

## Chapter 2

# How to Archaeologize with a Hammer

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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 Klonk [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 "anti-quarian 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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