital forms, drawings, reports, databases and photographs that was configured anew for each project and indeed each stage of each project. It produced results that met the standards of the discipline, but it required a lot of maintenance and was therefore quite slow.

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Checking, filing, entering, re-ordering, sending information to specialists, receiving information from specialists, digitising plans, redrawing plans, all take time and they are as crucial to a successful project as filling in a context sheet or indeed writing the final report. However, this maintenance was not done by the intellectual drivers of the project. As with much support work it is invisible. This work is usually invisible in the theoretical considerations of fieldwork as well.

Drawing inspiration from Framework Archaeology's work (Andrews et al. 2000) we thought that developing an integrated digital system would allow us to work in a more integrated fashion. We hoped that a well designed system would reduce the support work required and allow us to focus on the intellectual work that is really valued. But we knew that a poorly designed system would reduce our capacity instead of increasing it, so we took some time to really consider our practice so that we would understand our needs (Cross et al. 2004; Cross May and Crosby 2010).

Developing the system we thought was required was simply too expensive. Even though it would increase our efficiency, it couldn't be justified by the volume of fieldwork we actually did. So we adopted and adapted a system called Intrasis designed by the Archaeological Excavations Department of the National Heritage Board, Sweden. In implementing it we have learned more again about our practice. While it has improved the integration of our work, and therefore made it faster, we are still facing many of the communication difficulties that we always did. As the ATP paper here points out, while a digital system may support integrated and reflective working it's the thinking behind it that counts, not the delivery system.

All of this has led me to a series of questions about how we can understand and support fieldwork as an intellectual practice. These questions structure the rest of this discussion: When we speak of the subject object divide, who is the subject? What is the field and how does it differ from other realms of knowledge? What are the intellectual processes involved in fieldwork? How can we judge when fieldwork has been successful? Finally, what are the power relations that determine all of the rest?

## The Subject

Of course everyone starts out in the field, and many of the "movers and shakers" within archaeology either conduct regular fieldwork or wish that they did. As Yarrow has pointed out (2003), and many contributors here have explored, archaeological sites produce archaeologists as surely as they are produced by them.

But we still identify field archaeologist as a specialism or a role. There is often a divide between field archaeologists, especially those in the commercial sector and other archaeologists, whether specialists or academic synthesisers. There is a perception that those who do fieldwork are not equipped for theoretical debate.

This can vary country to country. Carver (this volume) argues that "it is almost impossible to discuss some of the finer nuances of Single Context Planning or reflexivity in Germany, where field methodology and excavation technique are subjects for *Techniker*." While in Sweden Lönn (this volume) notes that over the

last 20 years “the field-archaeologists became better educated. Today there are many archaeologists with PhDs working in Swedish construction archaeology”.

It also varies over time. Carver (this volume) goes back to Pitt-Rivers to talk about the status and training of diggers “from 10 to 19 men were employed in the excavations, consisting chiefly of men of the neighbourhood, who happened to be out of employ, and who consequently could not be expected to prove themselves amongst the most efficient of their class. No more useful organization could be established for archaeological purpose, than that of a permanent Corps of efficient workmen (Pitt-Rivers 1892:23–24).”

Clearly Pitt River considered himself as separate from the workmen he describes, and even if they were to be trained they would stay as permanent workmen. The current practice of archaeology has more a sense of a meritocracy, anyone digging on a site can hope to be directing excavations at another point in their career. While this promotes the sense of equality which is crucial to reflexive methodology, it does imply that digging is a ‘junior’ activity. There are few people who remain as excavators throughout their career, not least because the wages are so poor that those who cannot get a better paid job in archaeology tend to leave (Aitchison 2004).

This change over time is related to changes in the economic position of archaeology. Thorpe demonstrates that British field archaeologists before the advent of competitive tendering were engaged in just such nuanced discussions as Carver would wish, but the economic drivers of the current system undermine this intellectual strength.

One of the most important findings from our work at EH is that the subject in the subject/object divide of fieldwork is not an archaeologist, but a team. Yes, the experience of fieldwork and the material that we encounter construct us as archaeologists, but they do so by constructing us as members of teams. So the information infrastructure that is often called a ‘recording system’ is in fact a communication system that allows a team to share its experiences and observations during its creation of an understanding about the work it has undertaken.

