

adjusting for partial recovery, which would alleviate the necessity for water-sieving the entire site. But these are specific solutions, and we must beware of over-generalizing or seeking that unattainable ideal: a panacea for archaeological sampling problems.

Dr Graeme Barker writes:

I read John Cherry's note with great interest. I was a little perturbed to see his inference from my note that I was *not* recommending water-sieving on the basis of the Monte Covolo example—I surely was. Only intensive processing techniques of this kind will guarantee the systematic recovery of very small bones or pieces of bone surviving in the deposit and many of them—fish bones, for example—will be extremely important. The note was intended to widen the discussion to the problems posed by much larger sites than Monte Covolo, such as many rescue sites in this country. However, my remarks were simply about excavating the kind of faunal material needed for the economic analysis of a site. I fully concur with John Cherry that there is no panacea: a technique recommended for recovering bones will probably not be equally suitable for microliths, beetles' wings or wagon burials. It has been shown at several sites (as now at Phylakopi) that intensive sieving usually produces a much higher percentage of meaningless blobs of

pottery than undiagnostic pieces of bone. The different questions asked of a site, different research priorities, different soils, different environments—of course they will all play a part in the choice of excavation procedures.

However, I do think that the point is worth emphasizing about animal bones. Many more excavators in this country are now collecting animal bones than was the case a few years ago. Nevertheless, from my own fairly limited experience at Sheffield, at the receiving end of requests to work on faunal samples, I think that it is still true to say that there are excavators at large who, whilst appreciating the value of economic information about their site, have much less appreciation of the recovery requirements involved. In the case of animal bones, the variables are so enormous that I have grave doubts about John Cherry's final point, that in time we might be able to develop 'correction tables' to juggle with the data after excavation; although that is not to say that we should therefore relinquish recovery experiments. However, although you may not miss as many bones on a small as on a large site, reliable faunal samples only come from intensive processing. There isn't really a second-best procedure at the moment which produces a second-best sample of bones—faunal samples tend to be either adequate or useless for analysis.

Glozel and the two cultures

Professor Colin Renfrew attended the Archaeometry and Archaeological Prospection Symposium for 1975 held in Oxford, at which three papers on Glozel were presented. He here summarizes these papers.

Glozel remains a puzzle. Despite the reservations expressed by the Editor of *ANTIQUITY* (XLIX, 1975, 2-4) and held in private by many archaeologists, I found myself, supported by Professor Christopher Hawkes, alone among those present to voice unease at the present situation. For whatever archaeologists may say in private, there is a growing consensus among the community of archaeological scientists, as reflected at Oxford,

that the Glozel finds are genuine and date from the later part of the first millennium BC.

The history of the Glozel affair was reviewed in a colourful and detailed paper by Dr V. Mejdahl of the Atomic Energy Commission Research Establishment at Risø in Denmark. Dr Mejdahl did not hide his view, in his account of all the suspicious goings on in this murky story of dissension and contention, that the authenticity of the finds could be upheld. But his presentation was well balanced. His conclusion, that in the light of recent research the history of the Glozel affair cannot be used definitively either to refute or authenticate the finds, is an entirely reasonable one.

Dr H. McKerrell of the National Museum of Antiquities of Scotland gave a summary account, written jointly with Dr Mejdahl, of the laboratory examination of a number of Glozel objects which supported the findings already reported by them and their co-authors in *Antiquity* (XLVIII, 1974, 265-72). The thermoluminescence evidence is the strongest, and the important point was made that some of the inscribed tablets have a vitreous glaze which has flowed over the inscription, suggesting that the tablet was fired to a high temperature after being inscribed, and hence that the TL determination does indeed date the inscription itself. The paper seemed to me clear and well-documented, and the results have been obtained in more than one laboratory. No-one has yet come up with clear reasons of a technical nature for doubting the TL dates (although the proximity of a uranium mine to the site has been mentioned by Dr M. J. Aitken as a point of interest) and the validity of the findings has not been seriously questioned.

