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TAG and ‘post-modernism’: a reply to John Bintliff

JULIAN THOMAS & CHRISTOPHER TILLEY*

Two organizers of the TAG meeting at Lampeter, December 1991, take issue with Bintliff’s view of their intellectual position, starting with the label ‘post-modern’.

We are grateful to John Bintliff for his review of the Lampeter conference of the Theoretical Archaeology Group, and for his favourable comments on the organization and running of the event (ANTIQUITY 65: 274–8). We are equally pleased that he chose to frame his remarks in a combative and polemical style. TAG has always been intended as a forum for debate, and it is fitting that it should arouse controversy. We reply here not to silence criticism, but because we believe that Bintliff’s contribution raises a number of issues which need to be addressed, and that in a number of particulars his account promotes some common misconceptions. Specifically, the characterization of contemporary movements in theoretical archaeology as ‘post-modernist’ is rather misleading.

Precisely what the term ‘post-modernism’ means is itself far from clear. It remains one of the most nebulous of terms, simultaneously over- and under-defined. In the broadest sense, it can be taken to refer to a loss of faith in progress and western rationality, a loss of confidence in the fixity of meaning (a ‘crisis of the sign’), a reduction of identities to the status of alternative commodities (Lyotard 1984; Harvey 1989). As such it appears to refer to a set of actually existing circumstances rather than necessarily to an intellectual movement. Clearly, there are those who seek both to theorize and to glorify these conditions, seeing them as ones in which the individual is granted unlimited freedom to invent its own identity (Baudrillard 1990; Kristeva 1986). But on the other side of the debate, Jameson (1984) refers to post-modernism as the cultural logic of late capitalism, a new means of repression through a flickering of images without any substance, that defy serious contestation and ultimately permit acquiescence to systems of repression and exploitation today. Thus while in literature, the visual arts and architecture it is possible to define post-modernist movements (although largely on the basis of their rejection of modernist certainties), in social theory, post-modernity is thought of more as a problem to be

* Department of Archaeology, Saint David’s University College, Lampeter, Dyfed SA48 7ED, UK.

investigated. Those engaged in these debates range from Marxists to post-structuralists to hermeneutic phenomenologists.

If the existence of an intellectual project called post-modernism is arguable, that of a post-modern archaeology is still more dubious. To our knowledge, not a single archaeologist would describe their work as post-modernist, nor was there any use of the term in the session or paper titles at Lampeter TAG. What Bintliff appears to want to do is to gather together a disparate group of emergent strategies of enquiry, label them as 'post-modernist' and then use contemporary critiques of the postmodern condition as a means of writing their obituary. This is surely a sleight of hand. Bintliff repeatedly expresses his reverence for solid facts, yet appears willing to take a cavalier attitude to theoretical traditions in constructing his argument. Thus he manages to claim that Foucault's interpretive analytics (Foucault 1972; Dreyfus & Rabinow 1982) and Gadamer's later hermeneutics (Gadamer 1975) somehow represent variants of Derridean deconstruction, and hence elements of the post-modernist conspiracy. Similarly, Bintliff claims that Renfrew and he are harnessing Habermas' work to positivist, 'realist' scientific research. However, realist theories of science represent a direct attack on positivism (Bhaskar 1975), while Habermas has denounced positivist 'scientific' research strategies as being part of a repressive ideology of technological control in late capitalist society (Habermas 1972).

Having said all of this, Bintliff's fear of a post-modernist 'closed shop' operating at TAG does raise a real problem concerning the perception of archaeology. His title, and that of another related critique (O'Connor 1991) mentions scholasticism, implying that archaeology is being made unnecessarily cerebral and divorced from the everyday encounter with archaeological evidence. There is still a lingering feeling (to which most of us are prone in our weaker moments) that archaeology should be a relatively straightforward business, concerned as it is with real, tactile things. Yet this squares poorly with the realization that the understanding of past societies on the basis of their material remains is surely one of the most complex philosophical problems which human beings have ever set themselves. In order to achieve it, we can and must take whatever help we can from wherever we can find it. Archaeology is hard work.

