A Paradigm Shift in Classical Archaeology?

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This article, a revised version of the 13th McDonald Lecture given on 21 November 2001, sets the recent and partial transformation in the content and practice of Classical archaeological against the background of Kuhn's well-known work, first published in 1962, on paradigm and revolution in the scientific disciplines. Perhaps the most important question in this context — how would we know when a change in paradigm had taken place? — is harder to answer for a humanities discipline than for a science. But the attempt is made, first to set out a traditional paradigm for the subject; then to give examples of new approaches which seem to satisfy many of Kuhn's criteria for the introduction of a new paradigm; and, more briefly, to show that other approaches, innovatory though they may be, by their nature cannot bring about such a change. Whether a true paradigm shift has been set in motion, the future alone will show.

The first half of my title comes, of course, from that non-scientist's bible of the operations of the physical sciences, Thomas S. Kuhn's *The Structure of Scientific Revolutions*, first published in 1962 and since hailed, by the *Times Literary Supplement*, as one of 'The Hundred Most Influential Books since the Second World War'. I said 'of course', but that may have been a mistake. Note that I called the book 'the *non*-scientist's bible', and that it was *The Times Literary Supplement* that rated it so highly. These are hints that, quite apart from the fact that Kuhn's central propositions have become increasingly contested over the years (see for example Fuller 2000), his achievement anyway never acquired quite the same standing among practising scientists. In fact, a private poll that I have conducted has so far failed to yield a single one who has read the book: the explanation being invariably along those lines, so familiar from quite other contexts, that 'They're too busy doing it to want to read about it'.

So a word or two is in place about Kuhn's analysis of how revolutions happen in the sciences, noting his view that, on an initially small scale, they happen very frequently. As he later admitted in response to his critics (the best source for such elucidations and modifications of his original position is the 'Postscript – 1969' in Kuhn 1996, 174–210), he had used the word 'paradigm' in two senses. First, the sociological one, on which we shall mainly concentrate: 'the entire constellation of beliefs, values, techniques . . . shared by the members of a given (scientific) community'. Secondly, 'the exemplary past achievements' to which those members share in paying homage (Kuhn 1996, 175). You will notice how important the concepts of 'sharing' and 'community' are to both definitions. A 'paradigm shift' is what occurs when, perhaps at first in only one small part of one discipline, new beliefs, values and techniques are embraced. Such episodes are often preceded by a kind of crisis, which Kuhn defines very simply: 'the common awareness that something has gone wrong' (Kuhn 1996, 181). They are followed by a phase of competition, but this 'is usually quickly ended', one way or the other (Kuhn 1996, 177). Either the case of this innovatory sub-group is rapidly proved to rest on error or misunderstanding; or it quietly prevails, first in its own small corner, then more widely. In the latter case, a further consequence of such assimilation is that it often has the effect of creating a different kind of crisis elsewhere, in other parts of the discipline or in other disciplines altogether: nobody there had thought that 'something had gone wrong', the majority probably still don't, but eventually the new paradigm is shown to yield more

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robust results, with fewer anomalies, than the old one.

Really important paradigm shifts are ones which seriously interrupt the progress of the other main element of Kuhn's antithesis, 'normal science'. Normal science is predicated on the assumption that, broadly speaking, the scientific community knows what the world is like. Most scientists will spend most of their careers in extending the frontiers of that assumption, discovering new applications and solving residual problems. After a successful paradigm shift some of them, and eventually all of them in the areas affected, will switch to a completely different set of problems, and to completely different standards by which to judge their solution. The rules have changed, and the relevant part of the community gradually acknowledges that it had not after all known 'what the world was like'.

That is probably enough of my facile reprise of Kuhn's analysis. More pressing questions urge themselves upon us. First and most obvious, what has the practice of the 'hard sciences' to do with a humanities discipline? Can paradigm shifts occur in communities which, nowadays at any rate, would seldom lay claim to knowing what their subject-matter 'was like'? To historians, any such assertion about life in their chosen periods would be an invitation to debate, just as literary scholars would hesitate to accept it about the mind of their chosen authors. Is there a place for the paradigm in subjects which abound in competing schools, whose challenge to each other, far from being 'quickly ended', may continue for generations on end? To this last question, Kuhn had the ultimate answer: that his theses were actually 'borrowed from other disciplines', namely the humanities, which commonly see their subjects in terms of 'revolutionary breaks in style, taste and institutional structure' (1996, 208); his own originality, if any, consisted in applying the same concepts to the sciences which had been thought to behave quite differently. There remain, of course, important differences between the two cases: in the hard sciences, the old and the new paradigms are often absolutely incompatible with each other, as is seldom if ever true in the humanities; likewise, the necessity for common standards of validation is much more prevalent in the sciences. But that should still help to answer this first misgiving.

There are others, however. They include one whose point will become apparent presently: how wide does the field have to be in which the original paradigm shift is held to have taken place? Here Kuhn gives an equally explicit answer: it doesn't have to be wide at all. The change 'need not seem revolutionary to those outside a single community, consisting perhaps of fewer than twenty-five people' (1996, 181). What it does is to reconstruct 'the group commitments' of these people (notice again the stress on the factor of community) and in due course, if it is successful, those of others too. That sets to rest another source of doubt which I entertained when using the title for this article.

Then there is the all-embracing question: how would we know when a paradigm shift, in whatever subject, had taken place? I do not think that this is an insurmountable difficulty either. One obvious test is to take a long view of other people's disciplines and see how much they have changed since we last studied them ourselves. On this evidence, I think that Geography is the first example that comes to mind. Rather than falling back on vague definitions of the process, such as 'change in the way subject-matter is perceived and organized', or in 'the framework for thought and practice within the discipline', we can retrieve a more operational criterion, in one last phrase before we say good-bye to Thomas S. Kuhn: paradigm debates involve confronting the question 'Which problems is it more significant to have solved?' (1996, 110).

This is a quotation worth bearing in mind throughout: the debate that follows on an attempted paradigm shift, as Kuhn notes, will tend to involve progressively wider audiences. Thus, 'What would be most useful and applicable in your field, as a fellow-scientist?' 'What would you as a fellow-academic find more intellectually challenging?' 'What would most interest you as a member of the educated public?' 'What would *you* in the world at large find of the greatest benefit to humanity?' Francis Crick's alleged claim, at the bar of the 'Eagle' in 1953, that he and James Watson had just discovered 'the secret of life' (Watson 1968, 126), would have easily satisfied all those criteria of significance, as a genuine paradigm shift always would.

So we turn to one small humanities discipline, Classical archaeology, to see what has happened there. We have been looking only at the first of Kuhn's two senses of 'paradigm', but now I am going to start with the second, that of the 'exemplary past achievements'. No achievement in the history of the subject has more often served as an exemplary paradigm than the work of Sir John Beazley. And deservedly so: in his long life (1885–1970) Beazley achieved, on a gigantic scale and with near-complete success, what dozens of his predecessors and hundreds of his successors have undertaken, in smaller fields and

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with results that have never commanded quite the same authority. A sizeable sector of the 'normal science' of Classical archaeology has followed the paradigm first set up by Beazley's early papers of the 1910s and 1920s. There are many reassuring aspects of working by this kind of paradigm. Its progress is readily measurable and, in consequence, there is a striking degree of consensus about the value of individual contributions to that progress. Beazley exemplifies, to perfection, this consensus (see in general Kurtz 1985; for a critical re-assessment, Whitley 1997).