The third paper, by Dr B. S. J. Isserlin of the Department of Semitic Languages, University of Leeds, was a cautious treatment of the Glozel script. Affinities with the Phoenician, Latin and Iberian scripts were discussed, and the possibility that the inscriptions might be a modern concoction was firmly kept in view. After a careful discussion, Dr Isserlin took the view that no firm conclusion could be reached. From the epigraphic standpoint the tablets could on the one hand be regarded as fakes, but on the other hand it would not be impossible to view the inscriptions as genuine, and related to some extent to examples of the Iberian script.

The three papers, taken together, suggest strongly that the pottery and terracotta objects from Glozel, including the inscribed tablets, should be regarded as genuine, and with them, presumably, the remainder of the material. No one could claim that the presentation by Dr McKerrell and his colleagues had been other than lucid and scholarly. Where then the puzzle?

It is here that I should report that, although I have several times tried to clear my mind of prejudice and regard Glozel as genuine, in the light of this TL work, I still find it beyond my

powers of imagination to take Glozel entirely seriously. This confession may at once be seized on by scientific colleagues as merely another subjective judgement by an archaeologist too soft-headed to face up to the hard realities of scientific data. But archaeological reasoning is not necessarily entirely subjective, and it can, I think, be argued that the Glozel finds so far defy three coherent principles with which genuine archaeological sites almost invariably conform. It is, I believe, worth outlining these arguments, since so far there has been no considered archaeological response to the very strong case put forward by McKerrell and Mejdahl.

1. The finds as an assemblage, and the majority of the finds individually, are without significant parallels elsewhere, either in the same region or outside it.
2. The assemblage of finds, which is firmly dated by TL, contains no single object typical of the very well documented cultures of that region and period.
3. The assemblage shows serious chronological inconsistencies which, from the stylistic point of view, are difficult to reconcile with the authenticity of all the objects.

Of these, the first is perhaps the least important: there is nothing to prevent the discovery of something new and unique. But new discoveries, like Çatal Hüyük, or the Mycenaean Shaft Graves or the early Olmec sites, each of which was our first introduction to a new period or style, never stay unique. Glozel is nearly fifty years old, yet no persuasive and comparable finds from good contexts have been made elsewhere. If the site is genuine, it is perhaps time they were. Special preservation conditions, which might apply if we were dealing with wooden objects for instance, do not apply.

The second argument is quite separate from the first. If the site really belongs to the Hallstatt or La Tène periods, or indeed to the Gallo-Roman period, it seems inconceivable that no single find has been made there recognizably at home in the French assemblages of the time—no sherds of typical pottery, no coins. I can think of no archaeological site with abundant finds, that does not yield at least some material

NOTES AND NEWS

common to the cultural background of the time.

The third argument does not, of course, apply to the couple of neolithic axes in the assemblage, nor yet to the absence of metal, although this is of note when bone is well preserved. It refers to animal engravings on bone, including one of a reindeer, which are recognizably similar to palaeolithic engravings in style, and unlike anything from later French prehistory. Some of these engravings also bear signs of the Glozel 'script'. It is not easy to see how this could have come about other than by fraud.

I have tried to express these criticisms of Glozel in general terms, so that the entirely exceptional nature of the position may be perceived. I have not used the obviously subjective argument that the finds look like crude fakes, which to me they certainly do, because objects in a new and unfamiliar style have in the past been so regarded (the Mycenaean finds are an example) and have later come to be accepted as genuine when other supporting finds were made.

My point then is that there are serious grounds for unease, and something of a gap between the archaeological scientist and the archaeologist. Certainly it is true to say that few archaeologists would have taken seriously any fresh claim for the authenticity of Glozel, had it not been backed by such firm scientific arguments.

The solution should be quite simple. It hinges on questions of context. The 'excavations' at Glozel have all been of such an execrably low standard that we know now virtually nothing of the context of any of the finds. Any competent excavator could determine, in under a week, whether or not any new finds which he may make are from a context undisturbed for two thousand years. If the site is genuine he will resolve my second and third points, and we shall simply have to learn to live with the first. It is clear to me that no definitive agreement will be forthcoming about Glozel until there is a re-excavation, carried out to modern standards, with the participation of soil specialists experienced in archaeological problems. It is crucial that the excavation be con-

ducted by excavators who command the respect of their professional colleagues, as do many in France, such as MM Bordes and de Lumley, with an international reputation.

