In the late 1960s and early 1970s, the Editor of ANTIQUITY, Glyn Daniel, invited a series of reactions to the cross-currents of archaeology at the time. The first was a conservative statement on archaeology as the servant of history by Jacquetta Hawkes (1968). This assumed the mantle of the 'Proper study of mankind', and urged that the humanistic study of archaeology be saved from the encroachment of science. Such was the reaction by archaeologists over the age of 40 to this article that Glyn Daniel was prompted to offer a prize (which would still buy a student today two years of ANTIQUITY). The prize was to be awarded for the best response by a young archaeologist (presumably younger than 40) to the question 'Whither archaeology?' The winning contributions were published as two essays by Evzen Neustupny (1971) and Glynn Isaac (1971). The second of these particularly addressed the challenge of Jacquetta Hawkes, and introduced Clarke as a protagonist; although the more explicit statement of the new archaeology was an article by Richard Watson - 'The “new archaeology” of the 1960s' — published in ANTIQUITY in 1972, which drew a strong distinction between anthropological archaeology and classical archaeology. An essay Clarke had prepared to appear in the Cambridge economic history of Europe worked hard to bridge this potential divide between history and archaeology, although this important theme is only touched upon in 'Archaeology: the loss of innocence'. Unlike many North American New Archaeologists who rejected history, Clarke stressed that 'work in text-aided contexts will increasingly provide vital experiments in which purely archaeological data may be controlled by documentary data, bearing in mind the inherent biases of both' (1973: 18). This is a debate that Hawkes (1973: 177) addressed in his response to Clarke's 'Archaeology: the loss of innocence'. Hawkes was another scholar convinced of the dangers, and seduction, of equating archaeology with pre-history.

In the same issue of ANTIQUITY, Clarke was invited to review Watson et al. (1971) with a prescient realization that 'there is little reason to suppose that the positivist philosophy of physics is especially appropriate for archaeology ...' (1972: 238). The views of Hogarth (1972), published in the following issue of ANTIQUITY, were a fresh presentation of the conservative reaction to the New Archaeology movement, concentrating more on the medium than the message. This paper, in many ways, brought the discussion to the completion of a first cycle and prepared the ground for Clarke. A more positive statement by Christopher Hawkes (1973), published after 'archaeology: the loss of innocence', marks the closure of a second cycle.

The most memorable contribution to these debates was undoubtedly the one published 25 years ago by Clarke himself, 'Archaeology: the loss of innocence' (1973).¹ This has come to be considered, at least in Britain, one of the seminal statements of the New Archaeology, by one of its leading proponents. It is this that we commemorate here. In spite of the fame of this statement in the Anglo-American world, we should not exaggerate its impact outside Britain. Courbin (1988) chose to concentrate on other statements of Clarke. In a more global statement

¹ The ANTIQUITY web site presents 'Archaeology: the loss of innocence' in full, together with several letters and other reactions published in ANTIQUITY in 1973:
http://intarch.ac.uk/antiquity/hp/cl-intro.html


on recent theory in Italy, the role of Clarke is much less pronounced (Guidi 1996). A recent statement of German theory (Bernbeck 1997) gives almost no attention to Clarke at all (and much more to other New Archaeologists). This, at first, appears to be a pattern of limited recognition that could be illustrated on a more global level. Perhaps some of what follows can illustrate the foresight and creativity of Clarke in perceiving the developments that continued after his death (only three years after the publication of 'the loss of innocence'), often in the hands of his pupils.

‘Archaeology: the loss of innocence’ had an impact greater than its level of citation, and the current Editors have invited five current practitioners of archaeological theory to comment on this seminal statement and, in turn, have asked one of David Clarke’s pupils to comment on their appraisal, a form of open review. We are conscious we might be building on a genealogy of self-commentary, but nevertheless consider it to be an important academic exercise. Not all those invited felt able to accept the invitation, but there are represented here a range of views from Britain (Palaeolithic and recent prehistory), the Mediterranean and the New World. One of the authors, Bruce Trigger (1970), contributed to the ANTIQUITY debate of the 1970s by exploring the relationship of archaeology to history and the social sciences. Three others are now older than 40, and thus would not have qualified for the Glyn Daniel challenge of ‘Whither archaeology?’. The editors welcome further contributions in the spirit of Glyn Daniel’s invitation: ‘a free for all, new and old’. We can now add ‘post-archaeology’ to archaeology and its other relationships, if one follows the suggestion of Chris Tilley’s last sentence!

In the Special section:

Alessandro Guidi discusses the reaction to Clarke from an Italian and southern European perspective.

Michael Parker Pearson describes his understanding of Clarke as an important figure in promoting social theory, and how this has changed since Clarke’s original writings.

Anthony Sinclair approaches Clarke from the perspective of a Palaeolithic archaeologist, noting the invigorating optimism of his general theory of archaeology.

Christopher Tilley, one of Clarke’s last students at Peterhouse, reflects on the impact of his work, and also on his ‘innocence’ and naivety of archaeological social theory.

Bruce Trigger assesses the historical role of Clarke, within the broader development of archaeology, both before 1973, and since the publication of ‘the loss of innocence’.

Andrew Sherratt comments on the papers and provides his own assessment of Clarke’s contribution.

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References


Clarke in Mediterranean archaeology

ALESSANDRO GUIDI*

When ANTIQUITY published the historical article by Clarke, I was a 20-year-old student, deeply engaged in field activities and substantially torn away from the ‘theoretical’ debate.

My archaeological loss of innocence happened only in the early 1980s, when I discovered (thanks to people like Maurizio Tosi and Anna Maria Bietti Sestieri) the enormous explanatory potential of processual theories. It would be absurd to label the whole of Italian archaeology as ‘atheoretical’; as a matter of fact, a powerful theoretical machine, the Marxist theory, had operated from the late 1960s, thanks to the group of Dialoghi di Archeologia. The problem was in the idealistic roots of our (academic) culture, characterized by a programmatic divorce between humanistic and scientific studies and from a substantial lack of interest for the anthropological theories.

For a young Marxist-committed scholar of prehistory, as I was, New Archaeology brought fresh air in this environment, allowing the opening of previously unthinkable perspectives in the explanation of an impressively growing corpus of archaeological data.

The 1980s saw the formation of an ‘Italian’ group of scholars that in many congresses and seminars in the Anglo-Saxon countries or in Italy could profitably exchange their points of view with the protagonists of processual (and, in some cases, post-processual) archaeology (Cuomo di Caprio 1986; Guidi 1987; 1996a). This ‘burst’ of theoretical interest ended in the late ‘80s (but not our participation in conferences, as demonstrated by the last Bourne-mouth TAG, with two ‘Italian’ sessions!). In the meantime, the debate on processual theories spread also in Classical and Medieval environments, with the important consequence of a generalized loss of innocence of our archaeology; in the same years, a parallel debate developed in Spain, building the premises for an ever-improving network of exchanges, projects and experiences in Mediterranean archaeology.

Naïve as they might seem, our efforts of the ’80s were an attempt to break the overwhelming climate of relativism and intellectual compliance that dominated Italian archaeology. As a matter of fact, the use of processual categories and of the anthropological theory and a complete integration of these ‘keys’ of interpretation of the archaeological data with our tradition of studies are the best ways to grapple with problems like the study of early state formation in Iron Age Italy, a classical ‘taboo’ of our archaeology, only in recent years finally acquiring the dignity of an historical question (see, also for the bibliography, Carandini 1997).

Coming back to Clarke’s article, it must be recognized that, at the time of its publication, there was no reaction on the Italian scene (probably also because very few persons knew it!). Apart from an article review of Analytical archaeology by the Polish archaeologist Tabaczynsky — well-acquainted with Italian archaeology and introduced, on that occasion, by the Italian scholar Gabriella Maetzke — published in 1976 in Archeologia Medievale (Tabaczynsky 1976), the ‘loss of innocence’ is quoted (not by chance, in the brief season of Italian ‘processual’ archaeology) in some ‘theoretical’ articles of the late 1970s and ’80s (e.g. Barich 1977–82; 1982; Maetzke 1981; Cuomo di Caprio 1986; De Guio 1988–89; 1989), often as a symbol of the need for a renewal of archaeology in our own country.

It is more interesting to investigate the general impact of Clarke’s works in Italy and in Spain. Examining the bibliography we discover that Beaker pottery of Great Britain and Ireland was readily reviewed in Italy (Cazzella 1971). The same scholar, Alberto Cazzella, critically quotes Analytical archaeology in an article of the early 1970s (Cazzella 1972); this notwithstanding, the book was never reviewed

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by any Italian archaeologist, and a translation to appear in a new ‘theoretical’ series was announced only this year, while in Spain the book was already translated in the '80s (Clarke 1984). It should be remembered, as a demonstration of the great popularity of this book, that already in 1969 a collective work of Turkish scholars was published, with the significant title Analitik arkeologi (Dincol & Kantman 1969).

The field in which Clarke’s work was more influential in both countries is settlement archaeology. In Italy, the congress about ‘Economy and territorial organization in the prehistoric societies’, held in Rome in 1982, in which spatial analysis techniques (Thiessen polygons, site catchment analysis, etc.) were employed for the explanation of Italian archaeological data. Economia e organizzazione (1982) was also the first occasion for a ‘public’ presentation of Spatial Archaeology (Cardarelli 1982); in the following years these themes were recurrently held, in congresses, books and articles, by many scholars (for the bibliography see Guidi 1996b).

In Spain things went still further; as a matter of fact, Arqueologia Espacial was the name chosen by Francisco Burillo for the National congress on ‘Distribution and Mutual Relationships of Settlements’ held in Teruel in 1984 that was the first true occasion of meeting for many young researchers engaged in Prehistoric, Classical and Medieval Archaeology (Arqueologia Espacial 1, 1984–85). Moreover, the next meeting, held in the same place in 1986, was significantly named ‘Colloquium on the Micropace’ (Arqueologia Espacial 2, 1986–87).

The recognition of Clarke’s influence on Italian and Spanish archaeology indicates clearly that in these countries, as well as in Eastern Europe, he was, for many years, the most popular among the New Archaeologists.