Even at the time when his studies began, the numbers of Athenian black-figure and red-figure vases in the world's collections, major and minor, were to be numbered in the tens of thousands (today there are more still). Beazley found a system whereby a substantial proportion of these could, for the first time, be definitively organized: that of associating each of them with the individual hand which had been responsible, not for their shaping, but for their surface decoration. Others before him had attempted this approach: mostly, they had started out from the fact that some of these painted scenes were actually signed with an artist's name. The trouble was that there were too few of these: they were to be counted only in hundreds, not nearly enough to establish control over the field. Beazley therefore set this criterion on one side, and concentrated instead on the purely visual technique of observing tiny stylistic details in each painter's method: the rendering of an eye-lid, an ear-lobe or a toe. These, he believed — not the conscious effects that the painter was aiming for, but the unconsciously-repeated features of his work — were the most tell-tale indicators of individual style. Several analogies have been drawn for this procedure, in the somewhat earlier intellectual activities of the late nineteenth century: most obviously, with the success achieved by Giovanni Morelli with unattributed Renaissance works, which it has never quite been proved that Beazley used as a model (this is contested by Williams 1996, 241–2); or, more laterally, with the rise of the science of detection, through which a criminal betrays his identity in the unthinking repetition of certain small details which give a pattern to his crimes, much as Beazley's vase-painters gave up their secrets to him (Beard 1986; Elsner 1990).

Thus Beazley assembled, by tens, fifties, in some cases a few hundreds, the works by each single hand. Where — as most often — no signed piece was among them, he had to invent names for the painters that he had discovered. Intellectually, an amazing aspect of this achievement was the visual memory it required: Beazley travelled assiduously to each major collection in Europe and North America, drawing and photographing; but it was obviously necessary for him to retain, for years on end, some means of visual recollection for each individual piece, until the time came to fit it into his framework. From this, it was for Beazley a secondary step, for everyone else a vitally important one, to arrange these artists in chronological groups. Sometimes Beazley could do this in the most direct way possible, by showing that a painter in generation 'Y' had actually learned his craft from an older predecessor in generation 'X'. But more broadly, each one of his artists could be grouped with their apparent contemporaries, by observing such features as common advances in anatomical rendering. Thus a whole, vast field was for the first time organized into a system applicable by many other scholars to their own work.

I should like now to establish a simple point, again exemplified (though on a huge scale) by Beazley. Before, during and since Beazley's time, traditional Classical archaeology has been defined in terms of subject-matter, of specific *classes of material*. These classes of material were, in a sense, already laid down for the discipline by external factors. There were geographical categories — Greek, Italian, Ionian, Sicilian, Athenian. There were (on an even more important and defining level) chronological ones — Archaic, Classical, Hellenistic, Imperial Roman. Then there were those largely peculiar to this branch of archaeology, the categories determined by *individual* past activity. In art-historical studies (always the most prestigious branch of the subject) these involved the handiwork of individual artists and schools. But there was also that category, only found in this branch of archaeology, which was determined by the writers of ancient texts, of which the outstanding example is (or was) 'Homeric archaeology', a branch of the subject whose whole *raison d'être* lay in the survival of two literary works, the *Iliad* and *Odyssey*, and whose primary aim was to throw light on their interpretation.

The traditional practice of the subject was to take one such class of material and (to employ the verb in commonest use), 'do' it. As often, an anecdote will save pages of description. At some date in the 1950s H.R.W. Smith, the revered Professor of Classical Archaeology at Berkeley and a former Beazley pupil, was interviewing a potential graduate research pupil, a young woman. Enthusiastic about her potential, he is said to have offered her this encouragement: 'With five years and a bit of luck, Miss X., I can make you the Miss Haspels of the

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Figure 1. *(© The New Yorker Collection 1999 George Booth from cartoonbank.com. All Rights Reserved.)*

Etruscan undecorated *oinochoai*'. The story has an excellent source and I find no difficulty in believing it to be true. What Smith was offering to his potential pupil was the status of a counterpart to Émilie Haspels, author of *Attic Black-figure Lekythoi* (1936). This was a considerable and much-admired piece of work. Haspels, a contemporary admirer of Beazley's method rather than a pupil, had applied that method to a large sub-category of Athenian pottery, the so-called *lekythos* shape. She had produced a typology of variant sub-forms, a chronological scheme, and a series of attributions to individual painters, with such success that, when Beazley came to publish his own *magnum opus* on black-figured pottery twenty years later, he was content to refer his readers to 'her fine work' (1956, viii), to omit the hundreds of specimens of the *lekythos* which she had listed and to adopt her attributions, except when he had something to add — from him, a unique accolade.

But 'the Etruscan undecorated *oinochoai*'? They can be beautifully-made artefacts but, as their name suggests, they lack surface decoration. This eliminated, at a stroke, some of the approaches used by Haspels or Beazley: the whole, multi-faceted approach of connoisseurship including, in all probability, attributions to individual hands. This would reduce drastically the potential impact of the proposed piece of work, and perhaps the young woman realized this. At all events, the sequel to the story is not without relevance to my theme: she changed her mind, withdrew her application and (it is said) married a Sergeant of Marines soon afterwards; but that is not the aspect of the story that I find so depressing.

One could describe the thinking behind Smith's offer, according to taste, as positivistic, empirical, conservative or purely cumulative. The classes of material, themselves the most traditional of concepts, were mentally arranged in a three-dimensional matrix of pigeon-holes, with the geographical as one dimension, chronological as the second, and descriptive category as the third. The Etruscan undecorated *oinochoai* were one of the (by now rapidly diminishing) number of empty holes. As the sub-divisions of the subject were pigeon-holed, so did its practitioners come to be. I myself, as a graduate student, worked on Greek military equipment; today, 35 years after I ceased to do so, I am still greeted at conferences as 'the armour man'. When my colleague Nigel Spivey was about to produce his prize-winning book *Understanding Greek Sculpture* — the title in itself shows some kind of rejection of the disciplinary tradition I have described — a foreign colleague is said to have protested 'But he's a pot man!'. But that is enough of anecdotal evidence.

That disciplinary tradition required you to do certain things, sophisticated yet of a largely descriptive nature, with your class of material. Prolonged reflection on the *function* of the artefacts was not necessarily one of these requirements; nor on what valuation had been put upon them in antiquity; nor on what lessons could be learned from their geographical description outside the centre of production; nor even on the vital question of the precise archaeological contexts in which they had been deposited, beyond the mere names of the find-spots. In short, the tradition gave short shrift to the whole *social* context (not to speak of the cognitive aspects) of the class of objects in question. Each contribution was destined to be an inward one, to other studies of other, parallel classes of object in a bracket of ancient time and space: nor was one often encouraged to cross even the minor, parochial boundaries between these classes.