Teams are strange things and it is quite difficult to design systems that support them well. A team’s goal may cut against individual goals. The power relations within teams are central to their success or failure (Shirky 2003).

A recent collection of ‘team photographs’ mounted on Facebook by BAJR (BAJR Federation 2011) strikes a chord in this respect. They remind me of wedding photos. Some, where the hierarchy has been particularly strong, even have that sense of Bride or Groom and the bridesmaids. The also have echoes of school class photos, even when they include no students or teachers. But in all cases they are there to remind us of an important event in the lives of those people that was formative of thought, social relations, and identity as an archaeologist. It is evidence that they were there. Often that evidence is lacking from the formal record, except in the form of this group shot, usually taken at the end of the excavation. Even seasoned archaeologists, who are reluctant to be photographed will line up for the site photo. The practice may be less common as commercial archaeology changes the site dynamic, as Thorpe describes in his paper.

As Thorpe argues in this volume, in Britain field practice has been a collaborative endeavour, stemming from discussion between colleagues more than from canonical

texts. As such it is particularly susceptible to generation politics; ‘it wasn’t like this in my day’ contrasted with ‘if only we could fix this outdated system’. It is an area where the new generation of practitioners will always have improvements and criticisms. The extent to which these are linked to changing understandings of archaeological theory is another matter. More importantly, the extent to which the discipline is open to new ideas is conditioned by larger political processes.

But does the team photograph capture the subject that is considered in the subject/object divide considered in this book? There are other subjects, other people who are part of the absent presence in fieldwork. Jones and Richardson no longer consider themselves to be fieldworkers, but their new roles in outreach and development control play an important part in fieldwork. When we think about the subject we need to think of this broader web of people as well. Leone et al. explore this broader sense of ‘the subject’ in the way in which comments on their blog influenced the design of their exhibit.

## The Field

Archaeology has often been described as doing a jigsaw puzzle where you don’t have all the pieces and you don’t know what the picture looks like. In most cases people seem to want to start with the edge pieces – defining the boundary of what we are prepared to work with is essential. Sometimes this involves physically bounding our work, laying out trenches as discussed by Jones and Richardson. Sometimes this is done by putting a conceptual boundary, in the way that Carman and Carman have chosen to study battlefields; “by choosing to examine landscapes that were used for a very particular kind of purpose in the past makes the identification and examination of differences in attitude and expectation as revealed by differences in use more reliable” (Carman and Carman, this volume). In both cases the field is defined by our observation of it, our choice as archaeologists to make it archaeological. Interestingly this is reflected by the notion of Archaeological Interest as defined in PPS5 where it is understood as the “interest in carrying out an expert investigation at some point in the future into the evidence a heritage asset may hold of past human activity”, not the information that might be derived from such an investigation (DCLG 2010).

But identifying material as archaeological, as worthy of archaeological observation is a theory laden practice. Lönn describes a situation where a certain type of site was not identified *as a site* partly “because of a lack of comparisons, this was a unique feature. The process was also cut off because of a theoretical trend that did not favour cultic interpretations” (Lönn, this volume). The features are not observed if there are not interpretations because they are not distinguished as interesting. It doesn’t really matter if the interpretation is ‘right’, just that there should be an interpretation – something to hang the observation on. Lönn concludes in relation to another feature type that anthropological analogy “made it possible to think of such an interpretation and adjust the excavation strategy and methods to test the hypothesis” (Lönn, this volume). Once we know what we are looking for we know how to look.

In addition to defining the field some of these papers lead us to consider its ontological status – resource, sacred space, zone of control. Leone et al. define their field by an ideological practice having been pursued there, stating, “this is how we see the “SL” exhibit itself, with its purpose in showing Annapolis as a place where liberty has been sought and produced” (Leone et al., this volume).

Wilkins also sheds light on the ontological status of the field in considering the question of sampling versus total excavation; “but digging larger quantities also entails larger costs, and if this work is undertaken in the public interest then it begs the provocative question of which approach is better value?” (Wilkins, this volume). The answer relates to the purpose of fieldwork. If the purpose of developer led archaeology is preservation by record then sampling only preserves a portion of the site. If the purpose is, as PPS5 and the sentiments that remain in the NPPF assert, to enhance the significance of the site, then sampling may well provide ‘better value for money’. Does the field have innate value, is it divisible, is it always better to dig (or record) more?