Yet in the early ’70s, Binford harshly criticized Clarke, who in Analytical archaeology ‘had adopted the statistical procedures of Sokal and Sneath . . . , the sophisticated locational approaches of Peter Haggett . . . , and the metaleanguage of system theory, but all of this . . . integrated into a traditionalist paradigm of culture’ (Binford 1972: 330; italics are mine). This is, effectively, the best interpretation of the critical (if also often negative) attention paid to Clarke’s work in a traditionalist academic environment; his major book, as a matter of fact, is a complex synthesis between the new theories and the ‘mainstream’ cultural approach of European prehistory.

For the same reasons, a ‘continental’ tradition of settlement archaeology, whose roots go back to the surveys and the extensive excavations of the period between the two World Wars, could not remain indifferent to Clarke’s speculation on the ‘levels of resolution’ of spatial archaeology.

This never-ending critical ‘dialogue’ between Clarke and many of us (also on the Classical and Medieval side) is, overall, a good demonstration of the growing need for an integration between the anthropological and the historical explanations, after the ’Great Divide’ of the last decades.

What is the legacy of Clarke’s ‘loss of innocence’?

It is difficult for the reader of the 1990s to avoid the impression that it fully reflects the climate of optimism and trust in science that dominated the ’60s and the early ’70s; this notwithstanding, many observations seem impressively up-to-date.

One example, among others, is the prophetic (and often unheeded) call for the building of predepositional, depositional and postdepositional theories; the present climate of discard of processual items is effectively justified from the ascertainment of the distance between the refined theories and a still absolutely poorly unknown archaeological record. Again, in this field some Spanish and Italian scholars worked very hard, reaching a good and refined theoretical level. Not by chance, the fourth Teruel meeting was dedicated to the postdepositional transformations (Arqueologia Espacial 4, 1993–96); on the other side, Giovanni Leonardi, Claudio Balista and Armando De Guio created a ‘school’ devoted to the improvement of the techniques of excavation and survey, well aware of these complex problems (see e.g. Leonardi 1992; De Guio 1992).

It is impossible to predict the future, but it is highly probable that the problems connected with formation processes and the compositional characteristics of archaeological deposits will be a major item of debate in the years to come. This will be, probably, the real ‘interpretive’ archaeology, a powerful tool to reshape our theories.

Using Clarke’s metaphor, it is right to acknowledge how the loss of innocence can be only a first step in individual development: the goal of 3rd-millennium archaeology will be the acquisition of full maturity!
The beginning of wisdom

M. PARKER PEARSON*

It is the best of times and it is the worst of times. On one hand, there are more resources and people involved in archaeology than ever before; there is considerable public and media interest in the subject; and there have been exciting developments in archaeologists’ uses of social theory. On the other, competition is intense for locally scarce funding; most field research is constrained by non-archaeological considerations; and fragmentation, insecurity and disenchancement are rife. The split between theory and practice has certainly widened since David Clarke’s day, whilst theory has become not so much Clarke’s unifier within the morass of empirical detail but its own basis for division and often bitter disagreement within the profession.1

1 One manifestation of this is the regularity and vitriol with which British books on theory are pasted in North American reviews (and vice versa), verging on farce and worthy of a study in its own right.

In 1973 Clarke set out the notion that archaeology had attained a state of critical self-consciousness, passing through consciousness (the naming and definition of the subject) and self-consciousness (the largely technical revolution in procedures, classifications, principles and rules). Critical self-consciousness is characterized by a metaphysical, philosophical and theoretical revolution, concerned with explanation and interpretation, which ushers in an accepted body of general theory whilst, at the same time, accelerating changes bring uncertainty, insecurity and general unrest. Some of my colleagues who knew David Clarke consider that he would never have gone down the relativistic road of some ‘post-processual’ archaeological theory, had he survived. Yet some of his statements in 1973 would seem to foresee and predict its arrival; that observations and explanations are all metaphysics-dependent, that ‘origins’ studies

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might emerge as semantic snares and metaphysical mirages, that ‘colonial’ concepts would be severely challenged, that differences in metaphysical ‘schools’ could not be judged as right or wrong, and that meanings of time and space are relative to observed phenomena. There has been much theoretical water under the bridge since then: structuralist and symbolic archaeology, post-structuralism, phenomenology, post-positivist philosophies of science, feminist and gender theory, postmodernism and calls for archaeology’s loss of political innocence. This explosion of interest in social theory, just a small and restricted part of Clarke’s idea of general theory, appears not to have been predicted by him. However, in many ways, we can identify more closely with 1970s archaeology than that era could with the thinking of the 1950s. In retrospect, that earlier epistemological break which saw the emergence of theoretically aware ‘New’ disciplines appears to have been more profound, not just for archaeology but for the social sciences generally.

Clarke’s dead wood

Despite Clarke’s extraordinary intellectual vision, we may identify several areas where the approach that he endorsed can be considered to have failed. There is no unifying general theory but a battleground of contested approaches. Indeed, archaeology has no theory of its own, a point made some years ago (Flannery 1982), borrowing from and tapping into a broad field of social theory. Although archaeology’s disciplinary boundaries have encroached into subjects such as anthropology, history and material-culture studies, the theoretical writings of archaeologists are ignored or lambasted by theoreticians in other social sciences. David Clarke’s own attempts to bring theoretical insights to archaeological analysis were glorious failures. His social models for the Glastonbury Iron Age lake village (Clarke 1972; Coles & Minnitt 1995) and for Beaker sequences and interaction (Clarke 1970; 1976; Kinnes et al. 1991; Boast 1995; Case 1995) may have been influential at the time, but they can be considered as profoundly erroneous today. Clarke’s cavalier and misleading treatment of the Glastonbury data, based on very poor factual foundations, is revealed by Coles & Minnitt’s careful study (1995: 180–90) and seems to have prefigured the Zeitgeist of theoretical archaeology in the 1980s and 1990s, with the primacy of theory over detailed knowledge of, and skilful attention to, archaeological material. It is not just in archaeology that we may hear university teachers bemoaning their students’ immersion in current theoretical fashions at the expense of knowledge of basic source materials.

One of Clarke’s most marked contributions to British archaeology was his deployment of technical language. I remember finding Analytical archaeology on the shelves of the public library, whilst at school, and being baffled and impressed by its extraordinary prose style. Here was a flagship for a discipline to be taken seriously, a discipline that was now far more than a cross between gardening and antiquarianism. For students of archaeology, this particular dialect of New Archaeology is currently just one language of theory that they must attempt to understand. With students constantly asking why theory must be written so opaque and complexly, our colleagues continue to develop written styles that display their cleverness with big words and obfuscatory phrases. Despite calls for plain speaking, some consider that archaeology requires these two levels of discourse, one for the specialists and one for the broader public. It has been said that the most opaque writings are often produced by the clearest speakers, a feature of Clarke’s own inspirational teaching. We can forgive the use of jargon in the 1960s as helping to legitimate the study of archaeology as a serious and ‘heavy’ profession, but how much is now mere linguistic posturing, as each generation of young researchers finds a vehicle to impress their peers and advance their careers? After the heady days of theory for theory’s sake in the humanities, there seems to be a renewed interest in writing well and in communicating complex ideas through ordinary language. Despite claims that ideas are tied to words and that the public must be ‘improved’ in their appreciation of difficult prose (Tilley 1989), there is a strong groundswell of opinion that the jargon of theory is an exclusive and elitist preoccupation, both amongst practising archaeologists and amongst others such as the ‘fringe’ who have long considered it just so much ‘intellectual wrist action’ (Sullivan 1997).

This is not to say that theory has outlived its usefulness. Theory is always required in our search for explanation and understanding, oth-
erwise we adopt the implicit assumptions and preconceptions of our own upbringings without any critical reflection. What we may see ditched as dead wood is the writing of impenetrable theoretical jargon as well as the pursuit of theory for theory’s sake. There are at least two roads that we may take from here. One is the better integration of theory and practice, both in interpreting the past and in addressing archaeology’s role as a cultural practice in the present. Out of this will come genuine advances in our understanding of the past and the present. The second road is the rejection of aspects of western social theory, on account of its imperializing discourse and context, by post-colonial archaeologists who regard the application of ideas produced by 20th-century German, French and English philosophers and theoreticians as being ethnocentric, arbitrary and alien (Andah 1995: 105).

Clarke’s view of archaeological theory was that it could be divided into five: predepositional and depositions theory, postdepositional theory (both of these can be equated with Binford’s middle range theory), retrieval theory (broadly sampling theory), analytical theory and interpretive theory. He describes both the latter and predepositional theory as composing social theory. As such, social theory ‘... is but a small part of archaeological theory and even in this restricted but important area the primitive terms... of social theory... will require... transformation’ (1973: 17). In retrospect, he seems completely to have underestimated the way that social theory, within all the humanities, would come to dominate our analytical concerns rather than remaining as a small element within his notion of general theory. Clarke also seems to have thought that his three-part theory of concepts (metaphysics), theory of information (epistemology) and theory of reasoning (philosophy of science) would develop separately and equally, so as to produce a general theory for archaeology. Yet the impact of social theory, especially in its postmodernist form, has been to deny universal understandings, to question the epistemological basis of knowledge and to attack the conception of truth as envisaged in the empiricism/positivism that Clarke espoused.

These developments in social theory have led to the recognition that supposedly ‘objective’ knowledge incorporates unacknowledged subjectivities, that facts are theory-laden rather than theory-independent, that truth and laws of culture are not universal but are contingent and temporary and that consequently academics have to re-evaluate the basis for their authority to make statements about the world. In such circumstances the loss of nerve among western post-processual archaeologists, in being unable to say what happened in the past with any credibility, presents an unacceptable position for many archaeologists. If we can never discover the truth, do archaeologists have the right to continue doing archaeology?

**Truth and relativism**

There is considerable misunderstanding by broadly processualist archaeologists of post-processual or ‘interpretative’ approaches to knowledge, which are often deemed to be promoting a disabling relativism. The question of truth has been a fundamental issue which divides the positivists of the New Archaeology from those whom they deride as various forms of relativist (Trigger 1989). Renfrew’s famous accusation that Shanks & Tilley’s approach (1987a; 1987b) advocates that ‘anything goes’ was strongly denied but how are we to evaluate post-processual creations of ‘Past-U-Like’, images of the past constructed not on any particular evidence but so as to critique politically unsound practices in the present?