We sympathize with Bintliff's desire for 'genuinely internal innovations within archaeology' (1991: 274). Nonetheless, it is rarely the case that disciplines function as hermetically-sealed hot-houses. Innovation comes from the gathering and sorting of diverse influences, and their being harnessed to a particular set of concerns (e.g. Gregory 1978). What has been dispiriting in the past has been less archaeology's borrowing from other disciplines than its failure to enter into dialogue, its lack of anything to give back. The newer theoretical approaches which Bintliff criticizes represent less a unified 'school' than a generalized opening of the discipline to the current concerns of the human sciences as a whole, and certainly not the uncritical importation of 'the whole spectrum of Post-Modernism, as if it were a unitary programme' (Bintliff 1991: 276). In reality, there is no 'post-modernist' or 'post-processual' archaeology, so much as a general re-aligning of archaeology to take on board a whole gamut of intellectual positions, many of which are anything but post-modern. What has been good about this is that it enables archaeologists to move outside of their narrow disciplinary boundaries, bringing with them a knowledge of series of areas of competence which are distinctively their own: material culture theory, long-term structural change, the rôle of the past in the creation of modern identities, the temporal and spatial character of social relations, and so on. It is refreshing to talk to social anthropologists, human geographers, sociologists, philosophers and historians and have them remark that archaeology seems both more interesting and more relevant than they had imagined.

The extent to which these developments represent a total break with past archaeological theory is also a question which has to be considered. While a case can be made that the reconceptualization of material culture as a signifying system rather than an adaptive technology represents a fundamental watershed (Tilley 1989: 185), there is also a sense in which the opening of archaeological thought to the social sciences represents a continuation of the project of 'critical self-consciousness' which began with David Clarke (1973: 6). One may discern in Clarke's work a certain tension
between the adherence to the philosophies and procedures of the natural sciences, and his concern with 'controlling models' (Clarke 1972: 5) and willingness to relativize archaeological 'facts' (Clarke 1978: 15). In particular, his acceptance of Kuhn’s model of scientific practice as a social activity governed by high-level assumptions of which the practitioner may be no more than vaguely aware (Barnes 1985) may have meant that a critique of positivism was an inevitability which was only forestalled by his untimely death. When such a critique did emerge (Hodder 1982), it proved possible in some circles to represent this as an aberrant and alien development, with which ‘sensible’ archaeologists need not concern themselves. Moreover, it is precisely Clarke’s ‘controlling models’ which may prefigure the perspectivist arguments which Bintliff regards with such suspicion. When Clarke writes of ‘a partly conscious and partly subconscious system of beliefs, concepts, values and principles, both realistic and metaphysical . . . related to certain aims and goals by the mediating effects of our archaeological philosophy and its values’ (1972: 5), one is put in mind of Gramsci, Foucault and the Frankfurt School. When he goes on to claim that we are all operating conceptual models constantly, whether we are aware of it or not, and that we need to strive to make our inexplicit assumptions into explicit theories, one thinks of Frankfurtian ‘emancipation’, of the hermeneutic circle or of phenomenological reduction.

What is important to stress is that taking these arguments to their logical conclusion, and accepting that all archaeology is written by people in given cultural and historical circumstances, with an incomplete knowledge of their object of study and of the world in general, need not open the flood gates to total relativism. The recognition that we can never create a ‘true’ record of ‘what actually happened’ doesn’t mean that anything goes. What it means is that while we accept that different accounts of the past may be written by different people, they are equally deserving of our scrutiny within a critical archaeology. We are in a position to ask each and every account not only how it corresponds to the data, what is its goodness of fit, whether it is logically coherent in terms of its own premises, but also what values it embodies, whose interests it serves, and what effects it will have in the world. Under these conditions our concern might not exclusively be with ‘the falsification of the past by those who might manipulate it to suit a priori ideologies of Left, Centre, Right, Green, Ethnic, Sexist or Feminist factions in contemporary society’ (Bintliff 1991: 276). On the contrary, we need to consider those who believe themselves to be writing an archaeology which is objective, apolitical and free of ideology. What are their ‘controlling models’?