Both positions consider a site, or the field to be primarily a source of information about the past (or in some understandings a source of the past itself). For Carman and Carman the field exists in the present and the understanding the past is not the primary purpose of fieldwork; “The purpose of the Bloody Meadows project is thus not so much an attempt to recreate what an individual battlefield was like on the day of battle (or indeed the events of the battle). It is rather to establish a meaning for the historicity of the place in the present: hence our simultaneous concern both for an understanding of the nature of war in the past and preservation and public interpretation in the present” (Carman and Carman, this volume).

While the field may be anywhere that archaeologists observe the world in a formal fashion, some places are classified as more archaeological than others. Carman and Carman raise the question of who perceives the field to be archaeological. Considering the low visibility of battlefield remains they note, “Since such remains are generally invisible to the naked human eye, however, the landscape of such places has been seen as ‘empty’ of archaeology and therefore available for other uses.” This contrasts with the ATP experience of digging a Scheduled monument – which has been previously and even legally defined as archaeological.

Yamin extends the question of what is and is not the field; “The most unusual find on the project was not found in the ground. A handwritten diary kept by 17-year-old Rachel Van Dyke between May of 1810 and July of 1811 is in the Special Collections at Rutgers University and Tod Benedict, one of the field directors for the archaeological project, ran across it while doing standard map research after the excavation was completed” (Yamin, this volume). The library, the museum, the archive are the standard counterpoints to the field. Anything can be the field as long as it is not one of those spaces, because the information in them has already been constructed by ‘us’. The field is a shorthand for primary observation.

But the field is also a shorthand for freedom, both a sense of freedom that many people experience just from being out doors but also a conceptual freedom. Seeking Liberty, the exhibition described by Leone et al., included aspects beyond the museum, evoking a sense of the field. “Implementing technology-based touring allowed for a broader audience and permitted the exhibition to expand beyond the

physical walls of the museum” (Leone et al., this volume). This promotes the idea that the field is less controlled by our agendas. But what this also does is extend the boundaries of the museum, which in many ways mirrors what other contributors are arguing about the process of research in the field. By bringing our structures of practice and thought to bear on the field we are domesticating it in significant ways.

## The Process

Non archaeologists often conceive of fieldwork as the essence of Adventure. Most archaeologists dismiss this both as a motivation and as an experience. Indeed in time pressured excavations people often feel an acute anxiety described by Jones and Richardson; “when a site’s stratigraphy is difficult to read and it is unclear at what depth the first archaeological horizon or ‘natural’ appears, it can be very uncomfortable and disorientating. Yet, for some at least, the unearthing of a field drain can ease the situation and part of the processes that render the complex layers of soil in a definable area into a trench and hence comfortable” (Jones and Richardson, this volume).

Leone et al. present the interpretive process as naturalised within standard Western ways of seeing. “On this basis alone, archaeologists were providing a first-hand look to tourists and Annapolitans of what the city was like during the early part of the eighteenth century when it became Maryland’s capital.” They continue by suggesting, “All five of these sites unveiled important information about disfranchised populations in Annapolis” (Leone et al., this volume). Archaeology within tourism is often presented in this unproblematised manner – ‘step back in time’, ‘come and have a look’. It glosses over the discipline involved in looking. A casual glance at an excavation will not allow you even a glimpse of a city unless you have learned how to look. I am not saying that only archaeologists can do this, but that looking in this way is a disciplined performance that forms the core of our work. Carman and Carman describe seeing with an archaeological eye. Cobb et al. describe splitting the world into subject and objects. What is it that distinguishes archaeological fieldwork from other ways of being in the world? Excavation is obviously digging, but what distinguishes archaeological digging from other digging is largely the creation of a ‘record’. Similarly, a walk becomes a survey once it is recorded, especially if a system of recording is used. Many of these papers look at recording systems in detail to illuminate and improve the thinking that goes into their use.

