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AN UNLOVELY CHILD: THE PROBLEM OF UNPUBLISHED ARCHAEOLOGICAL RESEARCH

Paul Callow

Introduction

This paper stems from a research seminar given at the Department of Archaeology at the University of Cambridge, and takes as a case study the writer's experiences during 1979-84 in preparing a final report on the Department's excavations at the Old Stone Age site of La Cotte de St. Brelade, Jersey, directed by the late Professor Charles McBurney (Callow and Cornford, in press). The wider importance of the issues raised here was made apparent by the problems caused, during the search for appropriate comparative data, by the very high proportion of key European Lower and Middle Palaeolithic sites remaining unpublished many years after the close of excavations, and by the simultaneous existence, under the same roof, of two other posthumous publication projects (Kathleen Kenyon's work at Jericho and that of Eric Higgs on the Palaeolithic of Epirus). Though the issues are addressed from the perspective of a worker in the field of early prehistory, it is clear from the comments of colleagues studying later periods that they are for the most part of general relevance. Similarly, although discussion centres on unpublished excavations (arguably the area needing most urgent reform), many of the points touched upon relate to other types of investigation.

The magnitude of the problem posed by non-publication may be appreciated from the current state of play regarding some British Lower Palaeolithic sites at which major excavation took place 10-20 years ago. Of Caddington, Clacton, High Lodge, Hoxne, Northfleet, Swanscombe and Red Barns, only the first has been fully published (Sampson 1978); in only one instance (Swanscombe) has the excavator (John Waechter) died. One may perhaps hope that the commendable early publication by Green (1984) and his co-workers of a substantial preliminary report on Pontnewydd marks a change of direction, but it is worth stressing that both Caddington and Pontnewydd have yielded manageably small quantities of stone artefacts (even when the finds from much older excavations are included). The problem is not exclusively British; in France, for instance, the situation is little if at all better, notwithstanding the much clearer unity of direction afforded by both the circonscription system and the existence of the Centre National de la Recherche Scientifique, which provides both funding and a regular career structure for researchers.

The La Cotte Project, as it became known, is a particularly interesting subject for examination in some detail because of the range of complications that were encountered, many of them associated with the

(Archaeological Review from Cambridge 4:1 [1985])

need for a drastic re-interpretation of the site; they fall into three broad categories (scientific and logistical; financial; ethical) which provide a framework for more general discussion.

Description of La Cotte de St. Brelade

The site consists of a system of deep, narrow ravines running through a small headland on the south coast of the island of Jersey (Channel Islands). The ravines contain Middle and Upper Pleistocene deposits with a total cumulative thickness of some 40 metres. Most of the layers corresponding to the last cold stage, with an abundant Mousterian industry, were removed prior to the second World War; work carried out in the 1950s revealed the existence of older implementiferous deposits which were the subject of the excavations directed by Charles McBurney from 1961 to 1978.

By 1962 the whole sequence had been exposed, and in 1966 work began at a high level with a view to opening up a new area of about 15 m^2 and obtaining large and well provenanced samples of artefacts. The excavation process was greatly slowed by the discovery of two important deliberate accumulations of mammoth and rhino bones (Scott 1980) in loessic deposits antedating a last interglacial soil; a third, almost entirely destroyed by erosion, may have existed below the others. Between and below the bone heaps there were extremely rich occupation layers, full of ash, flints and small bone fragments. Because the sea entered the fissure system during the last interglacial (about 125 thousand years ago) these survived as a buried cliff, mantled with reworked debris and more recent material. Investigations since McBurney's death have demonstrated the presence of deposits of an earlier interglacial, from whose top a TL age of 238±35 thousand years has been obtained.

The importance of La Cotte can scarcely be overstated. The bone heaps are of exceptional interest, as is the long stratified sequence of archaeological deposits, all of which are remarkably rich in industrial material. The artefacts studied for the publication, probably less than half those recovered over the years, almost equal in number the total inventory of British Lower and Middle Palaeolithic finds recorded by Derek Roe (1968). To put it another way, of the twenty or so richest excavated assemblages from the British Isles eleven are from La Cotte. For Quaternary science as a whole, the combination of erosion at times of high sea-level, episodes of soil formation, varied sediment type, and complex geomorphology and geochemistry over a great depth of deposits provide a means of relating marine and terrestrial events during the Middle and Upper Pleistocene.

