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THEORY AND ROMAN ARCHAEOLOGY

Richard Reece

The paper seeks to find the proper, or at least current, role of theory in Roman archaeology. It sets up a project to study the settlement pattern of Roman Britain from purely material sources and tries to investigate the presence or need for theory in each of the successive steps of the project. A division is found between the gathering of observations and their manipulation, which can be relatively explicit, and the interpretation of the observations which has to have an element of theory and subjectivity. Three basic questions arise from this title. Which theories? To which areas of Roman archaeology should they be applied? What is the purpose of such application?

Two simple, and polarised, objections against any such activities are clear: (a) we know what happened, we don't need theory; and (b) we do not yet have a good enough data base to justify the application of theory.

Perhaps we ought first to sort out some objectives and so find a possible archaeological problem which could act as an example. If our aim is to describe a dinner-party-orgy as it might have taken place in South Italy around the middle of the first century AD – a worthy and interesting aim – we should apply to the *Satyricon* of Petronius rather than an archaeological data base. If our aim is to analyse the changes in settlement pattern in Britain between AD 1 and 500 then we might be on the right archaeological track. I would go so far as to say that the settlement question could only exist in the context of information on material, that it could not have occurred without a sample of basic material. In that sense it is a 'material' subject and can only be answered from material evidence. We are in the business of archaeology. How would an archaeological answer to the question be found?

Taking nothing for granted we might be able to define a type fossil for the period in question, a general Roman type fossil as a theme for those

five hundred years. If any recourse to history is allowed then Pompei might be a good place to look for a specifically Roman type fossil, because there you have a site sealed at a historically known date. If historical sources are not allowed then tree-ring dating must be pressed into use. Rings must be counted back from AD 1990 to AD 1, and a site with a sealed deposit of that sort of date examined. Perhaps the London water-fronts will do if an error of a decade or so is allowable. I do not see for the moment any theoretical help for either tree-ring-dated deposits, or possible theoretical refinements in the date of the destruction of Pompei. Let us, for the sake of argument, take Samian pottery as our type fossil, our Roman Type Fossil, or RTF; I like to use the S sometimes, and only a sickly pedant would forbid it.

A map of finds of Samian would be a first step towards the settlement pattern of Roman Britain. Given extensive field-walking and field-survey the map could have quite a lot of dots on it. It would of course not be a map of settlements in Britain 1-500, but a map of find-spots of Samian pottery. Perhaps this is where theory comes in?

If so, our two polar objections come in as well.

(a) This map is clearly so far from the real state of Roman things that no injection of theory could improve on what we already *know* from other sources. Viz. Britain to the 18th century was agricultural, and the distribution of settlement mirrored the distribution of cultivable land. Work therefore on modern land-quality maps, and distribute on the maps random dots in proportion to the usability of the land, and you will have a far better, if generalised, map of settlement in Roman Britain than from any other source. Set your parameters so that the number of dots is greater than the number of Samian dots, and perhaps give four or five ascending numbers of sites. Then moderate your possibilities by reference to one or two examples of highly detailed field-work. The land will give the generalised distribution; the moderating field-work will give the level at which the sites are to be scattered.

(b) The Samian map is such obvious nonsense that to apply any theory to it would simply compound the folly. We are clearly not yet ready for theory.

Or am I already indulging fairly heavily in theory? If so I find the word and the concept a nonsense, for all I have tried to do so far is to think in stringently careful and analytical terms.

Can we agree to summarise so far? I doubt it, but I might as well try. The first purpose of archaeology is to define material accurately, to know which

material is where, and when. This gives three main parts to the basic primary functions of archaeology: definition of material, description of spatial spread, and description of chronological context. The second purpose may be to interpret this evidence gathered.

