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THEORY, PRACTICE, AND RESEARCH IN AN URBAN UNIT: A PERSONAL PERSPECTIVE

Michael J. Jones

What I offer here is no theoretical essay, rather a comment from the perspective of an urban unit on ways in which we might reconcile theory with practice, and historical approaches with theoretical. There are other, related divergences in the profession which are also causing concern (cf. Cunliffe 1990, on the widening gap between practitioners and academics). I shall have cause to consider two principal issues – the value of theory to the research work of a 'rescue unit', and the apparently wide discrepancy between established Roman (and classical) approaches and those involving explicit theory.

It is now respectable to be explicit too about our different subjective standpoints. I speak partly as a unit director who, although largely concerned on a day to day basis with practical matters, is acutely aware of the need to ensure good research value in what we do; but also someone who, although trained initially as a classicist, has always been attracted by theoretical approaches. I am not the only person in this or other fields who recognises both the faults and the value of the newer ideas (Graff 1992). Being on a generational as well as a professional cusp, I suppose I just about belong to the (then) younger generation of Romano-British archaeologists identified in his quasi-sociological paper by Rick Jones (1987a), certainly to what Jeffrey May (1991) has termed the 'somewhat anarchic generation' impressed by the ideas ferment of the late 1960s. Managing a unit through several different funding eras certainly enhances the ageing process – no wonder so few original directors are in post. But like many of the older generation, I had a classical education, on which little or no impact was made by the radical ideas of social scientists, before moving on to Roman archaeology.

As I made this change, archaeology too was visibly changing in Britain: some geographical models were being borrowed (e.g. Johnston 1984), but the development was most notably inspired by David Clarke's *Analytical Archaeology* (1969). In this period of burgeoning theoretical ideas, a vintage crop of Cambridge 'Arch. and Anth.' graduates (Daniel 1986: 201), had found university posts and helped to develop several Single Honours Archaeology schools. Their graduates and scions in turn have constituted a whole army of recruits to the cause (see Champion 1992 for an analysis of the intellectual history of this development).

These Single Honours Archaeology students, while acquiring valuable knowledge and skills, were mostly spared the pain, not to mention the discipline, of rigorous study of the classical languages, where every letter counts. Not that they lacked for discipline, but it was a more balanced training. In contrast, the pedantry of the classicist can encourage dismissal of brilliant new hypotheses if one or two details are inaccurate. Besides, for many classical students, theorising was hardly encouraged.

Many recruits still enter archaeology through other routes. As a result, the archaeology profession, now numbering several thousand in Britain, contains a wide spectrum of individuals with various strengths and weaknesses. With some notable exceptions, classicists and social theorists are as alike as chalk and cheese. Anthony Snodgrass, who has, like Colin Renfrew, been writing eloquently on the application of theoretical approaches to classical archaeology for a decade (e.g., 1985) has put his finger on part of the problem (1987: 10–11): classical scholars tend to be the sort of people who, like pure scientists, seek a 'right answer': the so-called 'convergent' mind. Yet much of classical archaeology was formerly confined to art-history, and there was plenty of scope for theory here (e.g. Carpenter 1960; Hutchinson 1962 on visual perception in Minoan Crete, and recent ideas on iconography; see also Finley 1985: 18–19). Moreover, its long history as a discipline means that its practitioners are not easily impressed by new ideas and this has accordingly enabled it to examine and subsequently jettison certain theories (Boardman 1989; Dyson 1989), while embracing numeracy, taxonomy, and some social theory (e.g. Morris 1992; Alcock 1993).

Two other factors may, however, make it less penetrable by the uninitiated: the need to undertake detailed, often repetitively tedious (and these days quite unpopular), tasks in order to grasp the subject matter, and the fact that it is undertaken on an international (though particularly European) scale so that new hypotheses require international recognition to be fully accepted. I return to the further implication of the first point later. The significance of the second is that approaches to the classical world are, in several other countries, even more traditional than the British (see Todd 1992), although there are signs of change here too. The reasons are various (see Hodder (ed.) 1992; Harke 1992; Cleuziou et al. 1992), though perhaps more easy to understand in Germany than in France. For all the impressive array of France's '*philosophes*', and the mark left by the historical tradition of Bloch and Braudel, the value of applying social theory seems to be largely dismissed by the leading urban archaeologists.