It could, of course, be argued that Glozel is no longer a very important site. Had it been of early neolithic date, as its early proponents claimed, it would have been of world-wide significance. If it is of Iron Age date it is simply a strange assemblage, a further curiosity of the European iron age with some new and interesting features. It would be a mistake, therefore, to exaggerate the ultimate archaeological significance of Glozel.

What interests me at the moment, however, is the way the controversy is developing, betraying as it does, a serious lack of communication between archaeologist and scientist. At first my feelings were that unduly sweeping claims of an archaeological nature were being made by the archaeo-physicists on the basis of insufficient scientific data. Now I find it extraordinary that no specialists in the European Iron Age have come forward to comment on the situation. Why this year, as last year, was there no strong archaeological representation at the Oxford Archaeometry conference, so that the archaeological as well as the technical problems of Glozel could be expertly debated?

It is over a decade since C. P. Snow discussed, in a rather portentous lecture, the existence of two cultures in our society, in essence a scientific and a humanitarian tradition. And at that time there were many who claimed that archaeology is a bridge between the two. Since then the discipline of archaeological science has grown very considerably, to the great advantage of archaeology. That the archaeological response has not been commensurate is reflected in the attendances at the Oxford conference where archaeological scientists outnumber archaeologists in the ratio of 10:1. This is seen in microcosm in the new *Affaire Glozel*. So far, with their impressive TL data, Drs McKerrell and Mejdahl have made a strong case, and it is one which can only be countered by careful criticism and analysis of the finds, based on general principles, rather than on a purely personal reaction to the material.

The whole story is a colourful one, and it would be a brave man who would dismiss the work that has recently been done, or cast aspersions on the thermoluminescence method in general. I have considerable respect for both, but feel that it is time the serious and weighty objections to Glozel were set out. For my own

part, as one who is not a specialist in the French iron age, I cannot suppress a feeling of puzzlement. There is something about Glozel which, to use the jargon of the dealer, is 'not quite right'. I forecast further surprises.

COLIN RENFREW

A second 'Affaire Glozel'?

Dr S. E. Warren, who is Senior Lecturer in Physics in the Postgraduate School of Studies in Physics in the University of Bradford, was also at the Oxford conference and heard the papers summarized by Professor Renfrew. We invited him to make some comments, and here he also refers to the papers on the same subject presented at the 1974 Oxford symposium.

We are, I believe, in danger of creating a second Glozel affair in which scientists maintain that the Glozel finds are genuine while archaeologists argue that such an acceptance would violate the principles on which archaeological reasoning is based. The initial challenge to the validity of the thermoluminescence dates released at the 1974 Symposium and now published (McKerrell *et al.*, 1974) in more detail was, in my view, perfectly justified at that time since a quoted age of 1800–2600 years BP was so much at variance with the current interpretation of Glozel as an archaeological fraud.

The contributions by McKerrell and Mejdahl to the 1975 Symposium can be summarized and interpreted as follows: a wider range of artifacts have now been dated to the same period and this is supported by a C-14 date on associated material; the possibility that the published TL dates were in error because the clay had not been fired to a sufficiently high temperature to release the previous geological stored energy, has been disproved by thermo-gravimetric analysis; the possibility that the artifacts might have been subjected to ionizing radiation subsequent to their discovery can be discounted because consistent dates have been obtained for other clay artifacts found in recent soil monitoring at Glozel by McKerrell and Mejdahl; in spite of the reported presence of a uranium mine in the vicinity, the possibility that this has pro-

duced an exceptionally high environmental dose can be discounted partly on the basis of a consistent zircon inclusion date and partly through a TL date for mortar from the kiln consistent with its current interpretation as dating to the medieval period.

