

Real Archaeology?

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For some reason people do not like arbitrary definitions, or decisions, or interpretations, even when there is full agreement that they are arbitrary. Many students feel uncomfortable proceeding by induction: suppose 'X', then how does 'Y' work out? Bad luck; let us make some arbitrary definitions.

There was the past which cannot be recovered, it has gone. There can be a subject called the study of the past. This has to work through examination of what remains of the past in the present. These remains, for me, belong to two rather different categories, one of which involves explicit communication (words, letters and pictograms), the other which involves implicit communication (material which does not involve words, letters and pictograms). Anyone who wants to put together as complete a study of the past as possible needs to take into account the two types of evidence where they are available.

I can see that some students of the past might want to make a third category in which structures and objects from the past are interpreted as symbols, a form of direct communication. When they have cracked the code and set out the alphabet and the syntax of this symbolic language, then it may join the explicit. Until then, it must be firmly lumped with the implicit. A language which has not been decoded or translated cannot be grouped with languages which have known equivalents in our modern languages.

The two categories of explicit language and material demand very different abilities and methods to wring the most information out of them. I have abilities in material and interests in written evidence; this means that I am a professional in one area and a dabbler in the other. At present, training in archaeology does not include any instruction in the understanding, editing, or criticism of written sources. This is as it should be, because archaeology is the study of the material left from the past, while history is quite clearly the study of the written sources left from the past. Archaeologists may use the results of historians' researches, but they often make a complete dog's breakfast of them, and only some of the more adventurous and open-minded historians seem to be aware of material. It is just possible that a very bright student who followed a four year degree in History and Archaeology might be competent in both disciplines.

The problem is that there are very few real archaeologists about, that is, people who work from material to ideas, their numbers are getting less rather than more, and both teaching and publication are moving away from Real Archaeology. In a way this is strange because the people such as Ian Hodder and John Barrett who have been at the forefront of (comprehensible) theories are coming back, in their published work and their conversations, to value material as a starting point for their ideas. But before material can be put in a form which will spark off ideas, it has to be organised and interrogated. The organisation of material, the basis of archaeology, is becoming much more difficult since a high proportion of the latest published reports do not give the basic material found in the excavation and on which any ideas in the report are based.

This leads to greater reliance on secondary sources, and 'What the Great Think'. I have in mind two recent dissertations, written at different levels and different subjects in different places. One concerned the beginning of towns in Roman Britain, the other,

the end. Both relied heavily on summaries and 'books about Roman towns'. In some cases they went back to interim reports and articles in conference proceedings. But all the time they relied on what people had written about Roman towns. Not surprisingly, they both came up with remarkably uniform pictures of the beginnings and the end of Roman towns. And, equally unsurprisingly, they both found that current ideas were amply borne out by a study of this 'evidence'. Neither should have happened in a Department of Archaeology, or in an archaeological dissertation at any level, because both were studies in contemporary history. Neither said anything at all about what happened in the Roman past, they were both concentrating on what has been said in the very recent past.

The point which particularly worried me, was that neither commented on or investigated how the ideas they were retailing had come to be, and neither seemed to understand why the pictures their surveys created were so uniform. Both put in quite a lot of work, but in my eyes it was totally misdirected.

There may be room for a subject for research covering the nuttiness of recent ideas about the end of towns in Roman Britain, and I would expect at least a paragraph within that to dissect my work in detail. I would accept such a subject on the fringes of archaeology, provided the students understood what they were doing and realised that they were producing case studies in epidemics of changing interpretation. The objective would be to try to understand the mechanics of influence in archaeological interpretation, and the relationship of the study of the past to the events and ideas of the present. Thus it is now a commonplace that the *floruit* and fall of the British Empire has had an effect on British interpretations of the Roman Empire. But this provides yet another problem and misconception. To demonstrate that some British commentators on the Roman empire were affected by their views on the British empire is not to demonstrate how terrible and wrong they were, it is to demonstrate how and why they were influenced. It would not be popular, but it would be highly relevant, to point out that people with a good understanding of one empire would have a better understanding of another empire than theoretical wimps who had never even done National Service. So long as the imperialists understood that they were imperialists. And perhaps the wimps ought to own up to wimpishness. But to return from social studies to archaeology.

The two studies that I mentioned earlier were pleased to show that the subjects that they were reviewing had consistent forms. They seemed to think that they were reviewing the beginning and end of towns in Roman Britain, whereas they were in fact reviewing secondary literature on those subjects. They were consistent because reviews of secondary literature must either be consistent or wrong. If excavators excavate and then publish only interim reports, they do not allow the possibility of other interpretations. Secondary comments have either to take what the excavators said, or, without any basis in fact, invent. If they invent, the chances are that they will be wrong.

Thus an interim report may say, 'The walls of the basilica were built soon after the fort ditch was filled in around AD 70, and since the fill was not consolidated, the walls sank into the ditch. It is likely that this happened in the period AD 75 to 95'. All secondary sources have to follow that dating; most of them have no reason not to. PLEASE NOTE that those who asked to see the contents of the ditch so that they could make their own judgements would immediately cease to be secondary sources and become primary sources. But the excavators' comments making the great building date to AD 75 to 95 are very heavily influenced by other excavations, by an inscription mentioning the years around AD 80, and by a historical source which says that a governor of Britain in the 80s

encouraged just such building. This is not real archaeology it is an unsavoury, heated up, stew.