Before we try to go on theoretically let us try to improve our map. We must improve our RTFs. A more common fabric than Samian pottery, but sometimes in the same contexts, is Black Burnished Ware category 1, or BB1, and its copies, BB2. We define it as a second RTF by its common association with Samian, but we may note that it often occurs without Samian. We can therefore plot new maps of Samian, with or without BB1. When we take a closer look at BB1 and take into account form as well as fabric it becomes clear that certain forms occur with Samian in primary contexts, and other forms never occur in those primary contexts but in others, higher up the stratigraphic record, where Samian, if it occurs at all, may be re-cycled. We therefore get Samian phase BB1 dots and post-Samian phase BB1 dots. From constant observation of super-position of deposits we conclude that this involves early BB1 and later BB1. We now have period maps. Some dots on the map can be associated with marks such as soil-marks, plough-marks, aerial photographs or even humps and bumps. Some of the sherds gathered from field spreads can be associated with assemblages more or less described from sites more or less excavated.

So spreads can be measured and dated. Maps can reflect the dimensions and dates of scatters. Is theory likely to elbow its way back in?

Objection (a) will now have changed to

(a2) We know the date and distribution of field scatters; we don't need theory.

This objection seems set to change and keep on cropping up as your information is improved. It will not go away when the first set of problems are overcome. It may well turn up in a different guise as follows.

(a3) If your sophisticated maps of distributions of RTF scatters is seriously, detectably, even 'significantly', different from what we *know* (i.e. from agricultural land) then it is clearly nonsense and no amount of theory will help it. I don't believe it.

(b2) The data base is as good as it will ever be; why subject it to theory?

We begin to approach a possible area of agreement. In order to have a good idea of how the past still exists in the present we need to know:

- i) how the material is defined;
- ii) where it is in space;
- iii) when it was in time.

Our objectors will go on from there, in unison: and this is the nearest we can ever come to what was, so why add theory?

Because, we answer, of the next stage. We asked questions not about scatters of RTFs but about settlement patterns. We have to turn scatters into settlements. In short we have to interpret. But before we go on to that stage we need to have a closer look at some of the things we have done to see whether theory has crept in unnoticed.

We move on now to the Definition of Material. Some ardent theorists will insist that the material is already theory laden, and that to define it further only adds to the theory. I see the force of argument for a flint 'arrowhead' or a bronze 'razor', where an attribution of use has been foisted on a piece of material in order to define it, when that object is part of an indivisible continuum. The dividing line between 'spear-shaped arrowheads' and 'arrowhead-shaped spears' may either be totally arbitrary or non-existent. In the Roman period I doubt whether this is a major problem if we stick to material objects. And it is part of my thesis that most material of the Roman period in Britain can be described, and even defined, in modern terms, without necessarily introducing interpretation.

For about thirty years excavators have sent me their Roman coins. They want as many scraps of bronze to be coins as possible because far more information can be obtained from coins than from scraps of bronze. After the first batch have come from a large excavation a second batch, much smaller, usually follows of further hopeful scraps which have emerged from cleaning. I therefore tend to see too many scraps and have to reject a few. But the number of bronze scraps which I have to label 'not a coin' are few, less than 1 in 1000, and the number of scraps about which I am not sure is even less than that. For the objects I accept as coins there are places in voluminous *corpora*. Note that I do not say that every 'coin' is described in a *corpus* somewhere; this is because if you understand the material you can find the place in a well constructed system in which your previously undescribed coin belongs. Since the indeterminate scraps to which I can attach no classification are so few, less than 1 in 10,000, I cannot take seriously any claim that there is a serious and important doubt about what is a Roman coin. I therefore take the simple descriptive definition 'Roman coin' to be relatively theory free. If anyone were to object that to define something as a coin involved countless theories of economics, market forces, and politics, they would have completely missed one of my main theme-songs, that we do not know what 'Roman coins' were to the Romans, nor have we any idea, *a priori*, what they were used for. All that is

theory, and none of it follows from the descriptive definition 'coin'.

Or Samian, which is the result of a highly centralised, organised and standardised production. The number of borderline sherds – is it, or is it not Samian – is small in anything other than a viciously acid soil. Snap decisions probably affect the total archaeological record very little, and if they do matter they could be decided chemically.