Critics would see interpretive archaeology as no more than a relativist language game, with no grounds for evaluation other than rhetoric and style: ‘telling it how you like it’. The postmodern wing of post-processual archaeology might be characterized as a rejection of all metanarratives and their claims to truth since they impose continuity and closure on the gaps and silences of past reality (Lyotard 1979). As a result, there is no legitimate foundation for believing in a unified, total history, either as actuality or as subjective enterprise; unity, totality and coherence can only be entertained ironically or strategically as heuristic principles. Rorty considers that we no longer need transcendental grounds for our beliefs since social consensus, persuasion and pragmatic criticism are all that we are ever going to get (1982; 1991: 12–13; Jenkins 1995: 104, 125). According to Zagorin (1990: 266), postmodernism embodies ‘... a new depthlessness and superficiality; a culture fixed upon the image; abandonment of truth as useless meta-
A depoliticized deconstructionism that reduces all concepts to metaphors and all history to a play of ungrounded representations... depriving critical thought of competence to judge between good and bad arguments, and between reason and rhetoric.

Yet these approaches have profoundly affected our stance of supposed objectivity by opening up the complexity of our subjective engagements between present and past. They have changed forever the ways that we do things:
1. the way that we view our situatedness in writing the past (Hodder & Shanks 1995);
2. the realization that our accounts can never replicate or fully recover the past but may only ever 'stand for' the past (Ricoeur 1989; Moore 1995);
3. the realization that we must challenge master narratives ('... the more fundamental the metaphysical controlling model, the less we are normally inclined to rethink it' (Clarke 1973: 14));
4. archaeology is both a representation of the past and a representation of this representation's construction;
5. archaeology brings historical self-recognition through historicizations that construct multiple pasts, fashioning differential identity;
6. potentially it permits oppressed groups to interpret the past in terms to aid their empowerment and liberation.

But how do we avoid the problem of total relativism? Since archaeological interpretation can only ever 'stand for' the past then our discoveries are enmeshed with our constructions. For Rorty, truth is the name of whatever proves to be good in the way of belief and good, too, for definite assignable reasons (pragmatic, local and particular, and intersubjective). Thus history/archaeology is truth-creating, not truth-seeking. In conventional terms, there is no truth. So how do we deal with this?

1. Not everything goes. Not all accounts can be said to 'stand for' the past. There are 'wrong' histories which are recognizably at odds with the presented material.
2. Unlike faith and dogma, genuine knowledge is always provisional, subject to revision in the light of new information and evidence, needing periodically to be restructured, fallible; open, therefore, to pluralist interpretation, yet at the same time knowledge as far as we have managed to get. All claims to knowledge are democratic by their nature, because they have to satisfy rules of consistency, external reference, evidence that is, in principle, accessible to all (Geras 1990: 162).
3. There are different orders of truth-claim, initially confused by Foucault (power/knowledge/truth) as well as by the postmodernists (Norris 1993: 302–3):
   a) inward certainty, privileged access, and authentic revelation — the self-validating pseudo-truths of master narratives;
   b) truths subject to more rigorous standards of critical accountability through openly accountable reasons, arguments, principles and values. (The truths that dawn on us as insight and understanding in our daily archaeological practice which we search after and debate.)
4. We can decide between accounts in terms of which are more wrong (Moreland in press a; in press b):
   a) Evidence is tangible, concrete and real. It constrains but does not determine what we are able to say, such that we can recognize when theories become implausible (Hodder 1986: 96).
   b) The selection of evidence forces us to confront our theoretical perspectives that present people as constructing themselves through material culture, text and language. Thus narratives are similarly constructed but are not necessarily false.
   c) The construction of 'history' is a community enterprise. It differs from propaganda or fiction because of that community's tacit rules. Haskell (1990: 132) lists these as requirements to:
      i. abandon wishful thinking
      ii. assimilate bad news
      iii. discard pleasing interpretations that cannot pass elementary tests of evidence and logic
suspend or bracket our own perceptions long enough to enter sympathetically into the perspectives of rival thinkers.

5 Curiously, even certain postmodernists broadly agree (Derrida, cited in Norris 1993: 300):

The value of truth is never contested in my writings . . . it should be possible to invoke rules of competence, criteria of discussion and of consensus, good faith, lucidity, rigour, criticism and pedagogy.


This does not mean that there can be a true reading, but that no reading is conceivable in which the question of its truth or falsehood is not primarily involved.

All of these concerns with the nature of truth, narrative and subjectivity have been largely contained within the humanities like guilty secrets not to be shared with the public at large. Some consider that any admission that we do not know the truth will give the upper hand to the creationists, UFOlogists, ley-liners and other groups who believe unshakably that they have privileged access to the truth. Yet archaeologists could benefit from a certain humility which sacrifices not our expertise and knowledge but our pomposity and self-importance as high priests of the past, communicating truths from on high to the wider public.

Doing archaeology for the public

Clarke was not particularly concerned with archaeology's wider audience, though he concludes with a discussion of whether theory would be welcomed by amateurs, historical archaeologists and practical excavators (1973: 18). Whereas earlier archaeologists such as Gordon Childe had been prepared to write for the reading public in an accessible style that explained how archaeological knowledge was created, Clarke seems to have been unconcerned with archaeology's public accountability and reflexive relationship to its public that many today consider essential. Clarke's writings created a sense of exclusivity within the profession. In the years since then, the mastery of theory has become a requirement for many university archaeologists whereas other archaeologists have been more involved in communicating discoveries and excavation findings to the broader public.

Intellectual theorizing is not the kind of archaeology that I enjoy writing about because, for me, it is another example of addressing the profession from the lofty heights of 'theory', doling out opinions and laying down abstract recommendations and criticisms. It is symptomatic of a divided profession in which academic kudos is given to the highly specialized writers of theory whilst the substantial efforts of most archaeologists go largely unrewarded. Those who have struggled with the writing and compilation of an excavation monograph will know how such works are the hardest to produce, far more so than any book on theory, and yet (post)modern reputations can be built without any sustained engagement with archaeological material that we have personally had a hand in acquiring. We might say that we have been living since Clarke's time in the era of the professional archaeological critic. We need reminding that Flannery's golden Marshalltown was to be awarded to the archaeologist who is in the field because they love it and not because they want to be famous, who doesn't fatten up on someone else's data or cut down colleagues to get ahead, and who knows the literature and respects the generations who went before (Flannery 1982: 278).

Shanks & Tilley's important recognition of archaeology's lost political innocence (1987b: 213; 1989) has not everywhere been treated kindly; some claim that this is 'dangerous nonsense' and that Shanks & Tilley should leave their comfortable academic positions to work in places where they might actually make a difference through their practical involvement (Bettinger 1991: 147–8). Although many have been made radical by Shanks & Tilley's prose, those involved in many of the significant engagements with political action during the 1980s do not make reference to Shanks & Tilley's post-processualism. The conflicts over the reburial of human remains and the restitution of indigenous property radicalized certain archaeologists as a consequence of their personal involvements with indigenous groups and because of their colleagues unacceptable attitudes and actions rather than through their appreciation of 'theory' (Webb 1987; Zimmerman 1989). The same is also probably true for the many archaeologists involved in indigenous
land rights claims, in forensic excavations of recent war atrocities and in criticizing pernicious political regimes. Although Marxism may have few of its credentials still intact, we should still heed the quotation on Marx's tombstone: 'The philosophers have only interpreted the world in various ways — the point, however, is to change it'. Some years ago in a public lecture in London, the Russian archaeologist, V.I. Masson, explained that archaeology was psychology — we should be changing the way that people think.

The first step has been taken, to attract wide public attention for archaeology locally, nationally, and internationally. In Britain archaeology is reported on at least weekly in the media. The next step is to show that archaeology is not simply diverting and engaging, the most fun you can have with your trousers on, but involves a greater quest for self-knowledge through knowledge of the past, an opportunity to restate 21st-century values, and to reorientate our attitudes to the world. Archaeology provides long-term perspectives for a short-termist society. It should make us appreciate 'roots' without leading to racial intolerance. It should encourage curiosity in the face of indifference or fixed belief. It challenges the universal truths and master narratives of world religions and other forms of chauvinism.

2 In support of Shanks & Tilley, one can mention the efforts of their colleagues in, for example, campaigning for rights of access to Stonehenge (Bender 1998).

At the local level we should be looking at the politics of archaeology within the local community. Currently our attentions seem to be focused on the micro-politics and power structures of the excavation trench, as in Hodder's stimulating proposal for a post-processual approach to fieldwork (1997), although it can be criticized for its flaws (Hassan 1997; but see Chadwick 1998 for a measured critique). Whilst there is value in attempting to change the world by beginning with ourselves and our practices, outside observers may see such moves as rather self-indulgent and pretentious. A self-critical field practice should perhaps start with the fact that archaeologists belong to larger communities. In the field we impose ourselves on local communities in many ways and are generally appreciated because we respect the rights, views and sensibilities of the living even if we do not agree with them. We need to concentrate on power and ideology, not so much within the field team but in the greater world, on what archaeology can do for society.

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References


DAVID CLARKE'S 'ARCHAEOLOGY: THE LOSS OF INNOCENCE' (1973) 25 YEARS AFTER 685
Footnotes to Plato? Palaeolithic archaeology and innocence lost

ANTHONY SINCLAIR*

Trawling through old, dust-covered folders I found out that I first read 'Archaeology: the loss of innocence' as a 2nd-year undergraduate for an essay on whether the New Archaeology was as theoretically sophisticated as it claimed to be. My notes of the time emphasize the beginning and end of the article; suggesting that Clarke's purpose was just to argue that

1 there had been a sea-change in the nature of archaeology leading to the development of a critically self-conscious entity in the New Archaeology; and

2 to discuss what a general theory of archaeology might look like.

I was not alone in reading 'the loss of innocence' in this way, and indeed, most of the references that I have found to it, note just these parts.

In recent readings, however, it has struck me that, like a good sandwich, the bits of choice (and the parts that ought to be remembered) lie in the middle — in the interplay that Clarke sees between methodologies, observations, concepts and information. Clarke points out in a series of small sections how theory-laden our interpretations of the past necessarily are, and how important it was to be aware of all aspects of our theoretical approaches. Each is inextricably tied to the other, and the product has consequences for all aspects of our interpretations of the past. Clarke's idea of a general theory of archaeology, and his characterization of the New Archaeology, were the product of his reading of this interplay.