Some comment is appropriate on historical evidence and current historical approaches. The comparatively generous provision of historical documentation – e.g. for the early medieval period – may have influenced French approaches, but we know of the limitations of documentary evidence. Yet ancient history itself has moved a long way from its former concern with political events seen from the point of view of the prevailing elite. A commonly stated view of the fascination of the Roman period, and particularly Roman Britain, was the interplay between the historical and archaeological sources (e.g. Hartley 1966), also recognising the tendency of hypotheses to become 'facts' or 'factoids' (Millett 1990: preface). We are no longer so content to serve as history's handmaiden (e.g. Reece 1993). Snodgrass, for one (1987: 47–51), has identified the often irreconcilable nature of the two types of evidence for what happened in Roman Britain as well as in Ancient Greece (see also Branigan 1989). Medieval archaeology meanwhile has been undergoing similar upheavals (Champion 1992: 146–47 for a recent summary; Rahtz 1981), but its value for understanding settlement layout and social and economic life is increasingly appreciated by historians.

To sum up so far, there are great discrepancies between classical and theoretical approaches, but there are clear signs that some earlier notions are being abandoned and ideas of proven value are being applied.

My own experience of postgraduate research convinced me of both the value and the limitations of a theoretical framework. In 1970 no-one, as far as I was aware, had attempted to collate all the evidence for Roman fortifications into an 'assemblage of (structural) artefacts', to study them quantitatively and draw inferences and patterns using scatter diagrams. When the work was published (Jones 1975), its audience was more concerned with its value as a compendium of the evidence than the type of approach; Roman forts were not, in any case, an area of study to which the leading (Cambridge) theoreticians were applying their minds. Some established scholars also felt that it lacked, as I was only too aware, an insight into the mind of the Roman military command, and I was encouraged to study important campaigns up to fairly recent times; was this also theory? The warmest reception to the methodological approach came in fact from George Jobey, a prehistorian. Research in this field has continued apace, with radical shifts taking place in perceptions of both the contribution of the patricians to the Roman effort – e.g. the debunking of Agricola (Hanson 1987) – and a greater appreciation of the Roman-native relationship (e.g. Breeze 1982; Rick Jones' current project at Newstead). Considerable advances in understanding have of course been achieved by large-scale excavations of complete forts and other research projects, but few 'big ideas' have come along: one of them being Edward Luttwak's stimulating thoughts on 'Grand Strategy' (1976). Like most big ideas, it represented a leap forward and a great intellectual stimulus, but also contained much which is no longer accepted (Hanson 1989).

There is another significant influence on the direction of postgraduate research, then and subsequently: the views of one's supervisors and patrons and the need to rely on a limited number of influential persons for endorsement. I am not saying that I personally was constrained in what I felt able to achieve, but it is true that at that stage we all need one or two sympathetic champions if we are to find a job (as we do subsequently when we apply for research grants). One can only go so far from the well-worn path of one's patrons at this early stage: later, there is more freedom. This must once have been the case too for the current elite of Romano-British studies, a generation strongly influenced by the genius of Eric Birley and Ian Richmond, and not such a homogeneous bunch as might be suspected from outside (cf. Scott 1990, on the tendency for traditionalists to regard theoreticians as all in the same boat; Bradley 1990). Younger academics will probably accept that scholarship in terms of familiarity with the historical sources and basic dating materials was of a higher order than is apparent now, but of course the exponential increase in data has led to increasing specialism. With the spread of chairs in archaeology Britain became gradually covered with as many different scholars and schools as Iron Age tribes, and, not surprisingly, some of them displayed (friendly) rivalries and expected loyalty from those they considered to be their clients. A contemporary of that generation, Philip Rahtz, has frankly admitted that one of the factors that so attracted him to archaeology was the reflected glory of exciting discoveries, in the tradition of past excavators (1985: 18). With the present emphasis on preservation, the 'kudos game' has largely shifted to the new theoreticians displaying their latest wares at TAG Conferences.