There seems little doubt that the consonance so lacking in the archaeological record is to be found in the current scientific examination of the artifacts. The present impasse would be resolved if it could be demonstrated that the artifacts are fakes manufactured from genuine but featureless fired clay and bone found probably at or in the vicinity of Glozel. McKerrell has, however, put forward one crucial piece of evidence in support of authenticity, namely that part of the inscription on one of the tablets is sealed by a vitrified layer and that its TL date is consistent with the others, implying that the inscription itself is about 2,000 years old. This evidence is certainly persuasive but before accepting it one would wish to see further tests carried out in view of its importance. It is possible that the vitrification has been produced by fluxing and flash heating the surface (for example by a blow lamp) without releasing the stored energy in the fired clay below the surface layers. The position at which the sample has been removed for TL dating is clearly important in this context and so is the detailed chemical analysis of the vitreous layer and the clay. If the vitreous layer does have the same composition as the fired clay then a uranium content of 6–7 ppm. would be just sufficient to permit fission track dating. A consistent date would put beyond reasonable doubt the authenticity of the inscriptions. If the material proves unsuitable for such a test then we must return to the proposition that the inscriptions and the arti-

NOTES AND NEWS

facts have been fashioned out of previously fired clay. Mejdahl commented on the softness of the clay fragments found in their recent digging. If this permits reshaping (as distinct from carving) of the fired clay when wet then there should be a variation in the direction of remanent magnetization within the fabric of some of the artifacts. Crawford (1927, 182) reports that the pots have been formed by a hollowing out technique and these, together with the phallic symbols, would merit sectioning and testing.

It is to be hoped that Drs McKerrell and Mejdahl will continue their vigorous and scholarly examination of the Glozel objects,

perhaps including some of the destructive tests which I have suggested. It is good to hear that a detailed inventory is to be made of all the objects (McKerrell *et al.*, 1974) and that this may mark the beginning of a closer collaboration among archaeologists and archaeo-scientists on what should be a common problem. Glozel provides a unique opportunity for collaboration both in the field and in the laboratory. Let us hope that the opportunity is not wasted and that an acceptable solution can be found.

CRAWFORD, O. G. S. 1927. *Antiquity*, 1, 181-8.

MCKERRELL, H., V. MEJDAHL, H. FRANÇOIS and G.

PORTAL. 1974. *Antiquity*, XLVIII, 265-72.

Thermoluminescence and Glozel: a plea for caution

We print here a comment by Dr Martin Aitken and Mrs Joan Huxtable on the McKerrell et al. paper on this subject (1974, 265-72). Both are members of the Research Laboratory for Archaeology and the History of Art of Oxford University. We have asked the authors of the original paper for their comments which we hope to publish in the December 1975 number.

In the December number of *ANTIQUITY* the results of a '... thermoluminescence investigation of a wide range of ceramic objects from Glozel...' were reported (McKerrell, Mejdahl, François, and Portal, 1974) with the conclusion '... that none of them can be the modern forgeries generally believed, and the site dates to within the range 700 BC to AD 100.' These findings are the subject of continued controversy and before the TL is woven inextricably into the fabric of the Glozelian cause we wish to make a plea against too ready an acceptance of conclusions that are based on work that to our mind is either incomplete or incompletely published. We comment here solely from the point of view of TL specialists. As such we do not presume to consider the question of whether or not the objects in the Glozel museum actually came out of the ground at Glozel though archaeological evidence that this is so is obviously a vital link that has to be established before TL dates for the objects can be said to date the site.

First of all, we question Professor Atkinson's interpretation (*Antiquity*, 1975, 85) as implying

four centuries of occupation—presumably on the basis that the plus/minus error on any individual date is unlikely to exceed 200 years. As the authors stress, uncertainty about external dose-rate alone may easily account for a much greater error when, as in the present case, the precise find spot of each sample is not available for inspection. This uncertainty is particularly serious for the Glozel samples because with the inclusion technique the external dose-rate (from radioactive impurities in the soil and rock that lay within 30 cm. of the sample during burial) contributes more than half of the total TL for some objects: the internal (beta) dose-rates are quoted as being in the range 0.2 to 0.36 rads per year and a value of 0.29 rads per year is given as the best estimate for the average external dose-rate—obtained by inserting monitor probes at various locations on the site where the samples are thought to have been buried. Having in mind the uncertainty that shrouds the burial location and the possibility of strong spatial variations in the external dose-rate it would seem that the ± 20 per cent uncertainty implied by the 800-year date range quoted is no smaller than it ought to be.