Eventually, even Beazley's work in vase-painting studies began to be questioned, not for one moment for its accuracy and validity, but precisely for its value as a paradigm. To attribute 245 vases to 'The Berlin Painter', it was asked: what exactly does this contribute to the further development of the

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subject? One very obvious contribution, not intended by Beazley and not even lying within the field of academic research, lies in the benefit to the art market, where the individual attributions by himself and his successors have greatly enhanced the price of vases (see Fig. 1). But the widest and most useful application of Beazley's attributions was as a guide to the chronology of any kind of discovery, made in any part of the world that had contact, direct or indirect, with the ancient Greeks between the sixth and the fourth centuries BC: this world extended from the Yemen to southern England. If there was no decorated Athenian pottery in an excavation-find, there would almost invariably be other objects, which elsewhere had been associated with yet other objects, which could in turn be dated in terms of Beazley's classification. But for this purpose, a piece by a mediocre contemporary of the Berlin Painter would serve as well as one from the hand of this artist.

Even art-historically, what kind of figure is this Berlin Painter? We know nothing of his place in contemporary society, not even his name; we do not know that he was not a woman rather than a man; it has even been argued that his and his lesser colleagues' talents lay, not in the original creation of their figures but, rather in their skill in copying them from drawings of works in precious metals (Vickers & Gill 1994, ch. 6; and, for the ancient monetary valuation of this very painter's work, p. 86). In the last analysis, the Berlin Painter has not just his origin, but his only real existence as an individual, in the uniquely sensitive perception of Beazley.

Such questioning of ruling paradigms began to spread more widely, in a series of new orientations which will form the subject of the rest of this article. I will leave it to others to decide whether they constitute a paradigm shift, but I think I can show that they satisfy one of the more testable of Kuhn's criteria: they have certainly given rise to the posing of that question, 'Which problems would it be more significant to have solved?'. First, though, another general point. Earlier on, I alluded to the influence exercised by the ancient texts, and Classical archaeology is often stigmatized, by its many critics, as being 'text-driven'. In a superficial sense, this is palpably false: most Classical archaeologists do their work, and always have done, without directly consulting an ancient text for days or weeks on end. The entire corpus of work on Greek pottery, for instance, from before Beazley's time to the present day, was undertaken in defiance of the fact that there is scarcely a line in ancient literature referring to this field of activity. What the criticism implies is something deeper: that the subject takes its orientation from, and adapts its whole narrative to, the lead given by the literary sources. Thus the archaeology of Roman Britain has been built around Tacitus' narrative of conquest; the study of Greek art around the text of the Elder Pliny; the archaeology of fifth-century Athens around the narratives supplied by Herodotus, Thucydides and Xenophon; that of Republican Rome similarly around those of Livy and Diodorus; that of Sicily again around Thucydides; and most notoriously, that of Aegean prehistory and protohistory around Homer. There is something in this, even if it still conspicuously fails to cover the case of Greek vase-painting.

But there is a deeper level still. Traditional Classical archaeology is stated, this time I think with complete truth, to have directed its energies at those *aspects* of the ancient world on which the written sources, taken as a whole, throw light. Thus, on urban but not on rural life; on public and civic, but not on domestic activity; on periods seen as historically important, but not on the obscurer ones; on the permanent physical manifestations of religion, but not on the temporary ones — sacrifice, patterns of dedication, ritual meals, pilgrimage; on the artefacts interred in burials, but not on burial itself; on the historically prominent states — in Greece, Athens and Sparta — but not on what has recently been called 'the Third Greece' (Gehrke 1986); and so on. The decisive stage in the recent history of the subject came, in my view, when Classical Archaeologists began to ask themselves: why not study this aspect, not because the ancient sources throw light on it, but precisely because they do *not* throw light on it? The examples I shall give illustrate how, usually in combination, some of these Cinderellas have recently been brought to the ball: rural life, domestic life, neglected periods, dedications, burial, the more backward regions of Greece. There have also been changes in *method*: not only were new problems investigated, but they were approached in unprecedented ways.

Let us take, as an example of the 'new way', the project presented in Joseph Coleman Carter's *The Chora of Metaponto* ii: *the Necropoleis* (Carter 1998). The first thing to notice about this publication is that it was produced as part of the results of surface survey of the rural territory, or *chora*, of a Greek city in southern Italy. I shall have more to say in a moment about this technique of field survey which, thirty years ago, first began to offer a serious alternative to excavation, at least on the part of English-speaking archaeologists working either at home or

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Figure 2. *Map of the territory of ancient Metapontum, showing the relative positions of the city itself (right centre) and of Pantanello.*

overseas; and which, at least in quantitative terms, has now almost reached a point of parity in activity. In fact the outlying cemeteries at Metaponto were discovered as a part of the study of the city's land-allotment grid, first detected from the air; the main rural cemetery, at Pantanello, lines the sides of two of the main arterial roads on which the grid was based. Pantanello is 3.5 kilometres from the city (Fig.

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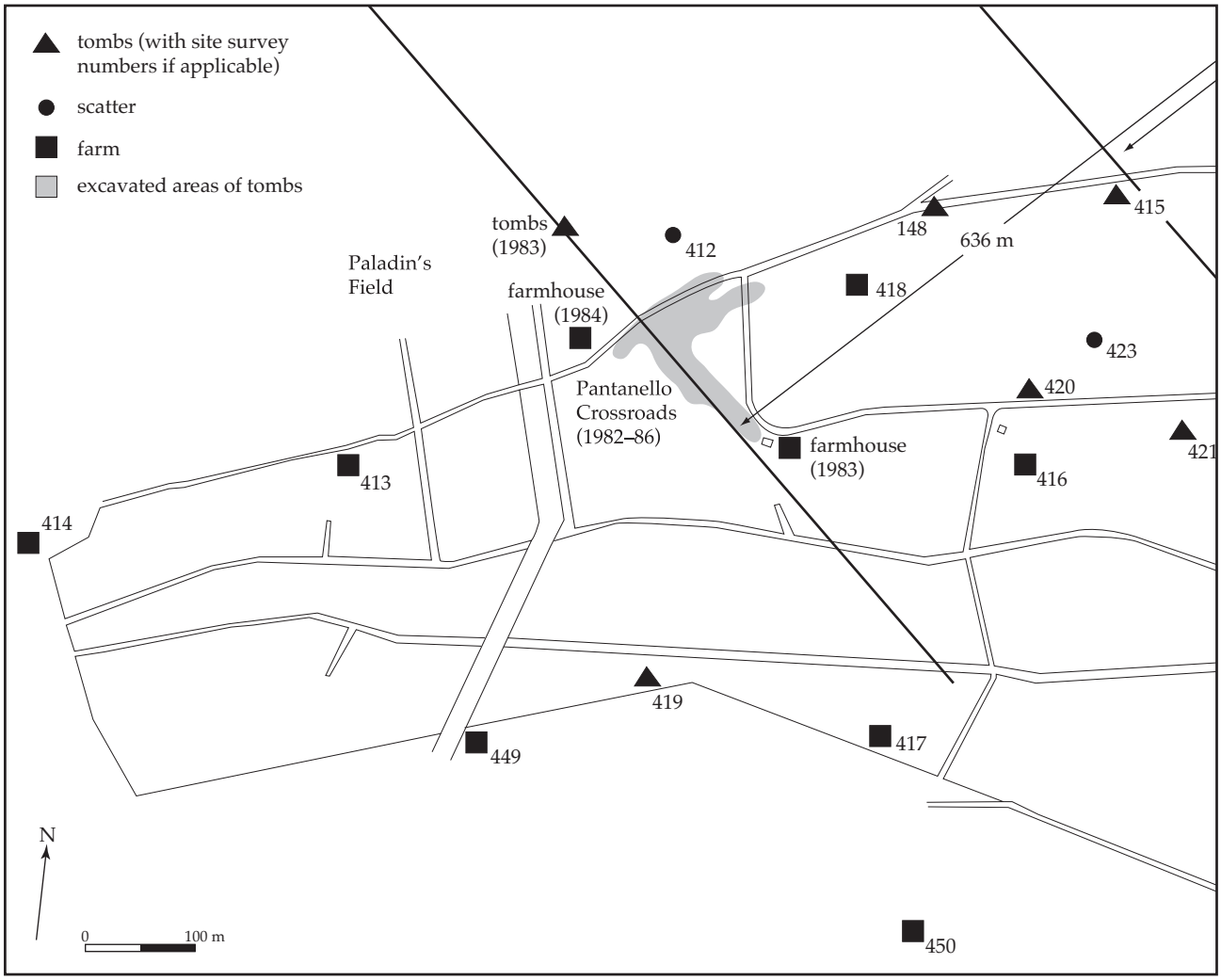