Or BB1, the result of highly standardised but loosely organised production. There is little or no doubt about fabric, and that could be resolved petrologically. There is strong agreement on the general forms. That is a BB1 pie-dish, this is a BB1 cooking pot.

The word Samian obviously carries no hints of the island of Samos, big or small s, pie-dishes suggest nothing about pies, nor cooking pots cooking. But BB1 is black and burnished.

Once the material has been well defined then it either has been found at National Grid co-ordinates WXYZ or it has not. And if there is doubt, it has not. It was found at that point in the stratigraphic sequence or this, and if there is doubt choose the later. It was in context a or b and if uncertain it was unstratified. It commonly occurs in upper stratigraphic groups, or lower ones, or both.

All this, our data base, has been arrived at without any obvious application of explicit theory, and quite probably without direct involvement of specific theory. Applications have involved certain principles which are thought to be important in almost any endeavour. Any definition ought to be clearly comprehensible, ought to avoid uncertainty, and ought to be universally applicable. If possible it should be susceptible to testing by physical or chemical means. If either definition or test involves theory the theories are likely to be diverse rather than convergent, so any support of definition by test will probably be in spite of the theories rather than due to the theories. Add to definitions precise observations of what was found where, and in what combination with other objects, and I think you have the basis of the information.

If people wish to niggle they may. But if they wish their niggles to be taken seriously they must show how the application of the niggle would change the data base created by more than one per cent or keep quiet.

It is possible that potential critics will have followed me this far, just out of bored curiosity, in the sure and certain knowledge that I shall collapse in an untheoretical heap in the next stage – if I attempt it. For, to a number of Roman archaeologists, the next stage must be interpretation. If I want to pursue my original goal of changes in settlement pattern then I probably do

have to interpret. My tendency would be to stop at the data base and say 'The settlement pattern is represented by the spreads of material mapped. Changes over time can be seen on the phase maps provided.' In fact I shall briefly stop there, but I will go on after to see where interpretation leads.

I could say that scatters of BB1 and Samian have been measured (5 yards across and 20 yards long) and have been assigned to either an early group, a late group, or a general group. All scatters of more than 50 yards across are early or indeterminate; no late scatters are 50 yards across. Of course no one would quote this, or perhaps even take it in. They would interpret it, and Reece would have shown that large villas were exclusively an early phenomenon. I would rather say that above ground rubbish heaps which plough out to large scatters were the early form of rubbish disposal, and rubbish pits, which plough out, if at all, to small scatters, were the late form. But the villa interpretation is much more interesting.

So how do we interpret? By what theories can interpretations be constructed, compared, proved or disproved? What is the interpretative theory which transforms what may be relatively objective categories or Roman material into stories? What is done with the material and what ought to be done with it?

To embroider material into interpretation we must have some concepts in our minds, some ideas of parallels ready to apply, and we must have some reasons for sticking the concepts to the material. You have to have the idea of a threshing floor ready to hand in order to apply it to a layer of worn stones set in a particular way. When it is pointed out to you that the stones are set in, and covered with, water-borne silt you may have to give up your interpretation in favour of a paved river ford. You must search your mind, or the minds of colleagues, or the library, for analogies, but give them up if they conflict with the material evidence.

We interpret by analogy. Some people are horrified at this, and want to believe that archaeology can be more exact a science than this. They would do well to heed Prof. Mary Hesse's warning (given in seminars, but not yet, so far as I know, published) that by doing this archaeology is working alongside the 'hard' sciences. Analogy is not a poor relation, but, according to a respectable professor of the History and Philosophy of Science at Cambridge, what scientists have often done and do now.

The next question concerns the way in which analogies are chosen, and then, how they are judged. The cynical and destructive, if truthful, answer is that they are neither explicitly or even knowingly chosen and that only the unpopular ones are judged.