Jones and Richardson, however, are critical of these systems which, they argue, can undermine our intellectual curiosity. “Thus a series of operational boundaries, in this instance – standard methodologies applied to excavation, are set up and are the means where the unfamiliar is made familiar or comfortable” (Jones and Richardson, this volume). Carman and Carman also stress the importance of the unexpected, paying attention to things that do not fit with our understanding. “It is, we believe, the *dysfunctional* behaviour (that is, the apparent mistakes or omissions) which can give a clue to cultural attitudes and expectations of the battlefield space which differ from our own” (Carman and Carman, this volume).

Thorpe and Carver point out that interpretation, the recognition of the archaeologist as subject is not a product of recent theoretical concerns but rather a core part of the archaeological project. Describing single context recording Thorpe describes reasoning skills “in which, intuitively, *dialectical materialist thought is brought to bear* on objects or attributes of observed phenomena which seem to contradict one another, in effect “bringing together the conflictual and contradictory, linking theory and practice” (Lefebvre et al. 1996:10)” (Thorpe, this volume).

Thorpe sees this intellectual capacity develop through an archaeological apprenticeship. “Rather than being bound by an authoritative canon of texts, archaeology is characterised by networks of personal interactions (see also Carmen 2006 at site level) and connections through which approaches and experiences are disseminated” (Thorpe, this volume). Lönn also recognises the way that this process of learning is essentially personal and social. “Knowledge like this differs from area to area and is often considered so simple and obvious that there is no need to write it down. It is only passed on from one archaeologist to another when being in that landscape” (Lönn, this volume).

Carver points out that description as an interpretive act was one of the early features of archaeology. In contrast ATP highlight the traditional separation between description and interpretation on context sheets and express a concern that this marginalises interpretation in the sense of fitting the context into broader narratives about activity on the site. In my experience, these boxes are prompts. Without a separate prompt ‘Interpretation’ would be even more easily skipped.

In my experience most people are very happy to talk about the interpretation of something that they are digging, but less happy to write about it. This is partly due to power relations. Many excavators think that it is someone else’s job, or are worried that their interpretation will be overridden in any case. Yamin describes this in discussing the idea behind the vignettes that she uses, “I also thought perspectives other than my own would add to the interpretation of the site, an idea that met with some resistance” (Yamin, this volume).

What is more, the interpretation field on a context sheet can always be added to, and modified, whereas only the person who experienced the context can describe it. We can and do change our interpretation based on comparison, further analysis or simply more thought. It is important to highlight that interpretive practice that has always happened ‘at the trowels edge’ but it is disingenuous to suggest that it stops there, or that we should privilege that interpretation above all others.

Yamin extends the process of fieldwork by examining the ways in which writing develops our understanding and interpretation beyond observation and analysis. “The process of writing vignettes is a process of discovering what you know and, importantly, what you still do not know and I wanted team members besides me to have the experience” (Yamin, this volume).

Jones and Richardson point to the central role of value judgements in the way we conduct our work. Using the example of field drains they demonstrate that archaeologists constantly make decisions about what not to record. The impact of these judgements concerns them. “However, what if, on one occasion, in order to reduce the site into a known comfortable state, the archaeologist made incorrect

value judgement, as a consequence an important but now destroyed element of the trench has been lost” (Jones and Richardson, this volume). Their concern reflects a belief that there is a correct judgement to be made and we have the responsibility to make it. This process of judgement lies at the core of recent changes to heritage protection in the British Planning system. It remains to be seen how a discipline long wedded to ‘objectivity’ will respond to the challenge.

Lönn also points out that the recording system, and the written communications that flow from it capture only one sense of what fieldwork is “There are the discussions at the excavation, about a feature which has been seen or maybe the showing of a picture. There is also the telling about earlier finds when something strange turns up at an excavation; or the call which is made to a colleague, who you know has seen something like the feature that you have just uncovered. There are also the talks around the coffee-table in the office. These pieces of information are personal, visual memories of individual archaeologists informing other archaeologists in the vicinity. To take the next step, there are small seminars and larger conferences, power-points shown at a lecture and so on. All this is verbal information and therefore temporary and the receivers are individual archaeologists or small groups” (Lönn, this volume).