Twenty-five years on, much has changed. There is a new environment and there are new consequences as a result of current and recent work. Some of Clarke's concerns are now out of date: the existence of a New Archaeology is an accepted historical event, the idea of a single uniform structure of reasoning or explanation justly died soon after Clarke's comments were published. The central core, however, remains as provocative today as it was when first

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published. The archaeology of the Palaeolithic world is without doubt more explicitly theoretical than it was. A number of scholars have, like Clarke, dared to think out loud about what an archaeology of the Palaeolithic might be; the thoughts of Lewis Binford (1983; 1991) and especially Glynn Isaac (1989) stand out as the classic and influential examples. But the prospect of a general theory of Palaeolithic archaeology still seems, tantalisingly, both within and yet just beyond reach. Part of the reason for this is the tremendous diversity of relationships between the elements noted by Clarke, none of which can be easily held in control. Part of the reason also lies in the problems that Palaeolithic archaeologists have encountered in defining hominin behaviour in the face of perceived dramatic changes through time, within a field of study that likes to see itself as a social science with its roots in the biological and evolutionary sciences.

As an exploration of this, and in the sincerest expression of flattery, I thought that I might follow Clarke’s original structure for the ‘loss of innocence’, although sticking more with the sandwich filling. In the spirit of the original, references are more implicit than explicit. My apologies to all those who feel that their work is thus not given due credit.

A new environment

New methodologies

Traditional methodologies of ‘Palaeolithic’ analysis have been expanded. Lithic analysis now commonly includes not just typological and technological observations but the study of raw materials and use-wear traces, along with the refitting of excavated debitage. Despite ups and downs, microwear analysis steadily provides useful information concerning the uses of stone tools; raw material studies provide patterns of calculated human movements in a landscape, and refitting studies are linking together intra-site actions, in a way that statistical analyses could never hope to. Scanning electron microscopes now make it possible to infer the use of lithic tools from other objects such as bone in the form of cut marks, often helping to interrelate the co-presence of faunal and lithic materials in a reliable fashion. Faunal analysis now includes not just the identification of species, presumed hunted, but the interpretation of patterns of hunting, butchery, scavenging, marrow extraction and many other practices that leave traces in the condition of faunal residues or bone assemblage composition.

Refinements in isotopic dating techniques (such as the use of accelerators in carbon-14 and uranium series dating) and palaeomagnetic dating techniques, as well as the use of new dating techniques such as electron spin resonance, thermoluminescence and recently optical luminescence to name but a few, have both improved our dating accuracy and made it possible to provide dates for periods where previously none might have been. When these new dates are coupled with refinements made in the understanding of the timing and character of the Pleistocene glacial/inter-glacial sequence, especially through reference to the recent ice-core records from the Arctic, an increasingly sophisticated palaeoenvironmental framework must be matched with a greater number of dated deposits.

New observations

New methodologies and fieldwork have combined to alter fundamentally the interpretation of our earliest ancestors. For the distant past, new fieldwork finds and DNA studies have reshaped the family tree: Ramapithecus is pruned away, and with the new discoveries of Ardipithecus ramidus, and Australopithecus Amanensis, A. afarensis, A. aegyptopithecus, Homo ergaster and H. rudolfensis a series of new hominid species have been found branches from which to hang. For the ‘recent’ past, Mitochondrial DNA studies have located the origin and diaspora of anatomically modern humans (AMH) in Africa, eliminating the idea of regional origins for our species, still further reinforced by recent studies of Neanderthal Mt. DNA, which show great differences in Mt. DNA between AMH and Neanderthals in Europe. Just as significantly, recent dating of established species has dispelled the old notion of simple succession in hominin species, to be replaced by one in which there can be multiple species and possibly, indeed probably, contact between them. Between 2.5 and 1.5 million years BP there is now a plethora of hominid species in East and South Africa, and it is hard to say with any real certainty whether we (Homo) were the toolmakers, as opposed to another equally dexterous species. The same abundance is visible elsewhere. In Europe approximately 400,000 years BP there are likely to be multiple, if yet undifferentiated, species of archaic homo sapi-
ens, as present for example at the site of Atapuerca. In Western Europe between at least 40,000 and 30,000 BP, and in other parts of Europe possibly much earlier, there is clear evidence for the contemporaneity of Neanderthals and AMH; whilst the early dates of c. 1.8 million years BP for *Homo* at Longuppo in China and in Indonesia, the early dating of hominids, presumably AMH, in Australia at least 80,000 years ago, and the very late dates of approximately 30,000 years BP for *Homo erectus* in Java, suggests a complexity of hominid relationships in this region that we can hardly begin to comprehend.

The dating of parietal art itself in caves such as Grotte Chauvet and the chemical analysis of pigments have overturned simple but widely held notions of artistic evolution, revealing that the first artists possessed sophisticated skills of observation, shadowing, the use of colour and the mixing of pigments from early on, even if recent observations on the use of hallucinogenic substances suggest that some of these artists might not have been in their right minds when they did so! Whilst the possible existence of exterior parietal art such as in the Côa Valley in Portugal indicates that the Upper Palaeolithic world might have been more decorative than we think.

Programmes of extensive regional fieldwork, both archaeological and ethnoarchaeological, have opened up new perspectives on our ancestors beyond the keyholes of vertical sections or single, open trenches to reveal patterns of landscape and raw material use coupled to artefactual discard.

In many places we now see the hominid world not as points of vertical, chronological depth, placed on modern maps, but as variably sized landscapes of hominid activity with all the problems of determining geographic (and social) scales and comparative chronology that this brings.

**New consequences**

*Theory of Concepts*

Ecological (‘economic’) and evolutionary concepts still form the backbone of our interpretations of the archaeological record, much as they did 25 years ago in the work of Grahame Clark and the developing ‘school’ of Eric Higgs, although the form that these concepts take has changed. Theoretical developments in evolutionary biology now place greater emphasis upon individual decisions and strategies made within a specific context and this framework has resulted in a concern with foraging strategies, individual decision-making and the multi-faceted relationships between technology and subsistence decisions, encompassing the factors of time and raw material availability, to name but two (Mithen 1991; Torrence 1989).

There is, however, an even ‘higher’ level of concepts that we use, that perhaps more fundamentally affect the way that we construct our subject. On the one hand we are interpreting the behavioural evidence of other, possibly very different, hominid species, whilst on the other hand we also have to interpret the evidence of our own species with all the self-knowledge that allows us. Both happen in the context of social groups of which all but a very few have direct experience, and historical anthropologists of gatherer–hunters would argue that none of us do. But where to draw this boundary of conceptual difference?

When ‘the loss of innocence’ was written, that boundary was more often placed at the first appearance of the genus *Homo* — the toolmaker; it is now more often placed behind our own species, *Homo sapiens sapiens* — the symbolic communicator. Whilst the eldest of the hominids were usually conceived of as primates, our concepts, rooted in the ethnographic record, portrayed the earliest societies of *Homo habilis* as simple gatherer–hunters, loosely modelled on the San, singing, dancing and sharing their lives at home bases or central places. Later hominids were complex, high-latitude hunter–gatherers based on societies such as the Inuit. Moving the boundary of humanity later in time leaves earlier hominids, bereft of extra-somatic, symbolic resources, caught up in an endless struggle of localized, social negotiation, whilst later social groups, material symbols of secure identity visible to all, stride purposefully through broad landscapes of potential acquaintances.

Both strategies can lead to an ‘us’ and ‘them’ perspective. Yet the variation in the archaeological and anatomical record urges us to take more than a binary approach: the former hominid species sharing aspects of our conceptual understanding of contemporary primate societies, the latter being inescapably human. The challenge that we face is to find a new source of analogies or a series of concepts with which to approach behaviour that is both ‘not-primate’ and ‘not-modern-human’. These concepts must, amongst other things, deal with the structure and organization of social groups, the use and control of landscape resources and, perhaps,
some form of symbolic or transcendant communication that is not modern human in nature. Whatever the case each species has been subject to the contingencies of natural selection and all our concepts will be essentially time-dependent and evolutionary in tenor.

Theory of Information
In simple terms, Palaeolithic, low-density, archaeology lent itself to evolutionary and ecological approaches that stressed the role of behaviour and behavioural change at a generational or species level, whilst the materially rich archaeology of later periods, including perhaps the Upper Palaeolithic and Mesolithic, lent itself to interpretation in terms of social groups and cultural behaviour. Clarke remarked upon this feature when considering the effects of a lower artefact density and the fact that new dating techniques had produced a long chronology; it has been given more extended consideration by Bailey (1984). Whilst there is obviously a difference between the archaeological record of the earlier and later Palaeolithic, what has been surprising for Palaeolithic archaeologists is how the archaeological evidence from the same period can influence our concepts for time, space and population in dramatically different ways.

For example, lithic and faunal assemblages from Palaeolithic sites are not simply of lower density than their later counterparts; their presence (or absence in the case of faunal remains) is critically dependent on the natural processes of accumulation resulting in time-averaged assemblages spanning up to thousands of years in certain cases (Stern 1994). It is no longer possible to talk about accumulations of bones and stones as living floors or the product of contemporary actions without close supporting proof. Whilst this is obviously most acute for the earliest Palaeolithic assemblages, it is still an evident and significant feature later on. Rare, indeed, is the single occupation activity area or site. Despite this time-averaged nature of much of the record, the examination of refitted lithic debitage or of the butchery patterns for individual animals, from some of even the oldest sites such as Boxgrove (Roberts et al. 1998) or Maastricht-Belvedere (Roebrooks 1988; Schlanger 1994), reveals evidence for momentarily discrete behavioural events with a tight relative chronology that archaeologists of later periods concerned with individual human agency might beg for. And again, other evidence, such as hand-axe morphology (Gowlett & Crompton 1994) points to a consistency in manufactured form that spans hundreds of thousands of years, beyond even the lifetime of species, let alone individuals or social groups. What sort of behavioural models can account for this? And how can such a diversity of information be conceived together?