Many Roman villas were dug up on estates. Many estates produced only one villa, and, almost by definition, estates have one comfortable if not stately home. There were clearly too few villas known to accommodate more than a small part of the Roman population of Romano-Britons, and they were too well appointed compared with other houses of the Roman period to be for the poor. It was therefore obvious that the villa was the home of the Roman estate owner just as the comfortable or stately home was the house of the modern estate owner. Other facts followed, such as occupation of the house – *the* house, singular – by a single family, with dependants if necessary to fill up the rooms, and a fence round a nice discrete property to give a comfortable few hundred acres at least. And the majority of Romanists are too blind to distinguish this plausible and possible fiction from fact. That it has never been explained in a *locus classicus* shows, not that it is a heap of rubbish, but that it is fact, for fact does not need to be explained, only to be quoted and used.

Hence we arrive at a definition of theory in archaeology. Theories are unsuccessful ways of arguing against facts. Facts are what are set out in the text-books of Roman Britain. Therefore anything which goes against the text-books is merely theory. This is neat, but it does hold a problem now that Frere's *Britannia* has imitators and dubious offspring. So as not to be misunderstood I had better say that I regard all sub-*Britannia* offspring, that is all books modelled on *Britannia*, as dubious, if I am polite. *Britannia* by itself was internally consistent, and I admire it for that, while not agreeing with it. Imitators have jiggered with bits of it and the results contain argumentative holes through which objectionable carts and horses can be driven. With *Britannia* you either accept it whole and its world to go with it, or you do it yourself. With various Roman Britains about, judgements need to be made, and that can only be done with the help of theories. So, in a sense, sub-traditional Roman Britains have fostered the need for a theory of interpretative judgement. Nutty Roman Britains just make things worse, or better, depending whether you want to bring interpretation out of the closet or not.

So far have our standards of common decency declined that there are now books on Roman Britain which show explicit interpretation. Richard Hingley (*Rural Settlement in Roman Britain*, 1989) had the nerve to take a Roman villa plan, and populate it as if it were a collection of savages' huts. North American savages, I think, at that. Which clearly makes it wrong, since Rome was the colonial power, so villas ought to be interpreted by analogy with weather-boarded, white-washed, colonial mansions. Which of

course helps us to understand the role of slaves.

So, to leave polemic for a little, we interpret by analogy. This has all the known problems, in particular the impossibility of interpreting a pottery kiln as a knuft-herping site since no one knows what knuft-herpers needed for their job. Remains need to be consistent with the firing of pottery, as known at present, or observed in primitive contexts, or read from early manuscripts, or dictated by technological considerations, reaching at least 500 degrees C and staying there for some hours, in order to be interpreted as pottery kilns. It may be that they also need to be consistent with human beings. It is no good suggesting an interpretation which simply will not work for human beings as we judge for ourselves. Here we may be seeing the entrance not of the interpretative theory, but the theoretical framework. It is too loose a method to leave each person with his or her individual perceptions and peculiarities to judge the humanity of each interpretation, we need a human scheme, a human theoretical framework, a common frame of reference, within which to interpret. Several of these frameworks exist, and we can both ease our own task, and that of our readers, by explicitly siting our interpretations in a chosen model of the world. It may be the Marxist model, or the market economy model, or the religious (flavour to taste) model. It is not so much importing a load of theory into our interpretation as doing our interpretation within a framework of theory.

This proliferation of theory worries me. We have theory level 1: interpreting by analogy, and all the needs of method, testing and evaluating. We have theory level 2: the theoretical framework in which we see human beings operating. We have theory level 3: the theory to which we apply the two lower levels, the theoretical construct which is the Roman Empire.

How do these levels relate in practice? To what extent are they explicit? And to what extent are they controllable?

The Roman Empire is based partly in historical references and partly in the pious hopes that material has been well gathered and used as a commentary. As it stands it is quite immune from valid theoretical niggling unless the niggers were to start afresh and build their own Empire. Even that would be of questionable value since it would be unique to them. We have to live with the construct as it is, recognise that it has no more reality than the physicists' constantly changing pictures of the atom, and try to knock some sense into it whenever we get the chance.