The quantity and variety of the information from the site posed special problems in publication. The few reports on other caves of comparable age and richness have generally leaned heavily towards the industries, covering the other aspects rather briefly; more even-handed

SITE AND REFERENCE	NUMBER OF CONTRIBUTORS	LITHIC ARTEFACTS		DEPOSITS (APPROX. CUMULATIVE THICKNESS)
		TOTAL STUDIED	TOOLS	
HORTUS (de Lumley 1972)	36 (44)	4,256	1,830	10m
PONTNEWYDD (Green 1984)	16 (21)	337	184	7m
LA COTTE (Callow and Cornford, in press)	29 (32)	89,947	18,807 11,087	40m

Table 1: Three of the most recent publications describing the detailed results of excavations at European Lower or Middle Palaeolithic cave/shelter sites (see also Figure 1). The number of contributors in parentheses is the sum used for the pie charts, and is the sum of the separate counts for each of the three contents types, i.e. an individual may be included up to three times. The second, lower, tool count for La Cotte is the number of unbroken pieces.

accounts are either based on relatively small amounts of material (see Table 1) or are rather restricted in depth of treatment. Therefore, no obvious model existed for a La Cotte report.

The rescue operation

In March 1979, six months after the close of the final season of excavation, Professor McBurney was diagnosed as having cancer (he died the following December) and he asked the writer to complete the research for publication. This I agreed to do, the task being facilitated by the award that summer of a Science Research Council grant to create a post for studying the industries (with the exceptional provision that, in the event of McBurney's death Professor John Coles would become the grant-holder).

A number of problems very soon became apparent:

1. many stratigraphic questions had not been resolved in the field, and most of the finds processed so far had been sorted typologically but not into assemblages;
2. there was a considerable backlog of finds processing (washing and marking) and other preliminary work;
3. hardly any environmental work had been carried out;

4. there appeared to be numerous errors in the field and laboratory documentation, and in the storage of finds.

A further unexpected complication was introduced by the lack of continuity of personnel during the excavations, particularly at 'junior management' level. The author had dug at the site as a student from 1968-73, and co-authored a short preliminary report (McBurney and Callow 1971). From 1970 to April 1973 he was in charge of stratigraphic recording; Ian Johnson, another student with Palaeolithic excavation experience, continued this work to 1974. However, during the final years of excavation the task was left to students who, though technically competent, had little background in the complexities of Pleistocene sediments, and changes of personnel meant that issues raised in one season were not pursued in the next. During the same period the funding was greatly increased, as was the rate of recovery of artefacts from the much denser layers in the lower part of the sequence: 90% of the finds were made in this period, and 60% in 1978 alone. Consequently, with McBurney's death and the absence of any earth scientist associated with the excavations, all the information needed about the crucial lower layers had to be gleaned from often contradictory field records and from such sediment samples as had been collected; the deposits themselves were inaccessible because the principal sections had been walled up for protection at the end of the dig.

The original schedule had called for completion of initial handling of the finds by the end of 1979; this was achieved, but meanwhile a new problem had arisen. One of the first jobs put under way by the author had been the punching of all provenance data (about 20,000 records) before the compilation of the main database for the artefacts. When the records were sorted according to the coordinates it was discovered that there was massive stratigraphic overlap between assemblages (about 60% in the 1978 seasons, which included all the material from the lower layers). Four options presented themselves:

1. to abandon the study of the artefacts altogether;
2. to assume that the errors were almost exclusively in the coordinates, and carry on regardless;
3. to exclude the most severely affected assemblages, leaving only two for study, from high up in the sequence;
4. to attempt to identify the errors and correct them where possible (otherwise opting for rejection), in the hope that the redefined assemblages would not be heavily contaminated.

Of these, (1) would have caused considerable embarrassment and would have meant that a large proportion of a valuable site had been excavated to little purpose, and (2) was scientifically unacceptable because if the underlying assumption was incorrect the between-assemblage variation would be artificially reduced (and within-assemblage variation increased) with the probability of grossly misleading results. The pressure of time and financial uncertainties made (3) extremely attractive; accurate deadlines could have been set and something would have been salvaged. On the other hand the information loss would still have been very great (and it is now possible to see that the surviving assemblages would have been far from representative of the site as a whole), while the site's owners, the Société Jersiaise, would have had every reason to feel aggrieved. The fourth option was therefore adopted, even though this meant commitment to an unknown amount of effort with no guarantee of final success. By the end of 1980 it proved possible to reconstruct the provenances with reasonable confidence and the rejection of very few pieces; this was achieved by means of a large amount of computer time and somewhat unconventional clerical work, including matching the handwriting on finds labels with the whereabouts of each individual on a given date (it turned out that about 25% of the 1978 finds were incorrectly provenanced; 17% being assigned to the wrong layer). Detailed examination of the collection could then begin.