We are unable to agree with Bintliff that the ‘core’ of our discipline must be provided by empirical facts alone. Such a claim depends upon a division of knowledge into ‘fact’ and ‘theory’ which is impossible to sustain. It is simply not the case that facts exist in a vacuum, waiting for us to come along and observe them, and that the sum of all these facts represents reality. Archaeological evidence is a social product, in that it is defined by our expectations and our assumptions concerning what is and is not significant. It is thus partly constituted by our theory, whether implicit or explicit. Similarly, our observations of phenomena provide no more than a vague impression of the real structures which lie behind them (Bhaskar 1975). What we observe are dead things which exist in the present, and no knowledge of of the phenomena which gave rise to their existence and distribution is possible prior to or in the absence of theory. This is an historical knowledge which implicates an understanding of past social processes: it is not the case that the realm of social relations is one which we can only aspire to consider when we have fully analysed our ‘hard’ data. Archaeological evidence is already inescapably social before we begin to excavate it.

This should not be taken as an argument for a lack of rigour in our collection of evidence or its analysis. The set of techniques and methodologies which archaeology has now developed is fundamental to our enterprise. But theory is equally fundamental, and requires as much of an investment of effort and as rigorous a set of standards as does the empirical aspect of investigation. Bintliff cites his own work in field survey as one area into which problems of assumption and presupposition are unlikely to intrude, the researcher being guided by the ‘surprises’ generated by the encounter with the real world. However, one might object that a
whole series of decisions which precede any such encounter are preconditioned by assumptions which embody social and political values. Where does one choose to survey? How closely-spaced are one’s field-walkers? What materials do they pick up? How experienced do they have to be in the recognition of particular artefact types? How big is the sample frame to be? These are straightforward questions concerning the allocation of limited resources on research work, yet behind them lie assumptions about what evidence is important or significant, how big a site has to be to be recognized, what kinds of activity are to be recognized, how wide a set of interactions is to be taken into account.

The same fact/theory dichotomy underlies Bintliff’s interesting discussion of General Pitt Rivers and his work. Earlier in his article, Bintliff criticizes Thomas for attempting ‘to find a common attitude to human society’ in the works of Martin Heidegger and Raymond Williams. This is not the case. What was attempted was to point out some of the fruitful avenues for research which open from Heidegger’s work. Bintliff, it seems, would rather glibly dismiss him as a ‘Nazi mystic’. Over the years a great many thinkers, few of them fascists, have made use of Heidegger’s work: Sartre, Barthes, the Tel Quel group, Derrida, Giddens, Ricoeur, to name a few. Yet, in the Rektoratsrede, Heidegger did indeed produce some unspeakably vile Nazi propaganda. The point of the recent debate on Heidegger’s politics (e.g. Bourdieu 1990; Lacoue-Labarthe 1990; Derrida 1987; 1989) has not been to declare him off-limits as a thinker, but to understand how his ideas were turned to the service of Nazism. In the same way, we need to understand how the potentially liberating work of Marx led to the Gulag and Romania. Far more important than declaring such knowledge ‘wrong’ or ‘right’, then, is the understanding of how and in what circumstances it was generated, and how it came to serve particular interests. Once we have engaged with them at that level, we as archaeologists can learn from Heidegger’s works on temporality, technology, space and identity.

Exactly the same argument applies in the case of Pitt Rivers. The investigations in Cranborne Chase and elsewhere were entirely guided by Pitt Rivers’ racism, conservatism and evolutionism: his desire to excavate linear monuments in order to substantiate racial boundaries, his interest in recovering skulls which could further his craniometric researches, his search for artefacts which could be forced into universal evolutionary typologies. What is so excellent about Barrett et al.’s (1990) work on the re-study and re-excavation of the Cranborne Chase sites is not that Pitt Rivers’ notes could be picked up and used in abstraction, but that they were reconsidered in the light of a critical awareness of his political motives (e.g. Bradley 1983). Indeed, it was the mistake of assuming that Pitt Rivers’ records (however exhaustive) represented an objective and unbiased account of what he found which helped earlier and less wary writers to construct the myth of ‘the father of modern field archaeology’ (e.g. Hawkes 1947). Using the knowledge which Pitt Rivers produced depends upon understanding the context of its generation.