Fieldwork is a social endeavour. In addition to the social relations of the team, visiting other excavations is an immensely important aspect of the intellectual practice of fieldwork. Lönn describes this in relation to the way in which a particular type of feature came to be identified and understood. “The archaeologist in charge, Eva Schaller Åhrberg, had not seen anything like it before and not knowing what it was, she asked around. Many archaeologists working in the area came to see it and discuss the find, but nobody had any idea how to interpret it.” She identifies the importance of *seeing* material – a visit creates “A visual memory and a mental preparation for the next find. The archaeologists that see and discuss this feature acquire a picture of what it might look like and in this way gain a visual memory of a possible hut” (Lönn, this volume). Leone et al. saw the same process in relation to caches or bundles “We could find them because we had made an effort to isolate the characteristics of their contents and placement after discovering our first one around 1990, and we subsequently looked for them systematically whenever we worked” (Leone et al., this volume).

The social memory is also important; who you saw it with, what the dynamics of the debate were. This shows in Lönn’s comment “Since the find was excavated more than 15 years ago, the memory is fading, which is sad. The chance to remember and relate to this find, when a new one comes along, is less even if there is an excavation report.” The wider social and intellectual context of the archaeologists affects the frame they see through – even if there is an excavation report. Partly because the excavation report will describe the find but not the uproar and it is that charge which makes it memorable, makes it something which can be recognised somewhere else.

So the archaeological eye is not a static, isolated thing but something that develops and changes through experience, debate, and social interaction. But that present experience is rarely discernable in our accounts of our work. Carman and Carman bring their experience to the foreground stating, “ours is not a search for an experience of

being in the past, but rather an experience in the present which simultaneously reflects and derives from the contribution of history to a particular place” (Carman and Carman, this volume). It is this experience, in the present with one eye on the past that constructs us as archaeologists. Leone et al. found that their work led them to a fuller understanding of the present. “This find made us think that as archaeologists we had not explored the greater presence of Muslims in Annapolis. We also had never asked before whether any Muslims live in the city today” (Leone et al., this volume).

Cobb et al. struggle with fieldwork as a process which “did not *reveal* a world divided into subjects and objects but rather *produced* one (Yarrow 2003: 67).” They are concerned about the retro-diction of our underlying world view onto the past. “In dividing the world into subjects and objects we risk colonizing the past with a particular way of looking at the world that we can loosely term modern” (Cobb et al., this volume). In this argument it is clear that for them, as for many contributors, the purpose of fieldwork is understanding a past separate from the present. Yamin, however, reminds us “that the past is not there to be dug up like a natural resource, it has to be constructed and a lot of the constructing goes on in the field” (Yamin, this volume). We will always see the world with a modern eye and archaeological fieldwork is inescapably a modern practice.

## Success Criteria

Recognising good fieldwork is important. The under theorisation of fieldwork leads to a perception that all fieldwork can be used in the same way. Fieldwork produces data that are used for synthetic work. It is the synthetic work that draws critical attention. The contributors here have identified a range of ways of recognising good work.

Carver sees the purpose of excavation as proving (or disproving) hypotheses and is dissatisfied with work that cannot be used for independent assessment of this proof “So long as documentation is interpretive and/or incomplete, we still only have Prestwich’s solution as our ultimate authority: *I was there, I saw that, so – in the words of Indiana Jones – “trust me”*” (Carver, this volume). Other contributors, such as the ATP see this personal basis of knowledge as the core strength of archaeological understanding.

Carver is also concerned with data quality – which for him reflects the accuracy of observation. He examines the adage, ‘excavation is destruction’ (with a humorous audit of its use in one piece by Hodder) “It almost seems that this, [...] has become a mantra we repeat to ourselves over and over again in an attempt to convince ourselves that it’s true, in part to avoid close examination of the second problem, that of data quality. *Who* recorded the evidence? *Who* did the digging and *how* did they excavate?” (Carver, this volume). The mantra about destruction overlooks what we create. The evidence of us is there all over the site, not just the absence of the evidence we have ‘removed’. We do run into the evidence of other people’s excavations quite a lot, and they rarely match with the records that were made of them.



Standards provide a key mechanism for archaeologists to judge successful fieldwork (English Heritage 2006; Willelms and van den Dries 2007). Wilkins associates the drive for standards with the economic necessities of competitive tendering. “Codification, structured management systems and objective methods were driven by the needs of a market created through legislation to operate a level playing field in the public interest (Adams and Brooke 1995: 96)” (Wilkins, this volume). Jones and Richardson also note that “By the practitioner making themselves comfortable, they can, in theory, do the job more quickly and easy” (Jones and Richardson, this volume). Once again, for these authors, standards facilitate our work rather than forming a framework for assessing it.