A second major difficulty arose in connection with the geology. In 1979 the interpretation of the climatic sequence was unchanged from that described in the 1971 preliminary report, i.e. that the deposits antedating the last interglacial were almost certainly laid down under periglacial conditions. Following recruitment of a team to work on the geology it was found that most of the lower layers represented an important interglacial complex, and that views on the environment during the greater part of the human occupation had to be completely changed. It was also realised that, because the complexity of the geomorphology and geochemistry had not been appreciated, a number of natural erosion features had been interpreted as anthropogenic, with spectacular implications for early human behaviour.

Scientific and logistical problems

It will be clear from the above that at La Cotte the inherited circumstances made a text-book operation impossible. To take only one aspect, we were fortunate in recruiting sedimentologists who, because of the site's importance, were prepared to accept samples under less than ideal conditions and to spend much time attempting to sort out problems that could have been quickly resolved from an open section. The initiation, in 1979 onwards, of specialist studies that could better have been conducted while excavation was in progress also affected the work schedule, not least because repeated revisions were necessary with the late arrival of information (of 29 contributors, only three had been invited to participate before 1979). In fact it took over four years to assemble the elements of a comprehensive climato-sedimentary model for the site; only then was it possible to explore the full implications for the human occupants. Equally, because of the backlog of finds processing and the delay caused by the provenance errors it was not discovered until 1982 that the description of the industries based on the 1961-2 excavations was misleading, and that it would be necessary to rethink the objectives of the study.

Another consequence of the scarcity of specialists at the time of the excavation was that in the 1980s a great deal of effort went into

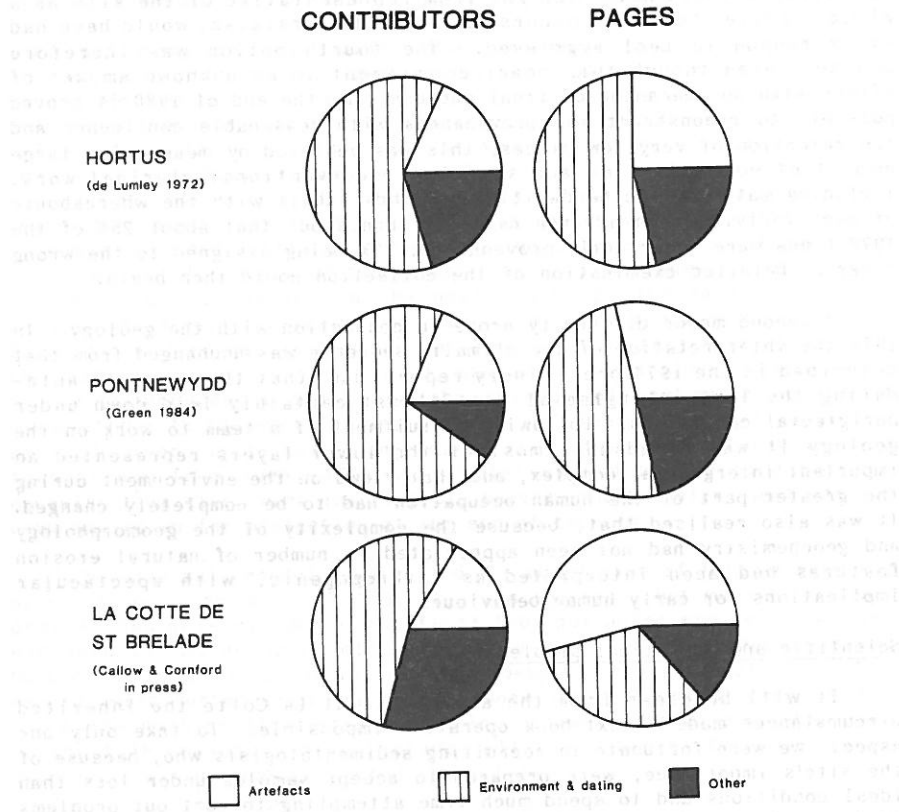

Figure 1: The contents of the reports on the sites listed in Table 1. The relatively large amounts of space devoted to the 'other' category for Hortus and La Cotte arises from a greater content of behavioural information than at Pontnewydd.