We wish to end on a salutary note. Bintliff’s attitude to theory, as manifested in this piece at least, is that it is useless unless practically harnessed, and for Bintliff that means furthering better ‘real’ archaeology. This, of course, means primarily field survey and excavation. Archaeology becomes a technology for the efficient extraction of objects from the ground, their documentation and description. Bintliff’s archaeology, if clearly not ‘postmodern’, appears to be positively ‘pre-processual’ in nature. Theory allows better data collection, which allows us to move closer to ‘what really happened’. To paraphrase Bintliff, we would like to suggest that this represents a total abdication of responsibility of archaeologists to their real duty to contemporary society. And that is not primarily to dig holes but to make sense out of the data, to interpret it actively in the present. This requires rigorous debate over theoretical frameworks and concepts, ways of writing and re-presenting the past.

Bintliff notes that an older generation of archaeologists either were not present at TAG or said nothing. This did not come about through any exclusion: indeed, as organizers we spent a great deal of time chasing such papers and sessions which failed to materialize. We suspect that Bintliff is close to the truth when he suggests that his peers ‘are largely caught up in developing research programmes conceived under previous approaches’ (1991: 275), but this may also involve an unwillingness to
engage in open debate over matters of theory. It is precisely this generation that now holds professorial chairs, sits on committees and decides on funding which mostly goes to either field surveys, or for excavation or pollen and bone analysis and the like. When has the British Academy ever devoted a large sum of money to research in archaeological theory? The fact that over 400 people attended TAG does not mean that theoretical archaeology is alive and well. The event is, sadly, quite irrelevant to the majority of the senior practitioners of the discipline: a little steam that will soon dissipate.

There is no time, in archaeology as a whole, for any genuine debate, because the microscope and the spade always come first. A reverence for the artefact is still accompanied by a striking absence of theoretical sophistication in comparison with other disciplines, and indeed some departments of archaeology still do not teach theory at all. University appointments generally demand a period specialization or an ability to teach a technique such as faunal analysis. Theoretical theses for higher degrees are frowned upon, if not positively discouraged. We still have only a handful of theoretical books, the journals are still dominated by excavation reports. After 30 years of intense feminist debate in society at large, archaeology has produced only one edited volume on the subject (Gero & Conkey 1991). The majority of the first generation of ‘post-processual’ archaeologists producing Ph.D theses in the early 1980s were excluded from academic appointments; the same is happening to individuals with a theoretical orientation producing theses now. The anti-theoretical basis of the archaeological academy simply continued to reproduce itself through Bintliff’s ‘science’, ‘truth’, ‘objectivity’, ‘real past’ and ‘professional responsibility’ — terms which, like most others in the discipline, remain accepted and taken for granted, unsujected to any critical analysis. Why is this the case? This question, and others Bintliff and we have raised, requires further debate. We disagree with Bintliff on many points, but salute his willingness to air them openly.

Acknowledgements. We would like to thank Ian Hodder and Matthew Johnson for their comments on an earlier draft of this paper.

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NOTES


John Bintliff* comments:

First of all, many thanks, Julian and Chris, for your response to my review of the TAG 91 Conference: it gave me a great deal of amusement over the Xmas vacation. I will concentrate on certain key points where you appear confused, have misunderstood (or mischievously misstated) my arguments, and are generally in need of Enlightenment (sic).