Figure 3. *The Pantanello cemetery (shaded) straddling one of the parallel roads of the ancient land-allotment grid (arrowed), with the adjacent farmhouses (square symbols).*

2): there are plenty of other cemeteries in what is the more normal location, on the edges of the built-up area, so some other group was buried here: who were they? The answer is pretty obvious: it was those forgotten people of Greek history, the country-dwellers — and, sure enough, their farmsteads were also found, spaced at approximately one per land-holding, all around the area (Fig. 3).

This was an inhumation-cemetery of about 330 graves, in reasonably good preservation, and perhaps the most interesting point is what was done with the skeletons. They were submitted for testing of their blood-groups and epigenetic traits, as well as for aging and sexing. An important detail is that this was done partly 'blindfold': that is, the biologists were not told of the archaeological indications of age, gender and date provided by the grave-goods, though they did know how the graves lay on the ground. In the event, it did indeed prove possible to distribute over 250 of the skeletons into some 45 small groups, each with biological linkages. This is an example of what I call the 'quantitative bonus' of Mediterranean archaeology: the greater size of sample forms a correspondingly better basis for inference.

Further, the relatively precise chronology (Beazley can take a posthumous bow) meant that these groups could be set out as family trees (Fig. 4), with a series of generations indicated with their approximate life-spans. The shaded rectangles here indicate the possible range of date for the *birth*, not the death, of each individual; gender is indicated by the white

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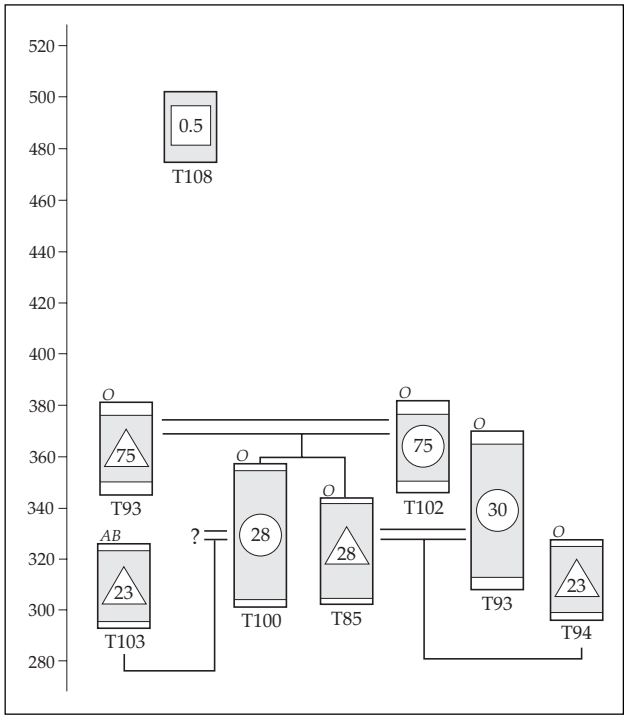


Figure 4. *One of the proposed family trees, N6.7, of the Pantanello burial group (see pp. 185–6).*

symbol, approximate age at death by the numeral within it. Thus in this illustration, we see what is taken to be a married couple, both born rather before the mid-fourth century BC and both living to a ripe old age. But their children fared less well: both the married son (buried as was usual with his family of origin, in Tomb 85) and the presumed daughter-in-law (Tomb 93) died young, while the grandson (Tomb 94) lived an even shorter time. More problematic is the case of the female, taken to be the daughter of the original couple, in Tomb 100: she is associated with a male (Tomb 103) who, for reasons of blood-group, cannot be her younger brother, and should not, in the light of the already-mentioned rule of patrilocal burial for married sons, be her husband. If he is then her son, why is she not buried elsewhere with her husband? Have we a case here of a child born out of wedlock, or of a husband killed far away in battle? In any case, both these individuals also died young, leaving the old couple to outlive not only their children, but even perhaps their grandchildren.

The most satisfying feature was that, when the family groups were plotted on to the cemetery, they proved to fall into fairly neat rectangles (Fig. 5), of standard dimensions, with little 'trespassing'. As well as being a remarkable objective vindication of the method, this last finding validated, at a stroke, one of the oldest hypotheses in the book of funerary archaeology: that closer groupings within a formal cemetery are indeed *family* groupings. Before this, parallel claims had been founded on much smaller samples; common sense had long since suggested that the hypothesis was a likely one; study of modern cemeteries, where over long periods families were often grouped in plots, had reinforced it; but for the more distant past, a hypothesis is what it had remained.

Of the many merits of this project — the skill of the excavation, the scientific objectivity of the skeletal testing, the exhaustive detail of the interpretation, individual by individual, the precision of the dating which made it all possible, and the relatively large sample size which makes it all credible — some you would expect in any good project, but I should be surprised to find the last two in any field of archaeology but the Classical. Yet the contrast with the traditional Classical cemetery report is also intense. No doubt there are still those who would have preferred to have the finds published on their own, set out according to the traditional classes and subclasses, ready to use, as they would once have been. But for anyone interested in the broader aspects of Classical Greek culture, I would guess that the Metaponto volume offers about a hundred times as much new and serviceable knowledge as a traditional publication. For good measure, the preservation of the Metaponto bones made possible one last touch: the re-construction of the faces of the fifth- and fourth-century dead (Carter 1998, 521–5).