recruitment and briefing. As a result, other new and potentially fruitful lines of research could not be pursued; this is reflected in the proportion of the publication devoted to environmental matters (Figure 1), which is scarcely adequate given the extraordinary complexity of the sequence. The fixed-term contracts of the project staff (based not on an evaluation of the work needed but upon administrative regulations) worked against the flexibility of response demanded by the problems

encountered; it is likely that a similar conflict of interest would have arisen had responsibility for the investigations been taken over part-time by colleagues with teaching commitments. One consequence was that in the interests of speed it was sometimes necessary to work inefficiently (and hence expensively), which may seem a contradiction in terms but is exemplified by the process of assemblage redefinition already mentioned. This was essentially iterative, with several passes through the artefact provenance data. In the interests of strict economy, physical handling of the pieces should have waited to the end, but this would have increased the duration of the project by the time taken to handle some 100,000 pieces. By immediately implementing the corrections suggested during each pass (i.e. working in parallel) the add-on time was kept to a minimum, but at the cost of considerable extra handling.

Leaving aside the problems specific to La Cotte, where at least there was no discontinuity in the work, a wide range of difficulties may arise as a direct result of long delays over completion of a piece of research. One of the most common is dispersal of the collections and/or loss of documentation. Just as serious is that the reasons for which the work was carried out in the first place may be overtaken by theoretical developments, or by discoveries at new sites, so that insufficient information exists to interpret the results in an up-to-date context.

Funding

Several points deserve attention (see Figures 2 and 3). Firstly, the cost of the fieldwork was a very small proportion of the whole: of the order of 10%, if costs borne by various institutions and not charged to the project are included. It is worth noting that, because of the Department of the Environment's failure to recognise that not dissimilar arithmetic applies to British rescue archaeology, a drastic revision of its financial policy was required at the beginning of the 1980's (Wainwright 1984). Secondly, the bodies funding the excavations were not in a position to contribute substantially to the post-excavation requirements. A number of people had therefore to devote considerable amounts of time to seeking grants from many sources. A particular cause for concern must be that an unforeseen consequence of the excavations was the imposition of an obligation on parties not originally involved in decision-making (of course the implications of this extend beyond questions of finance). Thus when the more usual supplies of scientific funding showed signs of exhaustion it was necessary to fall back on Jersey sources (private and public). As it happens, Jersey is a prosperous island but not all other communities could have risen to the occasion.

Ethical Problems

Anyone taking over someone else's research is naturally concerned to minimise the exposure of his/her predecessor to adverse comment. At

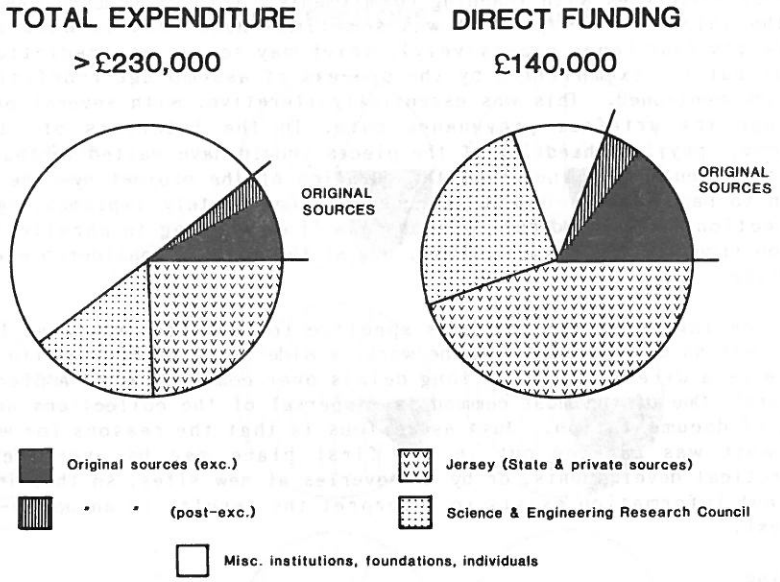

Figure 2: The La Cotte Project: sources of funding. The figures are approximate, as some items have had to be estimated; also the value of workspace and other facilities provided by the Dept. of Archaeology in Cambridge has not been included.

the same time, this goal may conflict with other responsibilities. In the short term, the most painless approach to La Cotte would have been not to ask awkward questions, basing the excavation report on the conclusions reached by McBurney and avoiding the creation of a large team of contributors. But apart from the scientific impropriety of such a policy it could not have survived the attentions of the first independent investigator to study the evidence once this had been returned to Jersey. This course would thus have (rightly) damaged the reputations of the participants without benefiting that of McBurney. Of course those putting up money also had a right to expect that the scientific issues would be placed before excessive loyalty. Moreover, with the benefit of hindsight it is possible to say that the scientific results would have been grossly misleading.