Ought it to be larger? Ought other origins than La Tène or Gallo-Roman to be considered as the alternative to modern forgery? We think that on the basis of the TL data published in the report (and presented at the 1975 Oxford Archaeometry Symposium) the date limits

are too narrow and that a wider range of origins ought to be under consideration. The immediate grounds for saying this are that of the twelve pairs of glow-curves shown in Fig. 6, there are three (744002, 744004, * 744401) which imply equivalent doses of about 500 rads; using the range of dose-rates quoted the dates obtained for these objects lie within a few centuries of AD 1000. There is one (744205) which implies an equivalent dose of around 2,000 rads and this suggests a date beyond 1000 BC. These four objects can be brought within the date range 700 BC to AD 100 by assuming extreme values for the external dose-rate but, although this *may* have been the case there is no justification for assuming that it was the case; some increase in the calculated age of the three low-dose objects could be obtained by assuming a substantial supralinearity correction but barely enough to reach AD 100. While it is appreciated these glow-curves were presented with the objective of illustrating that the objects could not be modern forgeries it does seem that they contain evidence that conflicts with the hard and fast conclusion that the date for Glozel lies within the range 700 BC to AD 100. It is a pity that the report gives a specific date for only one object (and that rather incidentally, in Fig. 4), particularly in view of the well-accepted radiocarbon dictum that 'one date is no date'. It is an urgent priority to have proper publication of the data: an individual date evaluated for each object together with a precise statement of the technique used and indication of the extent of the known-age checks made to establish validity.

A more general ground for disquiet is that the technique employed does not eliminate reliance on minerals in which the TL is liable to be affected by anomalous fading. A characteristic of this effect is that even for samples from the same site it may vary in importance, affecting some significantly and others not at all; when it is dominant the TL age obtained is much less than the true age, and, unless a sample has passed an experimental test for the absence of the effect, it is prudent to regard the TL date as

* The glow-curves given in Fig. 6 for this object are in such conflict with those given in Figs. 4 and 7 that a labelling error must be presumed.

a *terminus ante quem*. As this malign phenomenon first came to light (Wintle, 1973) in attempts to date recent lava flows from the Chaine des Puys barely 50 km. away, it seems likely that it will be present in the mineral inclusions of some of the Glozel samples. It can be avoided by eliminating all minerals from the sample except quartz (which is not prone to the effect); this requires severe treatment with concentrated hydrofluoric acid. The indication of low ages noted in the preceding paragraph may be due to this effect, but it is also possible that it is affecting all samples to a marked degree; consequently it cannot be accepted that the TL data given in the report rule out the neolithic dating, outrageous on other grounds, originally proposed for the Glozel objects by Morlet and by Reinach.

The important objective that *has* been achieved by the report is to show that an entirely reasonable application of TL to the Glozel objects indicates that they are not modern forgeries. A thorough treatment has been given to the question of whether the firing temperature was high enough to remove the geological TL and there is no longer room to doubt that this was so. Another possibility that has been put forward is that the objects have been exposed to a heavy dose of external (gamma) radiation either by deliberate application from an artificial source or by accidental storage for a long enough period in the uranium mine that is reputed to be in the vicinity. Suitable gamma sources are not easily accessible and the operation would be far from trivial, requiring the close collaboration of a radiation physicist; it would be interesting to know the level of dose-rate in the mine but it is likely that storage for several decades would be necessary. However, 'faked TL' can be detected by measurements on the sample itself: this is by comparing the level of TL carried by mineral inclusions with that carried by the fine grains of the fired clay matrix in which the inclusions are embedded. In one approach zircons are used (Zimmerman, Yuhas, and Meyers, 1974); these inclusions have a high uranium content and for a genuine sample they will show a very much higher archaeological dose than the fine grains, whereas