General theory
Much of Clarke’s thinking on the shape of a general theory of archaeology stresses the importance of recognizing how the archaeological record came into being. It is a general theory that is as dependent on understanding sampling techniques and the natural processes of site formation as it is upon interpreting human or hominid activity. There can be little question that in the last 25 years great strides have been made in what Clarke called predepositional, postdepositional and retrieval theory. This is perhaps most obvious for the Early and Middle Pleistocene record, particularly in Africa where the opportunities for hominids as hunters or scavengers and the whole process of the archaeological record as ‘fossils in the making’ has been extensively researched, but similar issues have been addressed for the Upper Pleistocene with regard to, for example, Neanderthal burials or their subsistence economies, or the interpretation of Upper Palaeolithic settlement patterns. The strength of work in this area might be taken as suggesting that the prospect of a general theory of archaeology was less distant than Clarke saw it.

Clarke’s language of samples and traces conveys the initial impression that a general theory of archaeology was a theory of layers, or a regular process where one theoretical activity led logically to the next, after the completion of the theoretical duty at hand. It also partly conveyed the impression that we needed to restore the record to what it had been at the moment of discard before we begin the process of analysis of past hominid activities. A sense of this can be seen in other areas of archaeological work, where there has been a search for that elusive ‘Pompeii’ for all archaeological periods. But perhaps Clarke, himself, was hampered by his own archaeological language, that of the ladder of influence. Clarke’s thinking was not so simplistic. His depositional theory and interpretive theory are surely part of the same. Indeed, they are what most university courses would actually define as archaeological theory.
per se. Clarke’s discussion of concepts and information paints a more complex picture where theory is not a process of unravelling but a point of tension: concepts and information acting like theoretical bungee ropes pulling the archaeologist in opposite directions. The position of equilibrium is not still, but full of potential energy to pull in either direction.

Clarke also mentioned that it was the regionalism, or the periodism of different archaeologies that stopped archaeologists from recognizing the wood of their common theoretical endeavour instead of the trees of their own speciality. Whilst some common interests can be seen in, for example, the approach to sociality (cf. Conkey 1985; Gamble 1998), the composition of fieldwork teams, conference-goers and the majority of references cited still reinforces the impression that Palaeolithic archaeology is a real specialism. Indeed, at a time when governments are forcing disciplines to define themselves and what they teach in the spirit of teaching-quality assessment, many Palaeolithic archaeologists might prefer to see their common interests with zoologists, primatologists, climate-historians and geomorphologists. The skills required to understand our evidence unfortunately now transcend the specialisms that construct us as archaeologists. It is perhaps impossible for anyone to be in control of archaeological theory in the way that Clarke perceived.

Footnotes to Plato??
The philosopher A.N. Whitehead remarked that ‘the safest general characterisation of the European philosophical tradition is that it consists of a series of footnotes to Plato’ (1929). Before you think that I am about to substitute Clarke for Plato, the relevance of Whitehead’s observation, I suggest, lies not in naming Plato, but recognizing that we endlessly, though not necessarily fruitlessly, refine answers to questions we have inherited from long ago.

Whilst Clarke’s ‘loss of innocence’ is not regularly quoted in Palaeolithic writings, Clarke’s theoretical influence on his contemporaries is obvious, and in his defining of questions that we still struggle to answer. The optimism of Clarke’s approach is still invigorating. Lewis Binford, in Bones: ancient men and modern myths (a book dedicated to the memory of Clarke) notes that his Nunamiut work was inspired by Clarke’s call for new theoretical thinking to drive forward the collection of new evidence, in this particular case the manner in which faunal assemblages are produced. The same is also particularly clear in the work of Glynn Isaac and the clarity of his thinking on a range of subjects from the typology of lithic artefacts, to the spatial analysis of lithic and faunal assemblages and the interpretation of the behaviour of early hominids and the factors affecting site preservation, a range that parallels Clarke’s Analytical archaeology. Clarity of thinking in understanding the social regionalism of Palaeolithic behaviour can be seen in the writings of Martin Wobst, Meg Conkey and, more recently, Clive Gamble, amongst others.

Of course we might look back beyond Clarke to the writings of Grahame Clark or Gordon Childe for our archaeological Plato. But that’s another story.

References
It is interesting to reflect that only nine years separate David Clarke’s paper ‘Archaeology: the loss of innocence’ and the publication of Symbolic and structural archaeology (Hodder 1982), which may be taken to mark the beginning of a ‘post-processual’ archaeology. Many of the ideas put forward in that book were being discussed and developed at Cambridge from around 1978. David’s paper, and its publication in Antiquity, may be taken as representing the high-water mark of ‘new’ or processual archaeology in the academy. Almost as soon as the ideas had been presented, and not really very well developed in the practice of doing archaeology, they were under fire and being replaced. Yet David was still attacking ‘traditional’ archaeology, fighting for his own position in the 1973 paper, and putting forward an agenda for the future of archaeology. It was a manifesto for future work. New Archaeology was then 11 years old and had already achieved a certain hegemony in Anglo-American archaeology, at least among younger academics more interested in ideas than recovering and describing evidence. In 1998 what is labelled ‘post-processual’ archaeology differs fundamentally from many of the ideas presented in the Hodder volume and it is doubtful whether anyone would still wish to follow David’s agenda or advocate early ‘post-processual’ ideas. The pace of thinking has inexorably heated up. Both David’s paper and the Hodder book are now primarily of historical interest in the development of a disciplinary consciousness in which archaeology is becoming increasingly self-reflexive, critically interrogating its intellectual presuppositions, procedures and practices.

David Clarke’s work was both visionary and constantly innovative. The ideas put forward in the ‘loss of innocence’ paper, and in his other publications, were subsequently adopted and adapted by many others. There can be no doubt that he was the most intellectually influential British archaeologist of his generation. One can now react positively or negatively to various aspects of his work but it remains fundamental to any consideration of the development of a disciplinary self-image.

His work was consistently felt as threatening by many ‘traditional’ archaeologists spawning numerous criticisms that he wrote dry jargon-ridden texts that could be deemed largely irrelevant because they were difficult to understand. He was complicating things too much. In principle archaeology ought to be a simple practice of recovering meaning from the past with the minimum of theory. David’s abiding legacy has been to make theory and philosophy, and explication rather than description, the core of the discipline rather than being somehow optional extras. Henceforth archaeology had to be primarily an intellectual practice rather than a set of technologies and methodologies.

It is clear that the stimulation for the ideas that David advocated in the ‘loss of innocence’ paper, and elsewhere in his work, were not primarily derived from reading the works of other archaeologists, but drawn from outside, principally from his reading of work in the positivist philosophy of science and structural-functionalist anthropology, sociology and geography. It now seems somewhat ironic that he used these ideas to advocate strongly an ‘archaeology is archaeology is archaeology’ position in which the discipline should develop its own independent theoretical, conceptual and methodological structure tailored to the particularities and peculiarities of archaeological evidence. This theoretical structure was to be applied top-down to inform an understanding of the past. What he did not seem to recognize is that there could be nothing distinctive about archaeological theory when it went beyond a concern with appropriate methodologies for excavation, fieldwork and conceptualization of factors affecting the physical survival of archaeo-
logical evidence, what he refers to as ‘pre’ and ‘post’ depositional and retrieval theory in the ‘loss of innocence’ paper. He was, in part, reacting against the position put forward by some historically minded ‘traditional’ archaeologists that archaeology can only really be a technology for extracting evidence from the past. In this it is distinctive. In all other respects, in what Clarke terms general, analytical and interpretive theory, it shares common features with all the other social and historical sciences. The irony here is that the death of archaeology could only result from the concept of distinctiveness. In this respect David’s paper itself betrays a startling innocence. How could an archaeological theory of society or human action be produced that would not simultaneously be a social and anthropological theory? How could the interpretation of meaning in past landscapes or artefacts, architectural forms etc. differ in any radical sense from interpretative work in relation to contemporary materials? Since the actions of persons in the past cannot be observed, and can only be inferred, archaeologists certainly have more difficulty in interpreting things than an anthropologist, but there is a shared general problem of Otherness and cultural distinctiveness. What David’s work has, in retrospect, stimulated is the intellectual position that archaeologists should themselves be engaged in the construction of social theories rather than leaving it up to other disciplines and then simply scavenging and applying the ideas to explicate an archaeological data-set. Post David Clarke this parasitism and intellectual laziness is simply no longer acceptable.

Just as most of David’s ideas were drawn from reading texts outside the discipline of archaeology, the same is the case for ‘post-processual’ works. Neither ‘new’ archaeology, or what has happened since, could ever have been possible without reading outside the discipline. The lesson (hardly novel) must be that reading only archaeology is bad for disciplinary health and promotes stagnation. This has some rather profound implications for teaching and learning: why, typically, should only archaeological texts take pride of place on undergraduate reading-lists? Why is teaching so much bound up with promoting disciplinary allegiance and asserting distinctiveness? Why are courses in archaeological institutions labelled as being archaeological theory, rather than social theory?

Why should archaeologists think they can learn more from each other in their conferences, seminars, workshops, lectures and publications rather than by talking with outsiders (so-called inter-disciplinary interactions being the exception rather than the norm)? Is this anything much more than a kind of ancestor- and hero-worship (this consideration of a paper by David Clarke being a typical example of the genre), and part of a struggle for resources between competing disciplines in universities with artificial boundaries? Leaving to one side the politics and pragmatism inevitably required for the disciplinary survival of archaeology, is it any longer intellectually necessary, or sufficient, for us to be disciplined? David Clarke’s work began a trend that has rapidly accelerated since, in that what is, or is not, archaeology is now no longer very clear. We have a proliferation of sub-disciplinary labels: contemporary archaeology or modern material-culture studies, pre-historic archaeology, historical archaeology, colonial and post-colonial archaeology, anthropological archaeology, cognitive archaeology, landscape archaeology, gender archaeology etc. We also have a proliferation of theoretical approaches: empiricist, positivist, mathematical, structuralist, symbolic, hermeneutic, Marxist, phenomenological etc. Every archaeologist now works, to a greater or lesser extent, through the medium of a discipline whose hallmark is fragmentation rather than coherence, disunity rather than unity. This was the inevitable result of David Clarke’s attempt to expand disciplinary consciousness and dispel innocence. The trend will inevitably continue in an expansion of the conceptual and material objects of archaeological discourse and inquiry.