On the other hand, as coordinator of a large number of specialist contributors of whom most were new to the project, the author was faced with another and less straightforward conflict. Decisions taken earlier

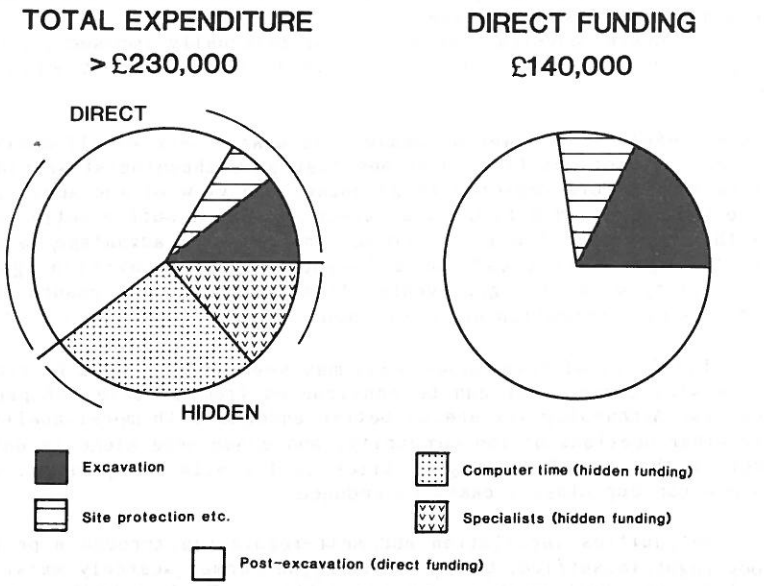

Figure 3: The La Cotte Project: disbursements. The specialist work covered by direct funding has been included under the 'Post-excavation heading, with the basic finds and records processing.

severely limited the opportunities of such colleagues to give of their best, and it would have been unjust to them to have glossed over the circumstances under which they were working, even though this meant laying bare many of the less satisfactory aspects of the research.

Turning from the particular to the general, it seems to be a frequent experience among those who have taken on work under similar circumstances that these delicate issues are not universally appreciated, either by friends and colleagues of the deceased in the case of posthumous projects, or by the original worker if still alive. Indeed, several cases of deliberate obstructiveness or damaging allegations have been brought to the writer's attention over the years.

Reducing the damage

Why does research so often remain unpublished? The following explanations cover the great majority of cases:

1. lack of commitment (loss of interest or, all too frequently, precedence given to career advancement);

2. lack of financial support;
3. a stalled project (because the task is too difficult or the original work was inadequate);
4. death, physical incapacity, or externally imposed circumstances (e.g. increased workload in a University department with 'frozen' posts).

Olsen (1980), in a powerful assault on unnecessary or ill-advised excavation, has set out five questions that an archaeologist should ask him or herself before deciding to go ahead. In view of the above list, two more should be added to Olsen's catechism: am I sufficiently committed, to the point that I will subordinate my personal advantage to my scientific responsibilities? Have I made adequate provision against the possibility of my being prevented from continuing, to ensure that delay and loss of information are minimised?

The first of these questions may seem unrealistic in view of the successful career that can be constructed from a series of preliminary reports. Archaeologists are no better endowed with moral qualities than are other sections of the community, and conscience alone is unlikely to prevent the sacrifice of good sites on the altar of personal ambition. So how can our class 1 cases be reduced?

Antiquities legislation and self-regulation through a professional body ought to suffice, but in Britain the former scarcely exists and the latter lacks both members and recognition. Funding bodies are a third potential method of control, but only if these can be made to recognise the implications of the destructiveness of much archaeological work (compared to that of other disciplines). When the money dries up, more than money or time may be lost; this in turn calls for greater willingness to give continuation funding and closer scrutiny of proposals (thus if after a couple of seasons' work at a Pleistocene site no earth scientist is active there, it is time for some searching questions).

The fourth means of regulation is provided by the landowner; not all are interested or responsible, but a considerable amount of excavation occurs on land owned by the State, local learned societies, and similar bodies. Consider, in the case of research digs, a contract obliging the director or his/her institution to take out a substantial life insurance policy to cover the cost of publication in the event of his/her demise (a first charge on his/her estate in the event of non-payment of premiums, with disputes over quality to be settled by arbitration). This would certainly prevent unnecessary excavation and, because non-publication would occasion extra expense, would also discourage delay (life would also be easier for professional executors).