NOTES AND NEWS

for faked TL there will be no difference. It was reported at the 1975 Archaeometry Symposium that application of this technique to some of the Glouzel objects ruled out faked TL. Another approach is to compare radioactive-free quartz grains of diameter around 0.1 mm. with fine grains (0.01 to 0.001 mm. diameter). In this case the inclusions should show a lower archaeological dose than the fine grains because the former receive insignificant dosage from alpha particles (which have an average range of only 0.025 mm.). We illustrate this below with measurements we have made on a fragment of inscribed tablet that was given by Emile Fradin to Professor E. T. Hall.*

On the fragment concerned (198b1) we found a value of 535 rads for the archaeological dose of the quartz grains and a value of 800 rads for that of the fine grains.† Hence the measured ratio of fine-grain dose to quartz dose is 1.5 ± 0.2 , allowing an uncertainty of ± 10 per cent on each dose. This ratio is sufficiently removed from the value of unity to make it highly unlikely that the TL has been faked (though it is to be noted that the separation in values is less than three standard deviations and allows a finite probability, of a few per cent, that faking has occurred). It must be immediately emphasized, however, that this result is for *one* fragment and it does not necessarily apply to all others; however, a similar conclusion but ruling out faked TL more strongly has been reached by Dr S. J. Fleming from measurements on another fragment (to be published in forthcoming *Archaeometry*). For sample 198b1 we estimate, from measurement of alpha activity and potassium content, that the effective internal dose-rates were 0.4 rads per year for the inclusions and

* To whom we are grateful for encouraging us to make these measurements.

† The natural TL from the quartz grains (which had been etched for 40 minutes in concentrated hydrofluoric acid) consisted of the benign 375°C peak and the change of sensitivity between first-glow and second-glow was less than 10 per cent, the X-axis intercept of the second-glow growth characteristic was 100 rads; because of the restricted amount of quartz available the archaeological dose was derived by direct comparison with the second-glow TL, but otherwise the technique was that of Fleming

1.0 rads per year for the fine grains. Assuming an external dose-rate of 0.29 rads per year gives dates of AD 1200 and AD 1350 respectively. Tests on this sample gave no indication of anomalous fading but because it is a single fragment of undocumented origin we do not think that undue weight should be given to this dating; there is also the possibility that it was refired in the medieval kiln said to be on the site.

Extension of these tests to a wide range of objects is desirable because we must not assume that all objects have the same history, but assuming similar conclusions we are still left with the archaeologists' question: *is it not possible that in this particular instance there is something mysteriously wrong with the TL?* Such an enquiry, however tactfully made, tends to be counter-productive through causing a closing of the TL ranks. But the Glouzel tablets must have a message either for the archaeologists or for the TL dating specialists, and, having been in business for only seven years it behoves the latter to peer anxiously in case the message is for them. Although there is no conceivable reason why the Glouzel TL is wrong it is not impossible that eventually some new unsuspected effect will be identified. An earlier instance is the discovery of anomalous fading mentioned above; in terms of what was understood about TL at the time the results then obtained were inexplicable. Certainly that case is different in that lava dating represented a new application of TL whereas there has been widespread application to pottery fragments. If, eventually, fault is found in the TL, it seems likely that it will be associated with some abnormality special to Glouzel, in the (1970). Pre-dose dating (Fleming, 1973) was also applied to the quartz grains and the results were in agreement. The fine-grain value is the average of four determinations using the Zimmerman (1971) technique, from separate sample preparations, the individual values being 730, 750, 750, and 990 rads including supralinearity corrections of 120, 0, 30, and 0 rads respectively. TL measurements were made 10 days after disc preparation and irradiation in order to minimize possible effects due to anomalous fading; separate storage experiments indicated that any such fading was less than measurement errors (3 per cent) in 2 weeks.

ANTIQUITY

mineralogy of the clay or in the radioactive impurities.

There is a symptom by which unsuspected effects sometimes reveal their presence, whether in TL or any other form of dating—an inexplicable spread in the dates obtained for samples that should be contemporary. Therefore, apart from their archaeological significance, it is of high importance to know whether, after excluding samples affected by anomalous fading, the individual dates concentrate into one or possibly two groups (the second one corresponding to chance re-firing in the medieval kiln) or whether there is a diffuse spread.

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Somerset Levels Papers—Number 1, 1975

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