But Metaponto lies fairly near the outer fringe of the Greek world, and the evidence presented in the report relates primarily, though by no means exclusively, to burial practices. So what about something much nearer home, and throwing light on the living rather than the dead? Here again, the new fashion for rural surface survey during the 1980s and 1990s can come up with exciting finds. In the far south of Attica, near the tip of the peninsula at Sunion and only a little over 20 miles from Athens, the German archaeologist Hans Lohmann carried out a one-man survey (Lohmann 1993) of an unusually dry, hilly stretch of terrain. It would be interesting to know what, if anything, his more conservative seniors had expected him to find: some fortifications, perhaps, a few graves, and the odd rural sanctuary? In fact his most important discovery was of a whole network of isolated farmsteads, less regularly laid out than their Metaponto counterparts, as one might expect in such terrain, but concentrated into a simi-

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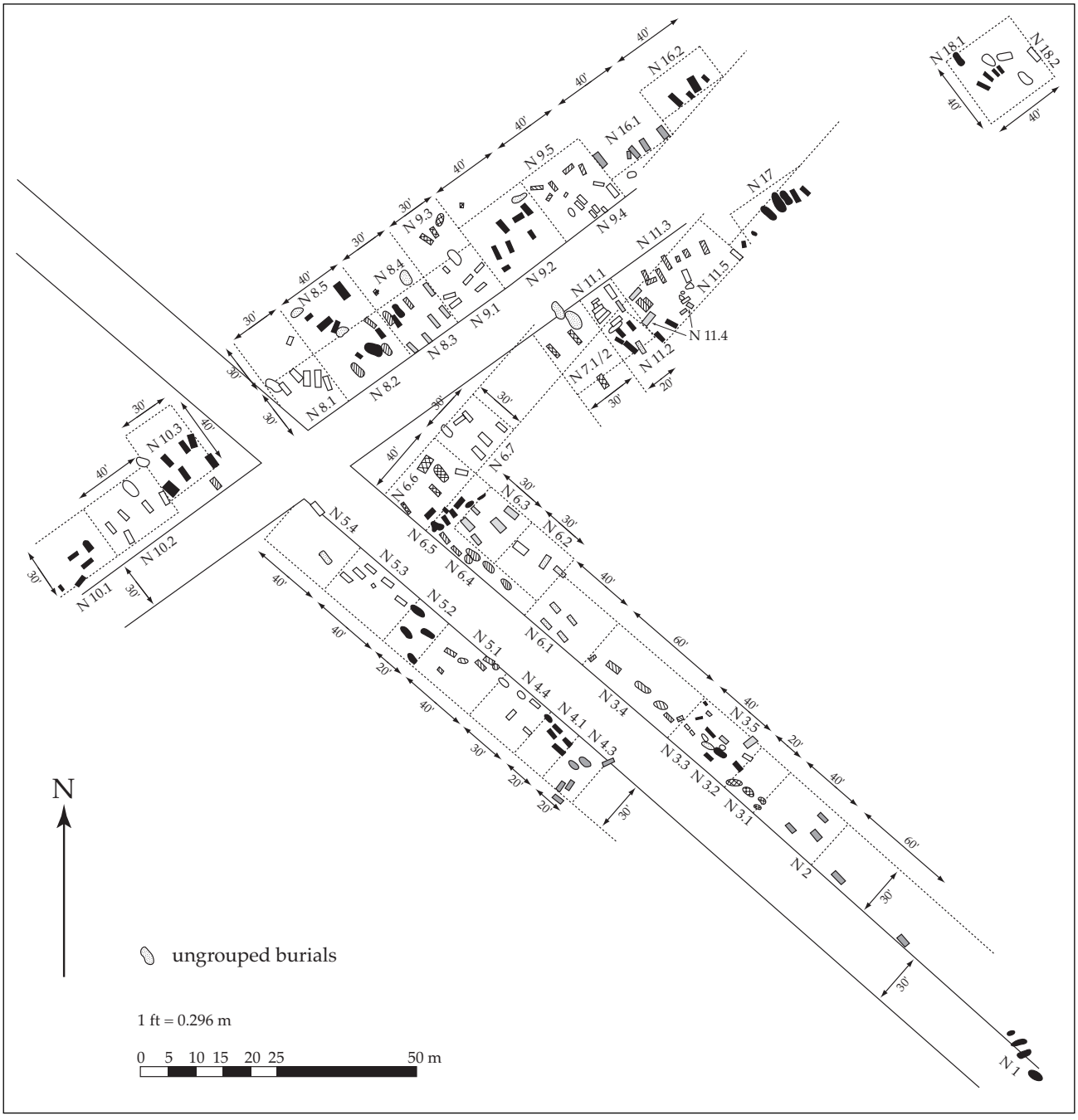


Figure 5. *The Pantanello cemetery divided into family plots. The groups of burials shown as differentially-shaded rectangles are each biologically linked (N6.7 can be seen right of the crossroads, with one member 'trespassing' onto the plot of N6.6).*

lar period: mainly the fifth and earlier fourth centuries BC. This indeed seems to have been one of the the few periods in its whole history when it was thought worth cultivating this impoverished land at all intensively, a testimony to the burgeoning population of Attica in these years. Mainly as a direct result of this later neglect of the land, Lohmann found clear traces of the farm-buildings still visible above ground, together with a number of their appurtenances (Fig. 6): terracing, threshing-floors, boundary-walls, even boundary-inscriptions, and once again burial-plots, but this time separate ones for each farm, not grouped

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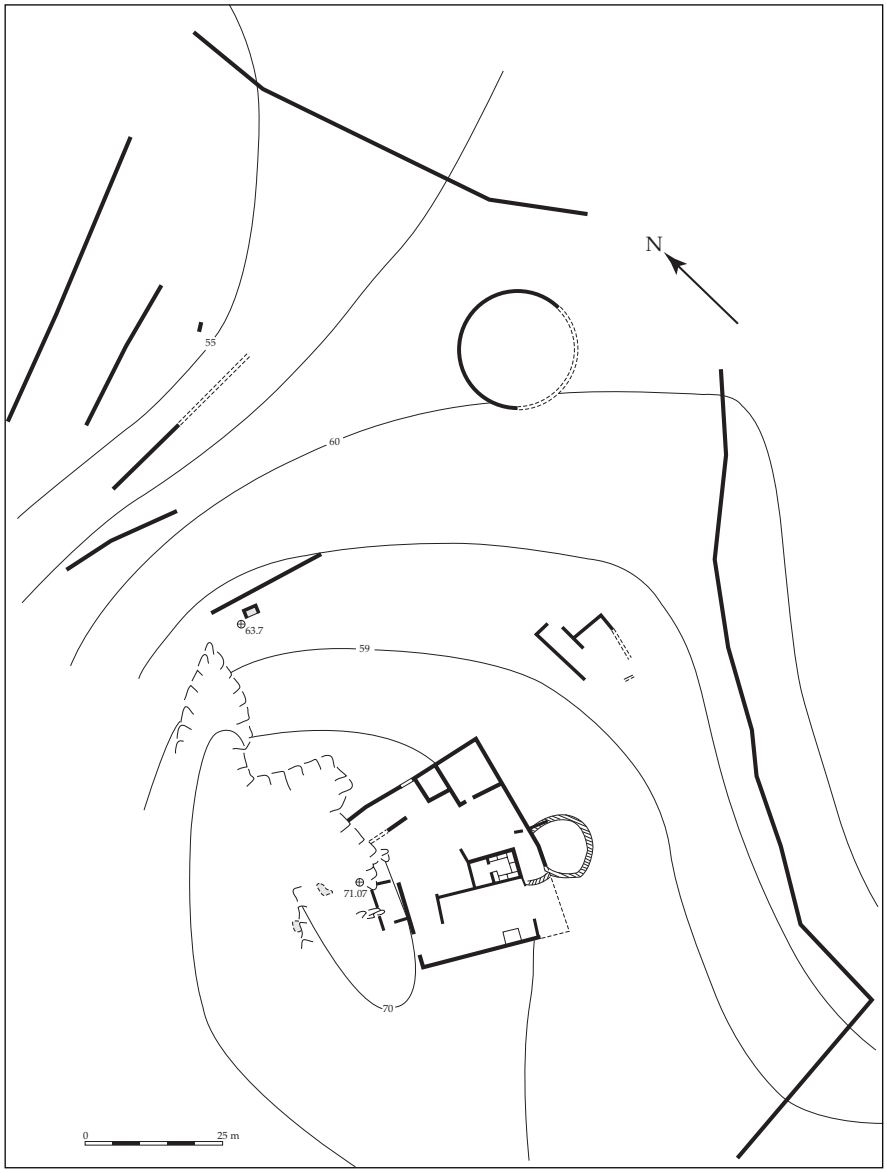