In the same year that the ‘loss of innocence’ paper was published Edmund Leach (1973) argued, in a collection of papers edited by Colin Renfrew, that ‘new’ archaeology was about 20 years out of date. Here, then, was a rather sad and depressing time-warp in which a discipline was reinventing for itself the theoretical wheels of a long-since discredited positivism and functionalism in anthropology. The paper was, of course, a somewhat overstated polemic, in that anthropology had hardly divested itself of this legacy. For example, the cultural ecology approach of Vayda, Rappoport and others was very influential at the time, and had a direct influence on David Clarke’s writings.
In hindsight it is clear how selectively and narrowly Clarke and others borrowed and transformed ideas from a wider anthropological, sociological and philosophical literature. Only a narrow Anglo-American branch of the logical positivist philosophy of science was utilized. A radically different and alternative ‘Continental’ philosophy concerned with human action, meaning and intentionality was not considered. Why were the anthropological works of Lévi-Strauss, Geertz, Turner, Douglas and the growing body of work in structural-Marxist sociology and anthropology (all available in the late 1960s and early 1970s and providing major sources of stimulation for a post-processual archaeology) ignored? What of the Frankfurt School of Critical Theory? What of Sartre and Merleau-Ponty? Their works, of course, did not fit into the required totalizing and authoritarian vision (for Clarke, ideally, all archaeologists should share in a single philosophical, methodological and interpretative structure) of what social science was supposed to be all about: too interpretative, too political, too subjective, too disturbing, too uncertain: in short, all too human. Clarke’s utter, and ultimately irrational, idealism in which the social and material conditions of the production of knowledge became strangely presented as a form of rationalist objective materialism in his work. The vision of a scientific new archaeology that he presented could only be promoted out of innocence of anything that might conflict with such a perspective. The contemporary proliferation and pluralism of archaeology in terms of what is studied, and the theoretical approaches taken, have resulted in a situation in which Leach’s accusation that the discipline was profoundly naïve in relation to wider sets of ideas outside its own limited parameters and interests, is now no longer possible to make. David Clarke replaced the innocence of traditional archaeology with a different kind of innocence. The claim at the beginning of the ‘loss of innocence’ paper is that ‘new’ archaeology had ushered in an era of critical self-consciousness, as opposed to a prior phase of mere self-consciousness. It had not. David’s achievement was to make the discipline more self-conscious than ever before. In doing so he created the conditions in which a Pandora’s box of theories and ideas could be opened, leading to the achievement of what he wanted: a modern, dynamic, constantly questioning, self-reflexive discipline. Pluralism, fragmentation and self-reflection, a critical and questioning attitude, a permanent end of innocence, are part and parcel of a post-modern world in which we can no longer afford to take anything for granted.

A ‘post-modern’ archaeology in 1998 is a truly contemporary discourse in a manner in which Clarke’s vision of a ‘new’ archaeology could never be. It is both informed by, and can make a positive contribution to, wider debates in the social and historical sciences about the meaning and significance of social relations, material forms, social reproduction and social transformation. A loss of innocence is dependent on the end of disciplinary isolation and, in this sense, archaeology no longer continues to exist.

References


The dual tasks of this paper are to examine David Clarke's ideas about the development of archaeology as they relate both to the era when 'the loss of innocence' was written and to what has happened since. In his treatment of the history of archaeology offered in that essay, Clarke subscribed to at least two of the key tenets of the behaviourist and utilitarian approaches that dominated the social sciences in the 1960s: neoevolutionism and ecological determinism.

Clarke viewed the development of archaeology as following a unilinear sequence of stages from consciousness through self-consciousness to critical self-consciousness. The first stage began with archaeology defining its subject matter and what archaeologists do. As its database and the procedures required for studying it became more elaborate, self-conscious archaeology emerged as a 'series of divergent and self-referencing regional schools...with regionally esteemed bodies of archaeological theory and locally preferred forms of description, interpretation and explanation' (Clarke 1973: 7). At the stage of critical self-consciousness, regionalism was replaced by a conviction that 'archaeologists hold most of their problems in common and share large areas of general theory within a single discipline' (1973: 7). Archaeology was now defined by 'the characteristic forms of its reasoning, the intrinsic nature of its knowledge and information, and its competing theories of concepts and their relationships' (1973: 7). Clarke looked forward to a fourth (and ultimate?) phase of self-critical self-consciousness, when the new archaeology would monitor and control its own development.

Clarke maintained that these changes could be explained adaptationally. He defined an academic discipline as an adaptive system 'related internally to its changing content and externally to the spirit of the times' (1973: 8). Past archaeological states were appropriate to past archaeological contexts, but 'a new environment develops new materials and new methods with new consequences, which demand new philosophies, new solutions and new perspectives' (1973: 8–9). 'Epistemological adaptation to the empirical content of...new observations' (1973: 11) plays a major role in bringing about disciplinary change. While Clarke saw new problems promoting theoretical diversification and competition, and characterized this as a progressive tendency, he also believed that 'in each era archaeologists represent the temporary state of their disciplinary knowledge by a metaphysical theory which presents appropriate ideals of explanation and procedure' (1973: 12). Such adaptive behaviour transforms archaeology from one 'level of practice' to another. There is a close resemblance between Clarke's explanation of the development of archaeology and the views that American neoevolutionists, such as Elman Service, the young Marshall Sahlins and Lewis Binford held concerning cultural change in general. Clarke believed that other disciplines, such as biology, geology, geography and the social sciences were transformed in analogous ways by similar environmental factors.

Clarke characterized the 'revolution' from consciousness to self-consciousness as mainly technical in nature and the one from self-consciousness to critical self-consciousness as 'largely a philosophical, metaphysical and theoretical one brought upon us by the consequences of the first' (1973: 7). He interpreted the latter changes as adaptive responses to alterations in the social and technological environment of archaeology. The social changes included a vast increase in the numbers of archaeologists and in the resources for carrying out archaeological research. The technological changes were a revolution in the techniques used to analyse archaeological data. Clarke provided a litany of innovations, which summarized the exten-
sive literature that British archaeologists in particular had published on methodological topics during the 1950s. Clarke identified two developments as exceeding all others in importance: computer methodology and isotope chronology. Yet he stressed that in both cases the intellectual consequences arising from the introduction of these new methodologies were of much greater significance than were the methodologies themselves.

By enabling archaeologists to do explicitly what they had always claimed to do implicitly, computer analyses had revealed the erroneousness and worthlessness of many traditional interpretations of archaeological data. Isotope chronologies had indicated the hitherto unsuspected slowness with which changes had occurred in prehistory and called into question traditional culture-historical explanations of cultural change. Although Clarke did not specify it, the hitherto assumed rapidity of cultural change reflected a migrationist-diffusionist view of the past unchecked by any independent measurement of time.

Clarke argued that the entire development of archaeology from the stage of 'noble innocence' (consciousness) to critical self-consciousness had occurred between the 1950s and the 1970s, and hence during the working lifetime of many archaeologists. Yet elsewhere in his paper he referred to the 'scientism and historicism' of archaeology in the 1920s and 1930s, and still more recently in America, and to the importation of the ideas of Spengler, Toynbee, Ellsworth Huntington, and 'the modified Marxism of Childe' as general theory into archaeology. This suggests that Clarke was aware that archaeology was not as devoid of theorizing prior to the 1960s as his developmental scheme suggested.

Clarke clearly explained the development of archaeology in evolutionary and adaptive terms. He viewed the competitive individualism of scholars, and their accompanying search for enhanced employment, as a result of academic reputation, as encouraging innovation. He also believed that individual archaeologists might choose to reject change in the short run, but could do so only at the cost of becoming 'a doomed race of disciplinary dinosaurs' (1973: 8). (In the 1970s the extinction of these reptiles was attributed to their evolutionary inferiority to early mammals rather than to their being the innocent victims of a cosmic calamity.) Yet, whatever role was played by individual archaeologists, stages succeeded each other as a result of the adaptive superiority of particular innovations. The principal causes of change in archaeology were demographic and technological.

There was also no role in Clarke's account for ongoing traditions, since better adapted systems of knowledge obliterated less well adapted ones. Clarke's account is notable for not mentioning a single archaeologist by name, except Gordon Childe. This absence of biographical content seems to reflect more than the reticence of a younger scholar (even one as sensitive to the rules of British academic politics as Clarke appears to have been) to get involved in the dangerous game of praising and criticizing the work of senior colleagues. For Clarke, previous stages in the development of archaeology, and hence the past work of individual archaeologists, were irrelevant except as a prologue to the present. Because the present was shaped by its own functional constraints, the past played little, if any, specific role in accounting for it. Clarke's view of the development of archaeology, like American processual archaeologists' views about the development of culture, were profoundly anti-historical.

Clarke's version of the history of archaeology ignored the series of overlapping, but roughly sequential, paradigms that had characterized the history of archaeology since the mid-19th century. Evolutionary archaeology was replaced by culture-historical archaeology, which in turn had been routed in the 1960s by neoevolutionary archaeology. Each of these paradigms was characterized by its own distinct theoretical perspectives, analytical techniques and social commitments. This refutes Clarke's claim that a 'comprehensive archaeological general theory' became possible only with the development of critical self-conscious archaeology. The history of archaeology also demonstrates that, because the specific understandings of each period are what are transformed by new understandings, changes in archaeological interpretation and practice must be understood historically, rather than simply developmentally, as Clarke had assumed. Established interpretations often survive unquestioned long after the theoretical presuppositions that gave rise to them have been refuted or become unfashionable. Micro-migrations have been retained, even as the larger migrations with which they were thought to have been associated have been abandoned (Trigger 1978).
It has also been demonstrated that the evolutionary and culture-historical approaches influenced archaeology around the world, even if contacts among archaeologists working in different regions were more restricted in the past than they have been since World War II. The universalization of archaeology that began in the 1950s occurred less because of changes in archaeological theory than because isotopic dating for the first time allowed sequences of cultural change in every part of the world to be correlated with each other as well as calibrated calendrically. This permitted the rate as well as the patterns of change to be compared around the world, which in turn greatly encouraged the revival of a comparative, evolutionary perspective in the 1950s. It is surprising that the leading role that Clarke’s mentor, Grahame Clark (1961), played in developing this crucial ‘world prehistory’ perspective went completely unrecorded in this paper. It is also significant that, despite the ever more rapid diffusion of new techniques and theoretical orientations, regional styles of doing archaeology are no less marked today than they were in the past.