Before I had time to establish my personal position in the national research scene, and to join my peer academic group, I left it largely behind, or more accurately I was myself left behind, when I moved into rescue archaeology at Lincoln in the frenetic days of the early 1970s. With considerable energy, but inadequate resources and moderate (but improving) technical standards, much was recorded before destruction. We were, I suppose, guilty of what has been called the 'positivist fallacy' (Snodgrass 1987: 62) – assuming that what is observable is what is important and can indicate the principal historical features of the site. Our research framework, as in much urban archaeology in Britain and abroad, was guided largely by Martin Biddle's work at Winchester as much as any period-specific problems. Even this approach was too 'modern' for some scholars: e.g. F. H. Thompson (1975: 253) refers to the 'nebulous concept' of urban archaeology. Much that emerged in the 1970s and 1980s is still being digested, as it is analysed and subjected to the latest theories of, say, site formation processes, or quantititave finds analysis.

In the meantime the insecurity of the unit's existence meant that our chief priority at times has been survival, to preserve an organisation with local expertise in the city – a model that is generally accepted as being in the best interests of urban archaeology (Galinie, forthcoming). We appear to have come through the worst, but more than ever have to keep on our toes: a Unit needs political, professional and academic credibility. Techniques have to be continually reviewed and improved where possible, and here the Units have to some extent taken the initiative (e.g. in recording and analysing stratigraphy). Management of projects takes up an increasing amount of time, and theories are constantly changing. We have had to sharpen our image with the developer, the planner, and the public, produce reports appropriate to our markets, and generally be accepted as a 'good thing': educational displays help here if they hit the right note, and of course are an investment for the future in creating a favourable climate for archaeological research.

We suffer in archaeology from what one might term the 'remarkable precedent syndrome': if one Professor in the south of England can get his excavation reports published within three years or if an organisation in the north makes profits out of a tourist scheme, those who hold the purse strings assume that we can all follow suit. Perhaps the greatest challenge to the modern unit is maintaining our greatest resource, a substantial group of energetic professionals who have acquired both technical and local expertise – and the latter takes some while to develop. Naturally most staff are, sooner or later, concerned about job security and so there is a need to ensure a smooth workflow, an art in itself. There is some alarm in the profession that, while it is not difficult to persuade students to undertake small dissertations on various types of material, post-graduate research tends to be directed largely to 'theoretically exciting' problems, while we may soon suffer from a depletion of resources in terms of, say, pottery experts (see also Monaghan, above), and find it difficult to get anyone to do a thesis on a body of historical evidence. Fortunately, some of these problems have been recognised, but action is needed soon (Fulford and Huddleston 1991, reviewed by Greene 1992). I have been surprised how few universities have followed the example of Leicester in establishing courses in post-excavation studies.

Whatever the obstacles, it is in our interest not only to produce publications, but to ensure that our reports are written in an awareness of current research problems, and further to devise new projects which are of solid research value. There may be some new large-scale field projects in the future, but in the context of the preservation ethic, and in a recession, these could be few and far between. At least there are now signs that the conservation role which archaeology is playing in society is not considered to be sufficient without a modicum of research. The planning system can be used to carry out research to aid decision-making, but on a limited scale. In preparing reports, therefore, we must take account of the fact that we may not be able to increase substantially either our database or our knowledge of particular sites and problems within the foreseeable future. Yet there is a great deal of data to hand which can be manipulated more easily than ever before thanks to computers.

For many years, I have struggled to keep pace with the output of work on Roman and later towns. What those of us in the Units need to know is: how can results from our sites illuminate problem areas or test current ideas, for instance, in Lincoln on Roman legionary fortresses and towns. Closer links with universities are one way of keeping in touch. The Roman Society provided some advice several years ago (Wacher (ed.), 1985), and some of its ideas were incorporated into a wider survey of urban archaeology produced by the Council for British Archaeology (Jones and Wacher, 1987). The CBA's Urban Research Committee continues to offer useful support with its study of themes such as victualling and innovation. Among the Roman Society's recommendations for future work, several priorities were of direct application to Lincoln; the fortress plan, any site with well-preserved deposits in a major Roman town, complete examples of domestic and industrial dwellings, suburbs and the urban fringes, the territorium, harbours and waterfronts, and cemeteries, especially those of the later Roman/early Christian period. There is obviously a great deal still worth investigating according to the collective wisdom of the Romano-British 'establishment' (see also Todd 1989). The significance of transitional phases – e.g., the late Roman and early Saxon periods – was recognised and was subsequently prominent in English Heritage's research framework 'Exploring Our Past' (1991). Here, the Roman period did not otherwise feature strongly except as an element in diachronic approaches to larger problems, such as 'towns' and 'landscapes'. Justifiably perhaps, greater emphasis was placed on the native perspective.