Figure 6. *Plan of the farm LE 16 of H. Lohmann's survey in southern Attica, showing the farmstead and out-buildings, with a boundary wall (above and right), terrace wall, (upper left) and a threshing floor (above, centre).*

into cemeteries. The original appearance of the best-preserved establishments could be readily reconstructed (Fig. 7).

Notice that both the prize examples of innovatory Classical archaeology that I have produced stem from field-survey projects. If there is one huge aspect that contrasts the new approaches from the old, it is this switch from the urban to the rural, undertaken in disregard of the near-silence of the literary sources and pushed through, I might add, in the face of indifference (to use no stronger term) from those of a more traditional cast of mind today. Neglect of organic materials has been another characteristic of the subject, so let me give an extreme and perhaps distasteful counter-example. We have just seen, from Italy and from Attica, that it was only by surface survey that the rural landscape was found to have been dotted with small, isolated sites which offer convincing evidence of having been inhabited: roof-tiles, tableware, cooking vessels, storage-vessels, lamps and so on. Perhaps the next big finding, however, was one that is not present in all parts of the Mediterranean landscape: that even between these small sites, much of the landscape is frequently carpeted with broken pottery and tiles, mainly of the Greek and Roman periods, much less commonly of the ages that came before and after. The closer you are to one of the Classical sites, the denser the scatter (Fig. 8). What is this material and how did it get there?

Here we are still in the realms of theory, because the answer is very hard to demonstrate. But the most convincing proposal, I think, is a more-or-less common-sense one: that Classical farmers are known to have fertilized their land, using domestic refuse and animal manure to do so. This rubbish would be collected over the year in some sort of compost-heap, then spread on the fields, roughly concentrically out from each farm. While it was being collected, broken pot and tile would be thrown into the same heap, become embedded in the refuse, and be taken out with it. The refuse would turn into soil, the potsherds and tile would not. Provided that the land remained in cultivation over the following centuries, ploughing would always bring a proportion of it to the surface. The earlier stages of this process, as ethnoarchaeological study has shown, are something which can still be witnessed in the more traditional agricultural régimes of Greece, with plastics taking the place of ceramics, and other, more perishable

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materials such as textiles also embedded in the refuse. This explanation — indeed, mere discussion of the issue — is so unattractive to some of our colleagues, perhaps so out of keeping with 'the glory that was Greece', that it has provoked protest on what seem primarily aesthetic grounds: 'Making a mountain out of a manure-hill will leave many readers with a ludicrous view of survey archaeology' (Morris 1995, 185).

But at any rate I have given my explanation, of something which needs explaining: for none of the easy answers, whereby environmental, climatic or *recent* agricultural factors might have gradually spread the material out from the site centre, will really do — for a start, the scatters run uphill as well as downhill from the sites. Mediterranean conditions, I should add, prove to be especially favourable to generating these scatters, as can be seen by comparing the density of Roman pottery emanating from Roman sites in Essex (Williamson 1984, esp. fig. 2), where 'more than 4 sherds per hectare' counts as 'dense scatter', with that of Greek pottery from Classical sites in the project I have jointly directed with Professor J.L. Bintliff in Boeotia, Central Greece (from

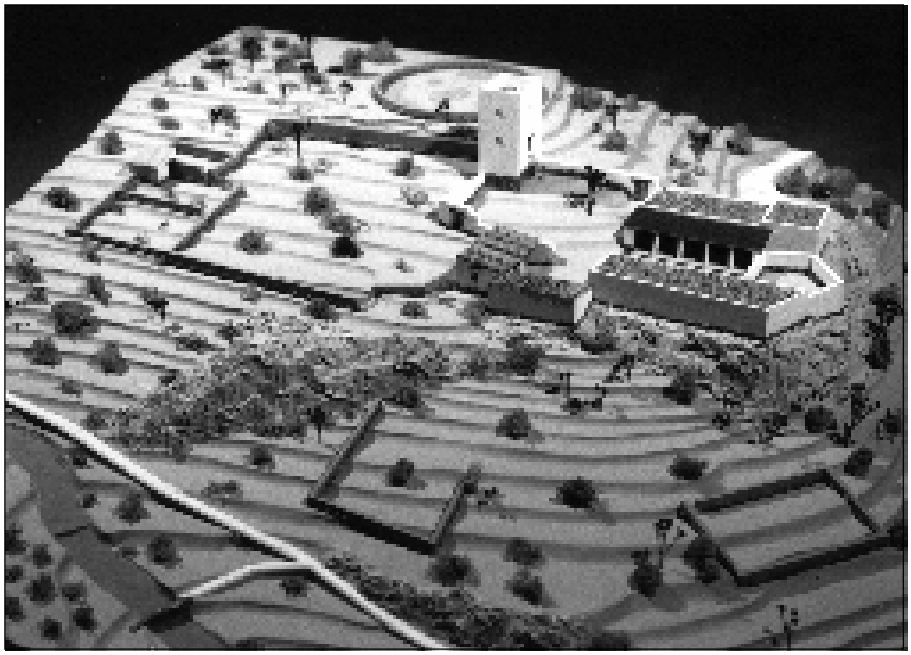


Figure 7. *Model of the farm LE 17 of Lohmann's survey, showing the farmstead and out-buildings (here including a tower), enclosures and, again, a threshing floor.*