Clarke and Binford, like many other young archaeologists in the 1950s and 1960s, myself included, were dissatisfied with the culture-historical approach. Its idealist epistemology, almost exclusive concern with homologies (similarities resulting from historical connections) rather than analogies (similarities resulting from independent development) and invocation of diffusion and migration as the principal explanations of culture change seemed old-fashioned by comparison with the functionalist and behaviorist orientations of the other social sciences and prevented archaeologists from engaging in worthwhile debates about the nature of change with other disciplines.

Yet, in condemning the inadequacies of culture-historical archaeology, both Clarke and Binford ignored the antiquity of this dissatisfaction and of the search for alternative approaches. The latter had expressed itself in the economic explanations that Childe had proposed in the 1920s and 1930s, after explicitly stating his reservations about culture-historical explanations (Childe 1930: 240–47); in the ecological orientation of Grahame Clark and a growing number of American archaeologists, including Julian Steward (also an anthropologist) and Joseph Caldwell; and the settlement archaeology approach initiated by Gordon Willey, which in turn had been encouraged by a growing interest in site plans that can be traced back to the large-scale clearances of prehistoric sites in the United States that had been sponsored by lavish government funding of archaeological research in the 1930s. These approaches signalled a growing interest in functional and in cross-cultural, analogical (and hence evolutionary) interpretations of archaeological data. By ignoring the long history of such dissent, both Clarke and Binford attributed more originality to their own programmes than the history of the discipline warranted.

What Clarke and Binford were opposing were, moreover, different specific regional variants of culture-historical archaeology. If Binford was seeking to replace the 1950s’ diffusionized version of the Midwestern Taxonomic Method that he had encountered in the person of James Griffin at the University of Michigan, Clarke was working to transcend the British culture-history being promoted by archaeologists such as Glyn Daniel, Christopher and Jacquetta Hawkes, Stuart Piggott and Mortimer Wheeler. They invoked the philosophical idealism of the philosopher-archaeologist Robin Collingwood, but did not apply it systematically to their work. Instead they embraced the distinction that historians of the von Ranke (‘show it as it really was’) school drew between facts, which were treated as constituting the basis and ever-expanding core of archaeology, and interpretations, which were regarded as provisional personal opinions. These archaeologists saw their discipline as a way of extending history into the remote past. A strong emphasis was laid on creating narratives and there was a tendency to distinguish between prehistory, which was equated with the synthesis of archaeological data, and archaeology, which was concerned with the recovery and publication of such data. Curiously, although it was identified as merely an expression of opinion, prehistory was regarded as a nobler calling, and one that demanded more academic training than did archaeology. At the same time, the belief that interpretation was inevitably mere opinion encouraged flights of fantasy that Clarke rightly protested turned archaeology into ‘an irresponsible art form’ (1973: 16).

Clarke’s critique of British culture-history was devastating. He argued that, by confusing archaeological time with historical time, culture-historical archaeologists had promoted a form
of diffusionary—migrationary catastrophism analogous to the theories of 18th-century geology. The new, slower time-scales revealed by isotopic dating made it 'exceedingly doubtful' that archaeologists could continue to use the old stock of historical models in a direct fashion. He denounced (without naming him) François Bordes' attribution of variation in Mousterian stone tool assemblages to tribal differences enduring for over 30,000 years as wholly unacceptable since 'political, historical and ethnic entities and processes of these kinds cannot yet be perceived at that scale in that data, even if they then existed' (1973: 10). He thus aligned himself with Lewis and Sally Binford's critique of Bordes without explicitly endorsing their alternative ecological explanation. More positively, Clarke argued for the need to construct a body of theory that could discipline every stage in archaeological interpretation and rescue archaeology from being nothing more than an exercise in creative fantasy. He also implicitly rejected the distinction between prehistory and archaeology.

Clarke's solutions to the problems of culture-historical archaeology, no less than his critiques, reflected the specific nature of British archaeology. Binford, who was trained in the American anthropological tradition, and in many ways was more influenced by evolutionary anthropologists, such as Leslie White, than by archaeologists, sought to integrate archaeology more closely into anthropology. He shared an old and widespread opinion that archaeology had to be a part of anthropology, if it was to amount to anything. To accomplish this, he proposed to use generalizations established by the study of living cultures to infer human behaviour from archaeological data and a strict adherence to ecological determinism and neoevolutionism to account for change. Clarke, by contrast, remained more narrowly committed to archaeology as a discipline, even if, in his opinion, it had originated as 'an arbitrary but commonly accepted partition of reality' (1973: 6). Rather than privileging ethnoarchaeology as a source of insight, Clarke advocated, as he had already done in Analytical archaeology (1968), the competitive use of paradigms focusing on the morphological, anthropological, ecological and geographical aspects of archaeological data. Clarke's views were, in fact, strongly influenced by the New Geography, especially as its approaches were being applied to human geography by another Cambridge academic, Peter Haggett.

Curiously, while Clarke's explanation of change in archaeology conformed closely to American New Archaeology's explanation of change in general, he did not extend this explanation to sociocultural change. He was more aware than was Binford of the complexity both of the archaeological record and of the problems involved in explaining cultural change. Especially in what turned out to be the final stage of his career, when he was writing mainly about specific aspects of European prehistory, Clarke displayed considerable appreciation of the role played by historical traditions, even though he viewed such traditions as being constantly remodelled to varying degrees by adaptive or expedient behaviour. He stressed that 'the explanation of complex events in sophisticated systems is an especially important and ill-understood area' (1973: 15). Perhaps his routine exposure to historians and to the archaeology of the historically documented European past made him more aware of the complexity of change than Binford had been made by his brief exposure to colonial American history. Far from avoiding documentary history, Clarke believed that text-aided contexts were a fertile source of 'vital experiments in which purely archaeological data may be controlled by documentary data' (1973: 18).

Clarke also rejected in this essay and elsewhere (Clarke 1972) the idea that there was a universal form of archaeological explanation that was appropriate for all contexts. The cross-cultural recurrence of a particular house-plan might require a totally different sort of explanation from the collapse of a particular ancient civilization. While, in Clarke's opinion, New Archaeology was an interdependent set of new methods, observations, paradigms, philosophies and ideologies, its creativity lay in the questions it posed rather than in the answers it provided. His description of explanations that rely on multiple re-invention as 'currently fashionable autonomous “spontaneous generation” explanations' (1973: 13) can be read as a specific rejection of the neoevolutionary bias of American New Archaeology. Finally, his claim that much of archaeology treats 'relationships . . . for which . . . patterns . . . nowhere survive within the sample of recent human behaviour' (1973: 17) reads as a specific limitation of the potential role that ethnoarchaeology might play in the interpretation of archaeological data.
Clarke’s resistance to Eric Higgs’ efforts to establish ‘ecofacts’ as the primary data of archaeology led him to reassert, in what he termed a Neo-Montelian fashion, the value of artefacts for understanding past human behaviour (Sherratt 1979: 199–201). In Analytical archaeology, he had suggested that archaeology might become the nucleus of a science of material theory that are latent in any archaeological interpretation: predepositional, depositional, postdepositional, retrieval, analytical and interpretive. These, together with metaphysical theory (defined as dealing with the most general concepts archaeologists use), epistemological theory (dealing with archaeological information), and logical theory (dealing with correct reasoning as it applies specifically to archaeology) would theorize the entire discipline. While American New Archaeology exhibited an early interest in sampling (retrieval theory), its main concerns were with inferring behaviour from archaeological data (interpretive theory) and explaining such behaviour (predepositional theory). Clarke’s view, grounded in the importance that he accorded to archaeological data, generated a more comprehensive and inductive view of theory.

How have Clarke’s ideas fared since 1973? During the 1970s American New Archaeology began to pay more attention to the specific characteristics of archaeological data. This was manifested in the growing emphasis on taphonomy in Michael Schiffer’s Behavioral archaeology (1976) and in the inferring of human behaviour from archaeological data being recognized as the major ongoing challenge to archaeology. This was a challenge that Binford attempted to meet with his elaboration of middle-range theory. These developments moved American archaeology closer to Clarke’s definition of archaeology as ‘the discipline with the theory and practice for the recovery of unobservable hominid behaviour patterns from indirect traces in bad samples’ (1973: 17).

More recently ecological determinism has been largely abandoned, neoevolutionism has waned, and the importance of historical traditions in shaping human behaviour again has been recognized. Darwinian selectionism, various social theories including Marxism, and idealist understandings of human behaviour are all competing for attention. The vast majority of these theories recognize the importance of accounting for homologies as well as analogies and recognize that all human behaviour is culturally mediated (Trigger 1998). As a result, modern archaeology seems more like what Clarke envisaged than what Binford did, although more culture-historical ideas are being incorporated into the new synthesis than either of these ‘New Archaeologists’ would have anticipated in the early 1970s.

In America, Clarke is remembered, as far as he is remembered at all, as a British follower or fellow-traveller of American New Archaeology — a view reinforced by Clarke’s use of the term New Archaeology to label his position, even though he insisted that there were many kinds of New Archaeology. This interpretation grossly underestimates the independence and originality of Clarke’s views. If Clarke was not wholly successful or accurate in contextualizing his position in relation to past or contemporary work, he offered a more prescient and rounded anticipation of the future than did any other of his contemporaries.

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References


A historical discipline must be true to its subject-matter: each phase must be considered in its own terms, not as the harbinger of what came afterwards. Otherwise we fall prey to what Herbert Butterfield\(^1\) perceptively stigmatized as the ‘Whig view of history’: the hindsight, and the moral self-righteousness, of the self-proclaimed progressive. David Clarke wrote at a critical period in the development of archaeology as we know it today, at the transition from what was largely a middle-class recreation to a widely taught university subject with the potential for transforming our understanding of all periods of the human past (and hence also the human present). He lived long enough to capture a vision of what that understanding might comprehend, but not long enough to experience the fragmentation of cultural responses to globalization and the profound social transformation which has accompanied its unfolding. His writings offer a brilliant characterization of a world now in many respects radically different, and a prescription which is worth preserving for an equally different future (Shennan 1989). It is important, therefore, to understand the meaning which the artefact had for its maker — the text for its author — and not to imagine that its value is exhausted by its congruence with present-day concerns.