If we come down to the level of a province, of Roman Britain, then there may be more hope. But we only have control over the theoretical construct that is Roman Britain if we go back every time that we do anything to the

actual material. If we take secondary sources then we are incorporating other people's theories into our theory as if they were material facts. Thus we may use a secondary summary, like John Wacher's *The Towns of Roman Britain*, 1974, provided we do no more than use it as a guide to the original sources. We must draw a very hard line between talking about material and talking about what people thought about that material. This almost restricts us to bibliographies, for any connected text in the chapters of such books is interpretation, and so is laced with personal theory. Such text is only usable for a study of 'twentieth century views of Roman Britain', it says nothing at all about Roman Britain.

This suggests that we can get at some aspects of Roman Britain if we go back to our sources, but we have to push further. Wacher on towns may guide us back to an original excavation report. Here we must be extra vigilant. We may trust a drawn section, though work at Stanwick shows how Wheeler could bend his sections to fit in with his historical preconceptions. We can probably trust explicitly described material since few reports describe material that was *not* found on the site, but found somewhere else. Yet context is vital. It would be true for a report to say that Ptolemaic coins had been found at Winchester; but it would be a *suggestio falsi* unless it added that they were found on a spoil-heap and not in excavation. If the context is given then it is clear that they have rather less validity than the coins in grand-father's box which he always said came from his allotment. Where material is well described, this is good. Where it is not described it is not necessarily proof of absence, for every printed and even archive report is a selection of what was actually found.

At this point I need a parable, and the only one that springs to mind involves the world of the word-processor. I hope it will not put off those who are not yet properly programmed. A conference organiser decides to publish the proceedings of the conference. Contributors are asked to supply their text on a word-processing disc. The discs come in and the editors, not realising there are any difficulties, but thinking that all word-processing is the same, send the discs off to a little publishing company which has otherwise intelligent customers, and they manage to plug all the discs into their system and print the results. The firm are surprised, but the organiser did say it was a very abstruse conference. The editors are horrified, for most of the papers are blocks of signs and letters and spaces which make no sense at all. The company comes to their help and explains that it all depends on the framework within which each contributor worked, an Amstrad here, an IBM there and a Mac somewhere else. And even when

you have found out what general theoretical framework each contributor was working in, you still have to find out the individual programme, Word, Word Perfect, Wordstar, Macwrite, that each was using to fit the basic information of words and phrases together, to interpret the material.

Our Roman Britain is just such a jumbled conference proceedings; the trouble is that we think we can read it, and we never notice that each person is working in an individual theoretical framework. We may sort out the Mac user, the Marxist, from the IBM user, the liberal – I steer clear of equating any computer with Thatcherism; it would be an actionable slur. But we are not even aware that there are different programmes, different ways of interpreting, and we read the results as if they all fitted together.

To try to sort things out we might have conversations more often in the following vein. The scene is set by a non-Marxist arguing with a Marxist:

NonM: I could never work in a Marxist framework, I need freedom.

M: So you say that outside Marxism, and other like isms, there is total freedom?

NonM: I do.

M: Which framework do you work in then?

NonM: I don't.

M: I know you to be a Capitalist Imperialist Swine; I therefore assume that you work in a CIS framework

NonM: That is rubbish; I work in post-Thatcherite, success oriented, Britain which is free from all theoretical constraints.

M: Thank you.

In other words, if they are needed, you cannot avoid a framework within which to pursue archaeology. You, just by being you, have taken in so many things that anything you do to material is heavily theory laden the moment you begin to interpret it, and perhaps long before. Your framework may not be an identifiable, taggable framework, it might be all your own. As a person living a full life, which is different from all other lives, this may well be a very good thing; as an archaeologist helping to build a picture of what went on in the Roman Empire it is disastrous. Disastrous because no-one else knows exactly why you gathered this material in this way and said that about it. And of course the trouble is that many archaeologists are people at the same time.

Is there a recipe for a better future? It sounds suspiciously as if everyone must write Their Roman Britain, and all will be revealed.