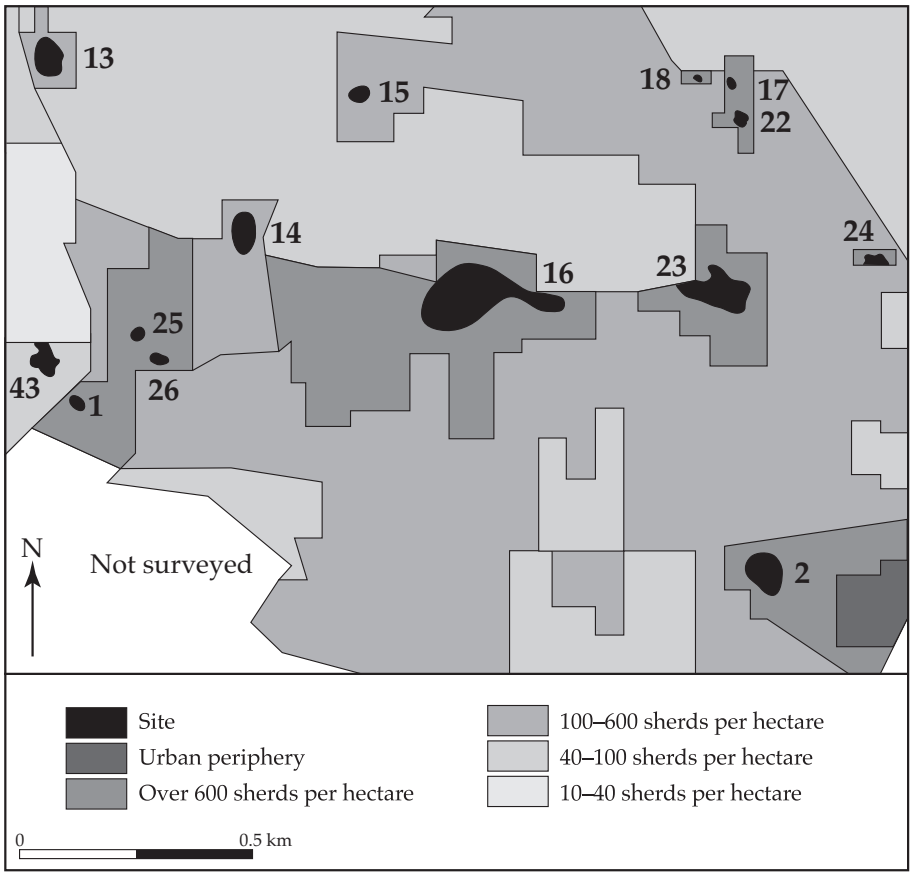


Figure 8. *A sherd-density plot from the Boeotia Survey, central Greece, in the territory of ancient Thespiai: the 'urban periphery' (lower right) shows the proximity of the city itself. The ground slopes at first from north to south, then is virtually level in the southern half of the map.*

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which comes Fig. 8), where the higher densities, outside the actual sites, exceed 600 pieces per hectare, 150 times as high. The 'quantitative bonus' of Mediterranean archaeology has struck again.

In this case, the difference is so striking that, in turn, it opens up a set of new problems. Are climatic factors alone responsible for this change in orders of magnitude as between Britain and Greece? That they played some rôle is suggested by the fact that even single-period Roman sites in Greece generate far more pottery than Roman sites in Britain. But Martin Millett has developed an alternative and powerful line of explanation: that the production, per unit of time, and the supply and consumption, per head, of ceramics showed major fluctuations in different epochs and regions (Millett 1991). As a simple statement of fact, this must be true; and it probably also carries considerable weight as one of the explanations.

I have not yet mentioned one work in the Classical field, so weighty and so uniquely influential that there must be many people, in the United States at least, who would automatically suppose that a title like mine must be intended to refer to it: *Black Athena* (Bernal 1987; 1991). The book has been taken seriously enough to be incorporated into American school curricula and would, in some quarters, unquestionably be judged to have generated a paradigm shift, in a much wider field than that of Classical archaeology, but there as well. For me, it offers a classic example of a work of genuine originality and radical implications which is nevertheless, in its nature, unable to give rise to such a shift within the professional community. This is partly, I would argue, because its aims are anyway not directed only at changing professional practice, partly because little of its methods and evidence is new. Interestingly enough, Martin Bernal himself conceded at the outset (Bernal 1987, 1) that 'the changes of view that I am proposing are not paradigmatic in the strict sense of the word'.

To take my first argument, it is entirely desirable to seek to undermine European racism in general, and in particular to show that it was an attitude that extended to much nineteenth- and twentieth-century scholarship. But in professional practice, the presence of this last factor had been widely accepted, and adjustments made for it, even before it was so brilliantly documented in volume 1 of the book. There was no 'common awareness that something had gone wrong'; there was no perception of unsolved problems. Instead, the question that has since been asked by many archaeologists is: if every word of *Black Athena* were accepted and absorbed by us, what difference would this make to our actual practices? Or, loosely re-phrasing the question in terms of the distant past: by re-writing the origins of a culture how, if at all, do you change its nature?

As for the methods employed in the work, they are familiar enough: in Jonathan Hall's words (1990, 252), 'the stage and a great deal of the scenery and props remain fundamentally the same'. Volume I offers, first and foremost, re-study of ancient literary texts, then of modern receptions of them; volume II addresses other documents including archaeological finds; while linguistic evidence, etymology and place-names are promised for III, and religion and mythology for IV. Those who set out to refute its propositions are easily drawn into using equally traditional evidence to do so. Any such fundamental re-orientations as, say, the rejection of the 'Indo-European hypothesis', are absent.

All this makes it particularly fascinating to read of a direct encounter in 1989, shortly before an academic meeting where volume I of *Black Athena* was to be discussed, between Bernal and Kuhn himself (Bernal 1991, xix). Whatever Bernal was expecting (he had prefaced his first volume with a quotation from Kuhn), the comments of the sage reported by him are rather non-committal. Two of them are still worth recording here: that the meeting was being held much too soon (about two years) after the publication, but that the important thing was the legitimating effect of holding it at all. Encouragement, but not yet validation.

It is not an entirely frivolous point (though perhaps an unkind one) to observe that more or less the same means are employed by infinitely less-serious writers to argue for far more extravagant conclusions. An example might be the author of *Where Troy Once Stood* (Wilkens 1990), who has set the Trojan War close to Cambridge, with Troy itself located at Wandlebury on the Gog Magog hills; and has extended the travels of the *Odyssey* far afield, not just to his native Netherlands but to the Azores, the Cape Verde Islands and even Cuba. The method here is, again, close analysis of Homer's text, supplemented by the evidence of etymology and place-names, with briefer glances at religion and mythology; and again, if serious researchers spared the time to refute Wilkens' thesis, they would do so partly by use of equally traditional evidence, partly in this case by common-sense observations (Homer's references to continuous rain and occasional snow are not, in themselves, a proof that the *Iliad* is set in Britain). Both examples, in my view, serve a useful purpose by exemplifying certain of the means by which a paradigm shift is *not* to be achieved.