It is of course quite appropriate for the archaeological study of the town to follow a multi-period approach: this is also the case in France, where 'la ville' forms research project number one for 'historic' archaeology (hence 'programme H1': Min. Culture 1990). But there is also scope in the French scheme for other major themes, on aspects such as cemeteries, mining and metallurgy, communications, etc., as well as more specific problems such as aspects of 'protohistoric' settlements and their populations, and minor urban and rural settlements, through to some site specific projects. Some of the larger field programmes involve several teams working on different aspects of the same '*projet collectif de recherche*' (e.g. Guyon (ed.) 1991), whose endeavour and direction are co-ordinated by regular meetings and project reviews. I hope that we can observe such initiatives and learn something from them; in many cases it appears that they do not take place in an awareness of the sort of research framework which many British theoreticans would advocate. It is at the same time true that each country has younger groups of scholars who are getting to grips with the changing theoretical approaches seen in Britain (e.g. Hodder (ed.) 1992). The development of theory during the 1970s largely passed the Units by, no doubt partly because they were so preoccupied with site work, with developing new practical applications and with improving and refining recording systems. Moreover, the difficult language of theory and its apparent lack of application to their problems meant that the effort to grasp its significance did not seem worthwhile. Worse was to follow: new theoretical approaches, new theories, and new techniques, many of them borrowed from other disciplines which were already casting them off, seemed to follow each other even more quickly. Of course, some of them left an obvious mark: e.g. spatial analysis, explicit sampling strategies, environmental context. Others, like ideas on social power (Mann 1976) or centre-periphery, would be the talk of one TAG Conference and unmentioned at the next. It was not too obvious to the uninitiated that these ideas were in fact finding their way into the literature but publication of appropriate papers did usually follow (for the application of centre-periphery relationships, see for instance Rowlands and Larsen 1987; Cunliffe 1988; Champion (ed.) 1989; Parker Pearson 1989; Millett 1990).

Now I know that a year is a long time in archaeological theory but units have only a number of specialists, and for those of us trying to make the best use of our time, the multiple whammy of theory after theory with no simple guide to their value (cf. Hodder 1986; Renfrew and Bahn 1991: 426–34) was quite bewildering. From a student perspective, a year *is* a long time, and the excitement of stimulating ideas obviously found a ready market here. A theoretically aware generation has now found its way into the units, and is carrying out good work. But, as I said above, some of our needs are for skilled practitioners in more tedious, practical tasks: perhaps they need a question to start with? It might also be said, perhaps uncharitably, that some younger academics enjoy and benefit professionally from the kudos of being the first to espouse and propagate a new theoretical application. Even academics who have been more open to new ideas are expressing concern that there is too much theory and not enough practice (e.g. Bintliff 1991, re post-modernism at TAG; Snodgrass 1987: 9, on the need to demonstrate that theories work in practice). At least there is some recognition that theories need to be applied to the Roman period with care. Moreover, the theoretical scene is constantly in flux, a fact which gives traditionalists an excuse not to take it seriously and decision-makers 'a pretext' to decry advice from academics. John Barrett has recognised this and has made a plea for a general agreement on the aims of archaeological research (1989). Competition may be healthy within the subject but a unified standpoint is at times necessary when the value and continuation of research is under attack.

Our needs in the units are for regular and clear communication of what is important, what we should be looking for, how we should be analysing our abundant evidence, and of course several approaches are valid. We need to know how to use the archaeological data to the best effect. I like to think that the forerunner of these TRAC occasions was the volume on *Recent Trends* drafted under Rick Jones' editorship and published in 1991, but largely written in 1986 (R. F. J. Jones 1991). Like much of that volume's contents, my own contribution set out some sort of agenda and although it made a gesture in the direction of theoretical approaches, was not written from a theoretically-informed position (M. J. Jones 1991). It is interesting to note that English Heritage is confronting the problem of the viable sample (Startin 1993), one aspect I did consider, and a thorny problem indeed.