What would the Real Archaeology alternative be? It would have to collect the material basis for any study. The sites and buildings involved would have to be chosen, and then all the coins and the datable pottery in layers pre-dating those buildings would have to be assembled. The material gathered would have to be listed so that future work could agree or disagree, and then the homogeneity of the datable material could be assessed.

There is a further complication here because the material of similar dates from widely spaced sites might not be of the same categories. And even if the material belonged to the same categories, it might not have been uniformly distributed through space, or perhaps even time. Thus a site on the east of the country may have been supplied from the East and a site on the west from the West, and the two sets of supplies may not either have been similar, or even in step. And some consumers may have been given priority, and received the latest fashions when the first ship sailed after the winter storms, and others may have been sinks for unwanted stock. So the second category only got their supplies after the five year clearance sales when the warehouses were spring-cleaned.

There is a study of this type by Martin Millett, in fact it was his D Phil thesis. He took deposits of red gloss Samian pottery in Britain from levels thought to result from Queen Boudicca's rampage, and tried to assess their similarity. To me, this was an absolutely fundamental study, yet the parts that have been published have received very little comment, if any. The point at issue is not to moan about the obtuseness and philistinism of the general pseudo-archaeological population, but to emphasise the enormous amount of work involved in Real Archaeology. That was three year's of research, and still the basic question was not answered. It needs more work. We do not yet know whether the deposits we take to be dated to AD 60-61 are a) consistent, b) really likely to date to AD 60-61.

Well, at least once the excavation has been worked up for publication, the material is set out for us in print so that we can do this type of study in the future if ever a breed of Real Archaeologists is produced. Oh, really?

Look at the majority of recent archaeological reports. The best are just as old fashioned as ever, they bore you to death with the publication of information and the excavators - or rather writers, since excavators rarely now write up their own digs - were obviously so worn out after getting all the material in order that they had no energy left to discuss it. The worst tell you everything they ever thought about the site and illustrate a few of the nice objects. You would be far better off reading a well written account of the excavations published for the general public, which at least gives one thinking person's account of the site. You cannot disagree, because readable accounts cannot include the sweepings of the dig hut, but at least you can get a thoughtful flavour of what it was all about.

Now now, that is being cantankerous. You know perfectly well that even if some 'reports' are a bit thin ...

Huh, that's rich.

Shut up and listen.

... the published versions might leave something to be desired, but there is always the material in store and an archive report to help you find your way through it.

I have made nasty remarks about archives due to be published shortly (in a series

of essays on London, where better?), so I will not repeat myself here. I will just relate the story of the MA student who came to the Institute after years as a site supervisor on some large digs who rightly decided that archives were underused. She planned her dissertation around an area which had been well dug and which seemed likely to produce some interesting results. If anyone could make it work, she had a good chance. Her dissertation was very interesting - on Nineteenth Century Antiquaries if I remember rightly.

But there is the material safely stored away awaiting study. Stored? Safely? Certainly awaiting study, but how can anyone get access if there is no-one in charge of it; no-one who knows their way around it; no-one who knows what is there and what its potential is? If a lot of money has been spent on excavating material, then it seems less than good practice to consign it to uncatalogued, unvisitable oblivion. If publication is seldom full and informative, and complicated or badly made archives are almost unintelligible, and curators of excavated material, where extant, are threatened with elimination, then (deleted expletives) why dig the stuff up in the first place?

What the hell, we have not got time for these fiddling little details, let's get on with the interpretation. Ah. So what is this interpretation, and what is its purpose?

It is the picture of the past that will be produced when these people calling themselves real archaeologists have died off. The consumer, the general public, will never take in these abstruse details about the homogeneity of Boudiccan Samian. The pots come from an early burned level; that is good enough, let's go on from there and do something interesting with the information. All this material navel gazing is what gets academics a bad name. Do something with it. Say interesting things. Communicate with the public.

Hmmm. So it does not matter if the facts are not exactly right?

In that detail, no.

So why not bring back my favourite essay question which amused a generation of first year undergraduates? 'Helen of Troy grills dinosaur steak for tired caveman after midsummer orgy at Stonehenge. The public should be given the past they want; discuss.'

It's sexist for a start.

Agreed, it supposes that Helen tired out the caveman while I presume you expect it to be the other way round.

But it's got so many mistakes in it.

But they are only little ones. It is all so long ago. Dinosaurs and Stonehenge are far beyond our comprehension, thousands of years. You may say you can differentiate between 3500 years ago and 350 million years ago, but I wouldn't believe you. You are just passing on what you have been told. It does not come from your own experience, it can't. Let it go.

But some things are always found on top, and others underneath, and that makes rules which we need to obey.

Are we being sexist again?

No. I mean dinosaurs always come from under Stonehenges, never on top. They are earlier; every time they have ever been discovered. It's something which really is. Something that really means something.

Oh, Real Archaeology, you mean?