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Even before this last digression, we had strayed into questions whose interest would be, has indeed been seen to be, incomprehensible to some practitioners of the subject. We have tried our hands at explanation, not in the past regarded as necessarily one of its objectives. We have indulged in a little mild quantification, again an activity long regarded as the province of less well-endowed branches of archaeology. We have looked at the claims on our attention of organic materials. There is, finally, the issue of the neglected periods of antiquity. I should perhaps have begun with this, the only area for which I can point to a published claim (not made by myself) that a paradigm shift has taken place. In traditional archaeology, these periods were neglected mainly because they were seen as 'uninteresting'. Crudely decoded, this means what one would by now expect: they were periods of which our ancient sources had little to say, or which showed a dearth of beautiful artefacts. Prime examples are: the Middle Bronze Age of Greece (but not the Late Bronze Age of the Mycenaean palaces); most of the Early Iron Age everywhere; much of the Hellenistic and Roman Imperial ages in Greece; later Roman Imperial Italy and, above all, the period after the capital was transferred to Constantinople.

One of these periods is the Greek Early Iron Age. In so far as it entered the traditional consciousness at all, it was there as a component of 'Homeric archaeology', already noted as a prize specimen of a sub-discipline that took not only its lead, but its very name, from the fact of survival of literary texts. But by the 1970s, writes Ian Morris (1996, 123), there had grown up 'a methodological distinction between a text-based model of the 500 years after the destruction of the (Mycenaean) palaces as a Heroic Age, and an artefact-based model of it as a Dark Age'. One might have expected archaeologists, of any persuasion, to lean towards the second of these models, but this had not always been the case. I still cherish the rhetorical question put by a reviewer of my own first book: 'Is it being old-fashioned to suggest that the special value, and special delight, of Classical Archaeology is that it helps us to interpret the Classics, and not the other way about?' (Anderson 1966, 269). Although this is a sentiment which I have never shared, I have always respected it for its candour, and for the consequent clarity with which the issue is posed.

But turn now to what, according to Ian Morris, happened next. 'At this point, we might expect to find an academic debate to rival the fury of that of the 1880s' (referring to the conflict then aroused by the palpable incompatibilities between the discoveries of Heinrich Schliemann at Late Bronze Age Troy and Mycenae, and the Homeric picture). 'Instead', Morris continues, 'there was a classic example of the paradigm shift . . . with hardly any sustained debate in print, the archaeological model quietly swept the field'. He goes on to illustrate this by the institutional evidence (something which Kuhn stressed but which I have so far omitted from this article), by pointing to the successive elections to chairs at University College London, Cambridge and Oxford; and adds that 'Outside Britain, the published versions of the three major international conferences on Greece between 1200 and 700 BC also suggest that by the 1980's the British scholars' archaeological model . . . was simply taken for granted by most linguists and philologists, as well as by archaeologists' (1996, 123). It all sounds just like what Morris calls it, a paradigm shift. But there is one jarring note: 'with hardly any sustained debate'. Why did this not happen? Was it because so few people were at all interested? Was it because the shift was so palpably overdue? I myself put it down to the maturity and fair-mindedness of Classical scholarship as a whole, but then I am a partial witness. Morris, another partial witness, thinks that, in 'This extraordinary success . . . the crucial factor was that the archaeologists simply produced fuller and more dynamic pictures than the philologists' (1996, 124).

Be that as it may, circumstances are crowding me into breaking my promise to avoid parochialism, and offering an answer to a final, unspoken question: does this have anything especially to do with Cambridge? It would be possible to answer in positive terms by pointing out that Ian Morris is himself one of the group of graduate products of Cambridge Classical archaeology in the 1980s and 1990s. It is significant that most of this group maintain links with prehistoric world archaeology: some (James Whitley, Catherine Morgan, Michael Shanks) actually had part of their original formation in the famous Cambridge school of that discipline. This is the moment to acknowledge that there is a causal link here: much of the innovation achieved in recent Classical archaeology does indeed stem from familiarity with the current advances in the wider discipline. And talking of institutional evidence, this and other fields within Classical archaeology are now energetically professed by those named, and by their former Cambridge fellow-students, in the universities of Chicago, Florida, Lehigh, Michigan, Princeton, Stanford, Birmingham, Cardiff, Glasgow, King's College London, University College London, the Open

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University, the Open University of Greece, Oxford, Reading, Sheffield and, in the person of the newly-elected Professor of Ancient History, right here in Cambridge. But it would be tasteless to continue in this vein: even now, we are within Kuhn's ceiling of 'fewer than twenty-five people'. Instead, I will appeal to a fairer source: our opponents and critics.

That there has been some form of backlash is, in view of what was said above, reassuring. It was rather slow in coming and it took a rather unexpected form, but here are a few quotations, all from the last decade, to illustrate it:

> What is a mirage is the Dark Age and the deliberate distance maintained between the second millennium and the culture of Classical Greece (Papadopoulos 1993, 195);

> Greek civilization . . . would certainly be better served if the spectre of the Dark Age, a phantom that has haunted the 'musty confines of Cambridge' . . . for too long, is finally laid to rest (Papadopoulos 1993, 196–7).

From this, it transpires that what is now criticized is no longer the preference for an archaeological interpretation of the centuries after the fall of Mycenae, but the specific and negative nature of that interpretation: the 'Dark Age'. This is a slightly different charge from that of turning one's back on the Homeric picture for that period, but there are features in common: a similar hankering can be detected, for a return to the picture of a glorious continuity, or a continuity of glory, extending from the Greece of Agamemnon to that of Perikles, which has been rudely interrupted by the insertion of a spurious Dark Age in between. A third, and perhaps less transparent, quotation addresses one of the accompaniments to this interruption:

> Early Greek archaeology [has been turned] into exclusive grist for the mill of the polis, the only currently acceptable goal for politically correct scholarship (Morris 1997, 64).

The charge here is that, to reinforce and underwrite this interruption in Greek culture, there has been a parallel concern to over-emphasize what is supposed to have happened at its end: the rise of a new, fresh Greek civilization of the polis, from the long-cold ashes of Mycenaean Greece. If we set aside questions of opinion and evaluation, these criticisms can claim to present the factual state of affairs in tolerably accurate terms, and I shall not engage in debate with them. The charge of 'political correctness', as often, is an acknowledgement that the party under attack has made some progress in the exercise of persuasion. But these comments are also relevant because they point the finger, explicitly or implicitly, at Cambridge as the villain of the piece. This is something that had not previously emerged.

So did the changes which began in the 1970s, in Cambridge and elsewhere, constitute a paradigm shift? The feeling that 'something had gone wrong' was certainly present in the minds of some, though they were only a minority. The narrowness of the field from which the changes began is, as we have seen, not an insuperable barrier. The institutional evidence at least says something about the change in the shared beliefs of the disciplinary community. Opinions have visibly changed on 'which problems it would be more significant to have solved'. Previous views of what the prehistoric, proto-historic and Classical Greek worlds (to say nothing of the Roman) 'were like' have certainly been adjusted. 'Normal science' in Classical archaeology, in dozens of universities, no longer means what it used to. Irrespective of any possible paradigm shift, by far the most important result of the changes has been, in my own opinion, to make of Classical archaeology a much more intellectually challenging discipline. But as I said before, others must judge.

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Anthony Snodgrass was, from 1976 to 2001, Laurence Professor of Classical archaeology at Cambridge. His research has been centred on the archaeology of Greece in the Early Iron Age and Archaic periods, with extensions in space and time, from prehistoric Europe to Roman Britain, and latterly with a concentration on field survey in Greece.

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