Wilkins also refers to the receding concept of preservation by record. Fieldwork is considered successful if it creates a ‘complete’ record of the site. He argues that the superfluity of data is used to justify an avoidance of theory – ‘yes we’ll get to that once we finish cataloguing all of this’. “The concept of an objective past, ‘preserved by record’, continues to justify the collection of ever-increasing amounts of data, on the proviso that “if enough records are made and sufficient phenomena observed, we will experience some kind of enlightenment (Bradley 2006: 6)” (Wilkins, this volume). In Britain the replacement of PPG16 has sounded the death knell of preservation by record but a new field practice based on significance is at the best nascent.

Standards can provide an internal judgement of good fieldwork but there are broader considerations. Lönn argues that fieldwork is judged by its capacity to contribute to wider understandings of the world. “In Sweden there is an ongoing discussion about how to direct the enormous amount of new material each year into certain fields of investigation i.e. how to make it conform to knowledge in a broader sense” (Lönn, this volume). Carman and Carman also judge the value of their work by the way in which it can expand other fields of knowledge. “By focussing on such less spectacular and less historically significant events we gain a different kind of insight into war in the past than from much military history” (Carman and Carman, this volume).

Part of the way that we judge fieldwork is by its outputs, most notably site reports. But as Yamin bravely notes, “Our reports are, indeed, notoriously dull, but even more serious is the fact that they so rarely convey anything worth knowing. Although we feel we are learning something important when we are in the field we manage to eliminate whatever it was during the analysis” (Yamin, this volume). Does the fault lie in the fieldwork, the analysis or the conceptual gap that we create between the two? The vignettes that form the core of that paper are compelling. The sense of verisimilitude comes from the skill in writing rather than the volume of evidence and its hard to know how they came to those interpretations – so this contrasts with the other contributors who are calling for greater transparency, but of course it is not an either or. Also this kind of writing takes real reflection and editing, not just reflexivity. By nature it is not something that happens ‘in the field’ but it is still part of fieldwork. And this duration of fieldwork beyond the time in the field is quite important. Yamin also takes the characterisation of audiences by their preferred reading style to task, “We say we need different reports for different audiences, but what we really need is one report that expresses our finds in different ways” (Yamin, this volume).

In Yamin’s vignettes the affection that these people have for the site means that all the characters are painted sympathetically; they aren’t racist (despite the author

acknowledging racism); they are social and loving and know their limits; they want the best for their businesses and families. In contrast Yamin (as the PI) “talked about the transformation of New Brunswick, from an aging industrial town with character to a sterile corporate home-base for J & J, a story that involved the burning down of most of the old buildings and the intentional dismantling of a National Register Historic District (Yamin and Masso 1996)” (Yamin, this volume). So this was a narrative that addressed current power structures, and it sounds like it was less sympathetic. The overall tone of the suite of vignettes is romantic and, as so often, is focussed on loss.

Good fieldwork creates an emotional response. In addition to the romantic metanarrative of loss the emotional reaction is often excitement, but it can take other forms. In relation to features identified as the result of slash and burn agriculture Lönn states, “There was an enormous discussion about the interpretation, almost uproar” (Lönn, this volume). Many of the contributors also talk about the importance of surprise as a core part of the process of fieldwork. This emotional attachment to the process is so recognisable – at least recognisable as good work.

Leone et al. accept that there may be no real measure of successful work, but they judge it by its effects in the world. “We also understand that there is now no way to track the results of our work’s success except through the enfranchisement some feel because of our work, and the opposition of others” (Leone et al., this volume). They are interested in how other people can appreciate their own role as a subject creating and being created by the materials which have been created by archaeologists and have in turn created people as archaeologists.

## Power

In the end it is all about power and most of the time power is about money. But there are other forms of power as well. Fieldwork is at the heart of archaeological endeavour, it is one of its defining characteristics. Yet, in some very important ways archaeologists who work in the field are outsiders. From the literal fact that many archaeologists who work in the field never come into the office, to the lack of access to the intellectual capital their labour underpins in the form of scholarly discourse, exhibits, etc., few fieldworkers are powerful within archaeology.