1 It is difficult to tell if T&T, in claiming with the utmost implausibility, that Post-Processualism has no connection with the Post-Modern/Post-Positivist intellectual movement which is being hotly debated in every other branch of the Humanities, are in sorry ignorance of the contemporary history of ideas, or are merely being ‘ecomonomical with the truth’. However, since the same critical agenda against positivism, science, the ‘new format’ subjects of the ’60s, together with exactly the same ‘reading list’ of Continental gurus, characterize both archaeological Post-Processualism and Post-Modernism, T&T are asking for more than the usual degree of gullibility required for swallowing P-P texts without critical indigestion. Indeed, they would even have us believe that there are no Post-Processualists now, but a mysterious (and doubtless historically inevitable!) ‘general realigning of archaeology’. The ANTIQUITY reader will be little impressed by such rhetorical puffery, and may well not need the confirmation provided by an unfortunate slip occurring later in T&T’s reply, where we are all upbraided for not giving jobs to ‘the first generation [sic] of “post-processual” archaeologists’.

But why the smoke-screen? As T&T admit themselves, if P-P is merely the archaeological variant of Post-Modernism, its credentials are in tatters, having been convincingly shown to be little more than a submissive cultural reflection of the recent transformation in the economics of advanced capitalist society (do read the Jameson and Harvey references T&T rather suicidally cite). Moreover, to judge by the rapidly declining influence of the practitioners of the main movement in every other discipline, its feeble offspring P-P will hardly stretch its baleful shadow much beyond the early ’90s.

2 Since Post-Processualists don’t believe in a real past – (well, some days; other days they write long books about it like The domestication of Europe (Hodder 1990)) – I suppose we ought to expect that their stories about the previous history of archaeology are more Mary Renault than Glyn Daniel. I am pleased to hear that P-Ps have met geographers, anthropologists, historians and so on, and have found things in common; we New Archaeologists were surely fooling ourselves that our joint conferences, seminars, and collaborative field projects with members of those same disciplines over the last three decades implied any meaningful dialogue. Take another P-P fictional text, paraded once again by T&T, that we must praise the achievement of the P-P masters, in taking us over ‘a fundamental watershed’, through the realization that culture is ‘a signifying system rather than an adaptive technology’. The implication here is that Processualists think societies only change in response to environmental perturbations, and all material culture is purely functional. For the simplest refutation we can consider David Clarke’s famous model of a sociocultural system (Clarke 1978: figure 23). Every sub-system of that society has its own dynamic as well as feedback channels to all

* Department of Archaeology, University of Durham, 46 Saddler Street, Durham DH1 3NU, UK.
other sub-systems, the components of the environment also fluctuate independently and show mutual feed-back with social organization, economy and the forms of structures and artefacts, whilst significantly, at the hub of the model is the ‘Psychological sub-system’ - what we would now call the cognitive and symbolic sphere – and this is given a dominant role in the model. Possibly P-Ps are too young to know that it was Binford who taught us to see artefacts not only as tools but also as playing pieces in past social systems and cognitive systems (he termed these aspects sociotechnic and ideotechnic).

3

On the question of objectivity in research, I am tickled by the suggestion from T&T that field survey projects such as my own choose the appropriate distance between field-walkers, or a suitable study region, for social and political reasons. There was I naively thinking we placed walkers at certain intervals to match empirical properties of surface survey data, such as the size range and distribution of activity foci, and selected survey regions on the basis of the neglect of intensive research there and indications that their archaeological record was long and varied!

4

If I didn’t know Julian and Chris so well I would certainly picture them from their writings, sitting in an ivory tower in the pleasant mid-Wales countryside and both with pebble glasses. Their attitude to all other practitioners in the archaeological community, who are not P-P theorists and have other specialist roles and skills, is so myopic. Why, they wail, does research money go to excavators, laboratory archaeological scientists, and scholars from other institutions who cannot be doing any theory since they aren’t using our theories? Don’t people realize that nothing can be said about a site unless the entire social system is reconstructed, unless every burin spall and X-ray diffraction result is mustered for a telling attack on the British class system of the 1990s?

It seems to me the answer must be that the committees of grant-givers are hopelessly enslaved to an outmoded vision of scientific, empirical research, where they actually want some tangible results to emerge from a project, a more-than-passing acquaintance between theory and real archaeological data, objectives that involve concrete problems of human development in specific places at specific times – gosh, what outdated prejudice!