So what should we in places like Lincoln be doing with our evidence? We have some help from historians, although not all of them are yet convinced of the value of archaeological evidence. David Peacock's 1982 study of pottery was a notable exception in impressing ancient historians with the contribution which archaeology can make. No doubt this partly reflects the fact that historians, belonging to a more established discipline, tend not to need to grasp on to theory as much as the new, insecure discipline of archaeology seeking to establish itself in its own right. Yet historical approaches have moved on considerably in recent years. Moses Finley (1985: 18–23; 59–66) discussed the relationship between history and archaeology, and argued that the need is not more exhaustive monographs on individual cities, however scholarly. More important to Finley, who accepted the value of models, would be to establish how the economic system worked to support such a large settlement (see Lazreg and Mattingly 1992 for a good example). Finley's successors have carried the debate further, but no consensus exists (Garnsey et al. 1983; cf. Wallace-Hadrill 1991; Eagles 1990). It is certainly clear that we need to study the urban-rural relationship, which includes studies of environmental samples such as animal bones linked to survey of the hinterland (see Millett 1991 for a recent account of one survey). A variety of relationships is to be expected: as Philippe Leveau, a tireless French fieldworker, has written, towns are not the only element of Romanisation, nor are they synonymous with civilisation, merely a sign of a certain form of it; there are several different types of urban systems, several different types of countryside (Leveau quoted by Vallet 1991: 77). Further help is provided by books like that produced by Dominic Perring on *Roman London* (1991), combining the latest archaeological data with recent theoretical ideas, although for many problems it presents only one of several possible interpretations (e.g., cf. Williams 1990). There is pressure to produce cosy,simple histories, for the local and popular markets; we know that the reality was both more complex and uncertain, and that life for most was always fairly wretched, exploitation rife (e.g. Walsh 1992: introduction).

How then are we reconciling these approaches in Lincoln? Work is advanced on a major publication project, with substantial funding from English Heritage, on those sites excavated before 1988. The academic framework and objectives of this project were largely devised by our then inspector, Mike Parker Pearson, now a lecturer in archaeological theory at Sheffield University, in conjunction with my colleague, Alan Vince, who manages the project. Not surprisingly they take due note of the research framework recently published by English Heritage (1991). Progress on the project is being monitored for English Heritage by Tim Williams, who is known and respected by our team for his helpful and knowledgeable support: he too has had much experience of analysing urban stratigraphy and finds, and his researches into Roman London (e.g. 1990) are worthy of inclusion in this volume.

I want to summarise the themes of the research (Vince (ed.) 1991), which of course covers the post-Roman periods too. The analysis of site and finds data can take place more easily because the data has now been computerised, on a multi-user system. Even with generous support, we have had to be selective, to confine finds reports to those which are of patent value, and to limit our academic objectives to three principal themes, which cover questions to which out present data can be applied. They are:

1. The extent of settlement in Lincoln and its suburbs through time and the development of major foci. Settlement is plotted using the pottery databases to chart the incidence of diagnostic types, supported by a synthesis of the stratigraphic evidence and non-ceramic artefacts. Major foci shifted through time, but they included the principal routes and public buildings. Some attempt is also being made to estimate the changing population level through time (see Marsden and West 1992), and social differentiation in relation to material culture.

2. The study of spatial patterning within the town and its suburbs: was there socio-economic zonation? Evidence for commercial activity will obviously be examined here (see Redman 1986 for a good analysis of this theme at Qsar-Es Seghir).

3. Lincoln's hinterland, its trading contacts and their effect on and relationship to the fortunes of the town itself. Pottery will again be a major, but not exclusive, source of information. No new fieldwork is planned; rather the information on the hinterland will be gathered from published and archive sources.

In the course of the project, another theme – that of the major social transformations of the transitional phases – will emerge and the results will be set in their appropriate academic context. But we realise that there are further important areas which will still require attention in due course: for instance, organic finds and the local environment, for which excavations by the river in 1987–90 produced the most useful data. More work is desirable in the longer term related to the wider environment, and on tenurial arrangements and social relationships, property boundaries and land division.