The second question proposed above calls for no more than the application of simple good practice:

1. recording should be very detailed, on the assumption that someone else will have to make sense of the evidence; erroneous records should be marked as such as soon as identified, to avoid confusion (memory cannot be relied on anyway). This carries with it an obligation to maintain adequate staffing;
2. a very full account of the interpretation of the site, and the reasoning behind it, should be kept up to date and regularly discussed with a deputy;
3. no new season should be commenced until a comprehensive account of the previous one has been prepared, all the finds processed, and any outstanding stratigraphic or other problems identified;
4. a proper system of quality control is essential to ensure that progress is real rather than illusory;
5. at any time the amount of work required to achieve final publication should be kept to a minimum (this is inconsistent with leaving the whole of the finds analysis and specialist studies to the end); maintenance of forward estimates of the outstanding costs may supply an incentive to keep these under control.

In fact by behaving as though he/she expects not to live to complete the work the excavator can do him or herself a favour, as the final stages will be very much easier as a result. Those digging in the developing world often have no choice but to conduct the secondary processing of finds in parallel, in the country concerned -- a policy which should be more widely adopted (with the development of the on-site computer there may soon be little excuse for leaving a substantial amount of the work to be dealt with after the close of the excavations). As with La Cotte, many of the highly specialised studies do not form part of the budget anyway, so by minimising the chargeable residue (and hence the size of the grant required) the prospect of completion should be much improved.

Finally, therefore, it is up to all archaeologists to match their research goals against available resources and to ensure that the latter are put to the best possible use. Because of the ethical responsibilities implied by the act of excavation in particular, if individuals over-reach themselves they, or others after them, will be forced into a commitment which can only be met with difficulty, but which cannot be abandoned with honour.

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COMMENTARY

Theoretical Archaeology Group (TAG) Conference 1984

The sixth annual conference of the Theoretical Archaeology Group (TAG) was held in Cambridge between the 14-16th December, with the aim of integrating theory and practice. ARC invited archaeologists from the Universities of Glasgow and Cambridge and from Norfolk Field Unit to either review the conference as a whole or to comment on particular sessions. This year TAG moves north to Glasgow for the period 16-18th December, and details will be available from: 'TAG Organising Committee', Dept. of Archaeology, The University, Glasgow G12 8QQ, Scotland. Proposals for sessions should reach them by 14 June.

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TAG '84: A Review from Cambridge

With a subtitle 'Theory and Practice' the object of TAG 1984 was clearly to attract archaeologists from outside academe. Past TAG conferences have tended to suffer from an overpreponderance of either rarefied debate or straightforward methodology dressed up as theory. By attempting integration the organisers were seemingly determined not to have one without the other. Theory must go with practice, which means academics must meet with fieldworkers, an all too rare mixture at British archaeology conferences. Having offered the punters something the organisers clearly thought was wanted, to what extent then can the 1984 TAG be said to be a success?

This cannot be answered unequivocally, because the programme, well established by precedent, entailed concurrent sessions at all times. Since TAG this year was in many respects trying to break free of old habits I decided to attend those sessions that promised discussion on practice. The four sessions that I attended were 'Archaeology and Landscape Conservation', 'Archaeology at AD 2000 (± 50)', 'The Dissemination of Archaeological Knowledge' and 'Theory and Practice in Plough Zone Archaeology'. A seemingly happy blend of topics and issues pertinent to the profession, the funding bodies and the public.

On Friday afternoon the rôles of archaeology with respect to landscape conservation were broached, but the conflicts of interest and opinion that face the profession revealed by this session were to be raised again in other papers and discussions. Much of the British rural landscape is currently under 'threat' from agribusiness, in the sense that fields are being progressively amalgamated, with a consequent reduction in hedgerows, ponds, woodlands, heaths and so forth. The papers by Richard Muir and Tom Williamson were directly concerned with this threat, and Muir's title, a calque on Hoskins' classic essay, made his views clear. "Unmaking" may indeed be an appropriate conceptualisation of the ever increasing eradication of traces of earlier humanly-constructed landscapes and environments. Certainly the rate of destruction, as ably demonstrated by Williamson, is distressing. If an equivalent number of the upstanding earthworks had been destroyed as miles of hedgerows in recent years, I doubt if any archaeologist worth her/his salt would have let the matter rest there. Empassioned pleas must however be balanced with a sound grasp of the existing legislation and possibilities for