5

Since the 1991 Leicester TAG Conference witnessed another mutually-incomprehensible clash of P-Ps and archaeological scientists, and T&T are plainly still at sea when they come to Archaeology with a capital ‘S’, I feel duty-bound to try to explain to them what science can and can’t do, so that their paranoia in the presence of a white coat or a machine with flashing lights may be eased. Firstly, science is a methodology, not an explanation or interpretation; it observes, describes, takes apart, in as exact a way as possible, using as many ways of looking and measuring as can be shown empirically to yield non-random results. It is at the same time a methodology for analysing how complex things articulate, searching out recurrent association and context, pattern and significance. Once again, the P-Ps have missed my point in citing Pitt-Rivers in this connection: disregarding T&T’s unintentionally comic scenario, where all of the General’s precise field measurements (post-holes included!) are merely reflections of his racism, etc., what I said of him was this: that his insight into the virtues of objective recording on excavation were sufficiently free of his ulterior interpretative agenda, that he not only stated but carried out the principle of recording everything he could think of, not least aspects of his excavations he could see no further use for, except as raw data for future generations. And exactly as he predicted, in recent years John Barrett and Richard Bradley were able to reconstruct very different structures within the enclosures of Cranborne Chase from those the General had postulated, using not only new and re-excavations, but details in Pitt-Rivers’ excavation reports of which their author had not seen the significance.

Most scientists will tell you unconcernedly that the legendary discoveries of science often originated in a whim, a chance happening, or a totally fallacious hypothesis. The sponsors of the research, and the scientists themselves, may have been motivated by the lofty aim of increasing knowledge for its own sake, or more down-to-earth goals which we might want to consider either morally-approved or reprehensible.
What matters in the scientific value of such work has nothing to do with all this, and everything to do with the sophistication of the observations and calculations associated with the empirical results. Sorry, Julian and Chris, but working out how a round-house stood up from the depth and tilt of post-holes is a separate task from arguing that its occupant was an Iron Age chieftain with lots of slaves and a proto-capitalist economy.

Yes, we are back at Hawkes' 'pyramid of inference', and I stick to my guns: as we move from description of structures and patterned artefacts and ecofacts, and how they change over time, into the reconstruction of social and cognitive systems, especially in Prehistory, description increasingly yields to interpretative models which become harder and harder to fit conclusively with the data. At a certain point the high-level questions we ask of the Past are indeed most at home in Philosophy, and characteristically evoke a partisan ideological response rather than a desire to devise a research project where our assumptions could be falsified. This is the point where most thinking archaeologists will part company with T&T: we dare to start at the bottom, and battle our way upwards, as far as we can go, from the first step into the unknown that the trial trench and the initial survey transect offer; we will knowingly risk all our preconceptions about the development of society, the role of the environment, any '-isms' we will have been infected by. For the P-Ps, it all starts at the top: archaeology begins with a ragbag of Continental philosophies and a commitment to particular contemporary political causes. Before the first spade hits turf, the result appears known. The archaeologist is a political activist first and foremost.

My penultimate point picks up from the last. T&T's last form of attack is to seek to portray my position as totally anti-Theory, indeed anti-social, and pre-Processual. The perceptive reader of my ANTIQUITY paper (or of any of my recent books on theory) will not need reminding that I identify with theoretical renewal in the discipline and the contemporary social role of the archaeologist, whilst my views on the relationship between interpretation, theory and scientific description originate from the lasting influence of Clarkian Processualism. In a series of detailed studies I have published since the late 1970s on the post-war development of Theoretical Archaeology (the most recent being cited in the ANTIQUITY review), I have polemized on behalf of that sub-discipline and sought to contribute both to core ideas and the body of applied case studies within it. T&T have almost certainly not read these volumes, probably because they take issue with the P-P programme. Let me quote from Extracting meaning from the past (Bintliff 1988: 12):

An Archaeology that is reflexive in Wilk's [1985] terms is given 'Meaning' in our own lives and let us hope in those of our contemporaries. We have a serious responsibility to make the past 'count', not least to give value to our own lives as professional dealers in the past. On the other hand, only a careful balance between our personal views of contemporary life and an explicit rigour in academic research will prevent our creating a spurious past by conscious or more likely unconscious design.