Those of us who knew David remember his sense of humour as a predominant characteristic, and it is important to read his writings with this quality in mind. ‘The use of scientific techniques in archaeology no more makes archaeology a science than a man with a wooden leg becomes a tree’ was a fairly typical sample of a penetrating observation caught in a crackling phrase. The ‘loss of innocence’ paper is full of them: its form is a sustained metaphor, its content replete with snappy one-liners referring obliquely to the cherished ideas of opposing schools of thought. Consider, for instance, his description of academic specialization (Clarke 1973: 6): ‘... each expert has a specialist territory such that criticisms of territorial observations are treated as attacks on personalities. This gradually becomes a seriously counterproductive vestige of a formerly valuable disciplinary adaptation by means of which authorities mutually repel one another into dispersed territories...’. Old Cambridge hands will recognize this as a caricature of Eric Higgs and the ‘Bone Room’ (and in particular their defensive reaction to David’s foray into palaeoeconomy which was to appear in 1976 as Mesolithic Europe: the economic basis), described in terms of their canonical model of human behaviour — a thinly-disguised adaptation of V.C. Wynne-Edwards’ famous analysis of the mating habits of the grouse (Animal dispersion in relation to social behaviour, 1962)! David was both subtle, and possessed of a sense of fun: qualities all too rare in many areas of archaeology, then and now.

Although the above example is something of a period-piece, some of his targets are still represented in contemporary thought. ‘It is amusing to note that just as “invasion” explanations were conditioned by the metaphysics of the short chronologies, and produced a move towards “autonomous” explanations [Renfrew 1969], so “autonomous” explanations become meaningless amongst networked communities. Indeed the capacity of archaeology to reinvent for itself archaic explanation structures long aban-

\(^1\) Master of Peterhouse from 1955 to 1968.
doned in other disciplines is remarkable — invasion “catastrophism” can be joined by the currently fashionable autonomous “spontaneous generation” explanations and that mysterious “phlogiston” civilization [Renfrew 1972].’ Dare one even attribute premonition to his generalization that ‘disreputable old battles, long fought and long decided in other disciplines, are imported into archaeology to be needlessly refought with fresh bloodletting’, which might describe a fair proportion of the TAG sessions which I have attended since the first one in 1978? It would be a mistake, therefore, to see ‘the loss of innocence’ as the equivalent to Gordon Childe’s Valediction: a retrospective consideration of his life’s work before the author chose to bring it to an end. It is much better to regard it as a puckish sketch of the contemporary scene, by an archaeologist then younger than almost all of the commentators assembled by ANTIQUITY 25 years afterwards (though already author both of a standard two-volume work on British Bell-beakers, and of the first systematic monograph on archaeological methodology, before he had even been appointed to a full lecturership). It is a thorough misunderstanding to seek typological attributions of literal readings of the text, as if a leaden translation could capture the quicksilver thoughts behind it, and the often metaphorical and ironic modes of expression it employed; and it is somewhat self-serving to portray David Clarke as the prodromos to any future messiah. It is interesting that so many of the commentators assume that David’s account was set in a stadial, progressive metaphysic in which each stage formed an inevitable advance over its predecessors: it reads to me much more as a description of how each archaeology is of its time, and can only be understood in its context — ‘precipitate, unplanned and unfinished’ — and will therefore ‘move to new languages and new disciplinary systems not only to answer former questions which could not be answered but also to abandon former questions and answers which had no meaning’. Indeed, a principal contrast between David and his commentators seems to lie precisely in his flexible appreciation of the relativity of archaeological thought to its socio-cultural circumstances, and his awareness of the evanescence of particular attitudes. ‘By the same proposition we can predict the transience of the New Archaeology itself — but we should not confuse transience with insignificance.’ Post-processualists might say the same of their own movement, though they rarely do — perhaps why I suspect that many advocates of post-processualism lack something of his sense of perspective.2

David was not writing a comprehensive intellectual history of the subject (pace Bruce Trigger); nor was he setting in concrete a prescription for the rest of time (pace Chris Tilley): he was pointing out the value of using developing methodologies and technologies, and of treating archaeology as a comprehensive project with many common problems — each, at the time, seen as essentially unique (Isaac 1979). That archaeology is now largely unified at this level, and much of what he wrote now commonplace, is a measure of the degree to which its message has been absorbed (however radical it was for the readers of this journal in 1973 — see the outraged responses to it in ANTIQUITY 47: 93–5, 174–5 though note Hawkes’ sympathy at 176–8). As Guidi notes, it is now part of the everyday agenda of practical archaeology — though David’s formulation is still worth reading for its insights and metaphors (like his description of the archaeological record as a sparse suspension of information particles rather than a Gruyère cheese). In this sense, archaeology is a much more sophisticated dis-

2 It is hard to blame David for what Mike Parker Pearson characterizes as ‘the primacy of theory over detailed knowledge of archaeological material’ — in a paragraph which refers explicitly to his work on Bell-beakers — without recollecting that David travelled the length of the British Isles on his motorbike to draw almost every known beaker for the corpus which remains the most comprehensive collection of this material! (This included beakers in private ownership: David roared up to the front of one stately home to be answered by the butler, with the information that ‘His Lordship is fishing the evening rise, Sir.’)

3 Or rather mistranslation: by taking his metaphors literally, and completely missing the irony of his remarks, Trigger manages to make David subscribe to ‘neo-evolutionism and ecological determinism’, to suggest that he ‘viewed the development of archaeology as following a linear sequence’, that he defined an academic discipline as ‘an adaptive system’; and that his view of the development of archaeology was ‘profundely anti-historical!’ David espoused none of these viewpoints, and it is important not to accommodate him to the attitudes of American New Archaeology, many of which (like Hempelian positivism) he roundly opposed (see Shennan 1989). (This applies to Tilley’s reference to David’s supposed positivism, too.)

4 Or, indeed, his sense of chronology: Chris Tilley may date the onset of revisionism to 1978, but his own hard-line Binfordian essay (‘In this study culture is viewed from a systemic perspective . . .’) appeared in print in 1979 (Tilley 1979: 1).
cipline (hard as it is to use that word in a positive sense since Foucault) than it then was; what is now at issue are the discourses in which its practitioners should participate, and the necessarily political decisions which that involves. One of these is Mike Parker Pearson’s question of whether these discourses should be ‘academic’ or ‘public’; though why — other than as a matter of personal and institutional time-budgeting — should these be considered as mutually exclusive? All of the new discursive participants of recent years have brought fresh issues to the attention of academic archaeologists, who alone have the time and the responsibility to try and make sense of them; here the most sinister threat is the model of university education propounded by politicians, consisting of the delivery of a ‘curriculum’, which threatens critical thought of any kind. Emphasizing our disunity is not the best way to respond, and perhaps solidarity may emerge under pressure.

What I do not understand about Chris Tilley’s piece is his insistence that ‘there [can] be nothing distinctive about archaeological theory when it [goes] beyond a concern with appropriate methodologies for excavation, fieldwork and conceptualization of factors affecting the physical survival of archaeological evidence’. Can the past only be interpreted in terms of the present? Not much hope for understanding the Mousterian problem, then, since Neanderthals are all long dead. Has the past no properties except Otherness and cultural distinctiveness? (How about duration?) Does archaeology do nothing except replicate studies of otherwise observable behaviour, under conditions of poor preservation? (What happened to historical uniqueness?) Surely not. Of course ‘an archaeological theory of society or human action’ is ‘simultaneously a social and anthropological theory’; but it is a social theory informed by a unique source of information, and one scarcely represented in contemporary works which pass as ‘social theory’. Whether future theories should be labelled ‘archaeological’ (because this is a primary source of evidence of human practices since the emergence of the genus Homo) or ‘social’ or ‘cultural’ (since all human behaviour is both), is immaterial, so long as all of the relevant disciplines are well represented in the discussion. What is important is that socio-cultural theory should have an input from archaeology as much as from contemporary social thought, if it is to deserve its name. I fully agree that archaeology’s institutional and intellectual ghettoization is deleterious to scholarship — but this is bad for the social and text-based historical disciplines as much as it is for archaeology. The resultant theory should be no less ‘archaeological’ for simultaneously being ‘social’.

Several of the commentators stress archaeology’s contemporary fragmentation, citing a proliferation of theoretical approaches; though none quotes David’s own anticipation of this situation: ‘A temporary new sectarianism may be the price for the dissolution of the old disciplinary fabric, but this anachistic exploitation of freedom is symptomatic of the rethinking of primary issues’. I believe that he would have relished these debates, and fully participated — indeed, that he would have modified their course in a significant way. While not arguing against change, he might well have argued against change for change’s sake — the desire to invert everything that had gone before, and all too often to replace logical positivism with illogical negativism. While he would have lost none of the excitement, he might well have provided a greater measure of continuity. Periods of self-doubt have always alternated with times of optimistic certainty; but neither attitude is a permanent prescription. While his work will remain forever of its time, it will not always be seen from the peculiar perspective of postmodernism. Matthew Arnold’s poem ‘The Scholar Gypsy’ (1853) describes a scholar whose vision remained fresh, precisely because it had not been accommodated to contemporary concerns; and it might be read as a plea not to see our predecessors only in the light of what seems most pressing today, but to preserve the individuality of their message so that its appeal remains undiluted. It seems an appropriate epitaph:

Thou hadst one aim, one business, one desire! Else wert thou long since numbered with the dead — Else hadst thou spent, like other men, their fire. For early didst thou leave the world, with powers Fresh, undiverted to the world without. Firm to their mark, not spent on other things.

‘The loss of innocence’ is a sparking essay: read and enjoy it for what it is.

5 Tim Taylor’s *Prehistory of sex* (1996) seems to have reached both markets; and, personally, I have found non-specialist audiences as receptive as academic ones on questions such as the use of psychotropic substances in prehistory, in the context of phenomena such as shamanism and archaeoastronomy: see, for instance, Devereux (1997).
References