Empowerment will not come from theory, but from the conditions that we work in. Yamin describes these conditions very clearly. “Cultural Resource Management (CRM) as practiced in the United States is, like pretty much every other business, inherently hierarchical. The people at the top do the decision making and the people at the bottom do the manual labor. The people in the middle, the ones who run the field projects and generally write up the excavations, are not expected to do much more than describe what was found and, of course, how it was found, the familiar methods and results sections of a CRM report. Interpretation is generally done by the principal investigator (the P.I.) with input from field personnel to the extent possible and from impressions gained on field visits in combination with the results of the historical research and whatever theoretical perspective the P.I. deems appropriate” (Yamin, this volume).

A post-processual recording practice cannot overcome the real disparity of power in the world of commercial archaeology. This disparity of power, however, can undermine the progress of all theory. As the ATP point out “It illustrates clearly that *any* reflexive technique, from video recording to hand-written reflexive site diaries, can only be productive when excavators are empowered and recognised as decision makers and not merely data retrievers (Chadwick 2003; Edgeworth 2006)” (Ardnamurchan Transitions Project, this volume).

The strong focus on power and relations in the ATP paper lets us see the technology of excavation for what it is “The [participant] sheets themselves were thus caught up in the power relations that surround us, they were part of the material culture that we as archaeologists produced, even when filled in by students.” But even materiality and desire cannot destroy power structures that actually still exist “Perhaps the most predominant was that of students often writing what they thought we would like to hear. Clearly this only perpetuated exactly the kind of problematic power structures we want to undercut.” and “Another problem was that we intend to make the completed participant forms freely available online as part of the site archive, and this obviously meant that people were reticent to express their real experiences in some cases” (Ardnamurchan Transitions Project, this volume).

Many of the papers have considered power relations within a team and within the discipline. But archaeological fieldwork also operates in wider structures of power. In comparing the different archaeological traditions of Britain and Ireland, Wilkins looks at the ways in which the practical decisions of fieldwork are driven by the political position of archaeology within society. Recording methodology reflects and creates both theory and politics. He points out that “The archaeological market is an artificial creation that exists because the state wants archaeological information and creates legislation with which developers must comply” (Wilkins, this volume). But he leaves it to the reader to consider why the state wants archaeological information.

Leone et al. consider these wider relations of power to be the key driver and proper location of archaeology “Different from language, things are likely to hold more meanings and can add or lose meanings. Key here is the revolutionary works of Binford (1983), who stressed the ability to recover these meanings in archaeological things. This means that past peoples and oppressed groups have visibility and an occasion to provide opinions, critique, and opportunity for themselves” (Leone et al., this volume).

The contributors here bring intensity to discussions about the details of fieldwork because it is constitutive of their identity. And it is more fiercely fought the less powerful you are in the archaeological hierarchy because it is something that you may be able to influence while the Planning Policy is completely out of reach.

## The Value of This Volume

While archaeologists never tire of talking about fieldwork, and how it *should* be done, it is notoriously difficult to bring volumes about fieldwork to press. The Interpreting Stratigraphy series of conferences have faded out, and the editors of

this volume have shown admirable perseverance in bringing this collection to publication. One reason for this is that the people who are most interested in, and qualified to write about, the practice of fieldwork are usually too busy conducting fieldwork to write about it. This is exacerbated by the fact that, as Thorpe points out in this volume, the system of competitive tendering in commercial archaeology in the UK means that writing about anything ‘non-billable’ cannot be justified in commercial work programmes and so is left to academics (who do less fieldwork) or to the mythical ‘free time’.

It is because the field acts back on us and because our fieldwork constructs us as archaeologists that we care so much about it. Often this group of contributors would not even speak in the same session so it is good to read a volume with a big divide in it without a desire to emphasise the divide (or resolve it).

What really distinguishes these papers is that all the authors are passionate about fieldwork as the pre-eminent site of archaeological thought, the heart of our enterprise. They believe it really matters how you do it. Perhaps some of the passion of these papers (and indeed of many archaeologists) is constructed by our experiences of being in under funded over pressured fieldwork projects that are expected to hold the whole weight of the discipline.

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