7

My last point finds T&T shooting themselves through their collective feet. It is a little ironic that they complain about the lack of teaching jobs for P-Ps and their sympathizers, whilst ignoring recent teaching appointments at Sheffield and (with my enthusiastic support) at Durham, and their own recent appointment to lectureships at Lampeter. Feminist and ethnic rights perspectives have become more prominent with other appointments, for example at East Anglia, as well as through the progressive activity of derided Establishment figureheads such as the long-established holder of the Chair of archaeology at Southampton. If we take the 'theory about theory' specialist as a useful contributor to the very broad archaeological community, I think that the 'politically-correct' theorists have done as well as any major sub-disciplinary group in obtaining employment and research grants.

Nonetheless I have a very strong feeling, both from my reading in other disciplines, and from the changed situation at TAG this year, that this lively dialogue with T&T, and indeed the whole Post-Processual agenda, are becoming marginal, ephemeral to the development of archaeological theory for the 1990s. Outside of archaeology, post-positivist Post-Modernist perspectives find less and less support as the failings of the movement are mercilessly exposed by its critics and former adherents. At
the Leicester TAG, in contrast to Lampeter the previous year, a small group of P-Ps were on the defensive before a sceptical audience and a barrage of hostile papers.

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Making an honest man of Oxford: good news for Mali

R.R. INSKEEP*

Readers may know, from editorial comments and notes in ANTIQUITY, and from newspaper letters and articles early in 1991, the deep concern felt over the destruction of archaeological sites in West Africa, particularly in Mali, to satisfy the demands of art dealers for terracotta figurines (see ANTIQUITY 65: 904–5). Oxford University has re-considered its attitude.

Oxford University was involved in the terracotta market because of the role played by its Research Laboratory for Archaeology and the History of Art in dating (or authenticating) many of the terracottas for collectors and art dealers: a process necessary to save the dealers from being gulled by forgers. It struck some of us professionally involved in African archaeology that there was a terrible inconsistency in a great university with a fine track record of teaching and research in archaeology being involved, for pecuniary reasons, in the servicing of so destructive a trade. The matter was brought to the attention of the university authorities and the Committee for Archaeology in a number of letters. The debate that followed was not an entirely smooth one, but the wrongness of the matter was recognized, and in Michaelmas Term the Committee for Archaeology endorsed as official policy the recommendation of its Working Party, appended below. It is not thought that this will end the obnoxious dealing in stolen artefacts, nor yet the destruction of archaeological sites, but it is a step in the right direction, and one which it is hoped that others involved in the business of authenticating for buyers and sellers might follow. It may make it just that little bit more difficult for the dealers to carry on their trade in these particular artefacts, but equally important is the message that, despite the pressures to enter the market place, ethics do still count for something in Oxford. The resolution reads as follows:

With regard to fired clay artefacts of West African origin the Research Laboratory will in future restrict its services to the dating of:

(a) specimens recovered in the course of lawful archaeological excavations submitted by a responsible person. They must be accompanied by details of the site, context, excavator and excavator’s affiliation; by a verifiable certificate of export from the country of origin specific to the object; and by a photograph of the object,

(b) specimens held in the collections of recognized museums (excluding private collections/museums) submitted with full documentation including accession number, date of acquisition and, where applicable, a verifiable certificate of export specific to the object and accompanied by a photograph of the object,

(c) specimens which may be the subject of litigation involving the police or public prosecutors’ offices, accompanied by verifiable documentation.

Dating/authentication of such West African objects will no longer be carried out for private individuals, salerooms or commercial galleries.

* Pitt Rivers Museum, 60 Banbury Road, Oxford OX2 6PN, UK.