That programme as far as we can tell, is both 'theoretically informed' and appropriate to the available data. It does not prevent us from pursuing in more detail related problems. I mention some here which are my particular concern.

One is the plan of the forum – analogues need detailed study – and the implication of its design in terms of cultural influence, linked perhaps also to what it symbolised (Jones and Gilmour 1980). Rick Jones (1987b) and Martin Millett (1990: 72–73) have touched on, but in relation to the tribal capitals rather than the *coloniae*. Where do the influences derive from and through which mechanism (Blagg 1990; Blagg and Millett (eds) 1990)? The original inhabitants of the military colonies, those who oversaw the development of public-works, were predominantly the retired legionaries who had origins in the Mediterranean area – or so it is assumed; is this distinction reflected in architectural styles and what does this tell us about both the clients and the designers? Can we recognise a distinct elite culture in the early coloniae? If so, when does it cease to be distinctive?

Another major area of research is the nature of the late Roman town, and the symbolic and economic implications of its fortifications (Jones 1993; Jones et al. forthcoming). Some work has already been undertaken on the source of building materials for the city's strong defences, which survived as a 'relict feature' to influence subsequent topography (Jones 1980; cf. Greene 1986: 152–54).

I want to end with the last 'live' remnant of *romanitas* in the city. The existence of an early church-like structure in the middle of the forum was established in 1978, dated to no earlier than the late 4th century, and if you believe the radiocarbon dates of nearby burials, possibly gone by the end of the 6th (Jones 1994). Again it can be viewed as a symbol of power. There is no exact parallel for this phenomenon, although the Exeter forum may have contained a similar structure in the sub-Roman period. Nor is there any certainty in the interpretation of the evidence: I have, therefore been examining better-preserved evidence for early churches in Gaul and the Mediterranean provinces as well as in Britain. The research has suggested several alternative interpretations; e.g., was this part of the 'episcopal group'? Study of the continental evidence is helped by the existence – especially in those Catholic countries which boast better urban continuity – of the thriving discipline of 'Christian archaeology' (Duval 1991). Is it an appropriate appraoch (cf. James 1993)?

This fragmentation of various branches of archaeology, in different ways from in Britain (there is also a 'Merovingian archaeology' in France), strikes one when studying mainland Europe. The different disciplines do not necessarily mix. It is a problem from which we too suffer. As theory advances quickly, much Roman archaeology is still firmly rooted in the traditional disciplines. Let us by all means have a healthy and unrestrained theoretical development, but most of all, let us communicate with each other.

POSTSCRIPT AND ACKNOWLEDGEMENTS

Since this paper was written and delivered to the publishers, some echoes of its content can be found, set out more eloquently, in the volume *Archaeological Theory: Who Sets the Agenda?* edited by Norman Yoffee and Andrew Sherratt (Cambridge University Press, 1993). The papers by Richard Bradley, Christopher Chippindale and the editors themselves are of most relevance to the substance of the paper above.

Three reviews of Martin Millett's *The Romanization of Britain* which have now appeared form a useful collection. Although all are by the middle generation of scholars, they take some issue with both Millett's general approach and some of his detailed arguments. See M. G. Fulford in *The Archaeological Journal* 148 (1991): 307–09; R. J. A. Wilson in *Journal of Roman Studies* 82 (1992): 290–03; and P. W. M. Freeman in *Journal of Roman Archaeology* 6 (1993): 438–45.

The paper as here presented is a revised version of that read at the conference, with some of the more ephemeral and flippant material deleted. I thank the organisers of the conference for agreeing to include such an eccentric contribution and subsequently to publish it. In its present form I take full reponsibility for any errors and misrepresentations, but need to register my debt to the following, who kindly commented on an earlier draft and who were without exception encouraging in their response: Dr D. G. Coombs, Pamela Graves, Dr R. F. J. Jones, Dr D. J. Mattingly, Dr M. Millett, Dr M. Parker Pearson, and Peter Rush. Much of the research for this paper was undertaken during my tenure of an Honorary Simon Professional Fellowship of the University of Manchester, and I thank Professor G. D. B. Jones and Dr J. P. Wild for their support.

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