Things are happening on the way to the forum... A review of 'Britons and Romans: Advancing an Archaeological Agenda'.


Reviewed by Adrian Chadwick

Introduction

In their introduction to Britons and Romans, Simon James and Martin Millett state that the volume arose out of the debate concerning the development of national, regional and local research frameworks. This discussion was partly sponsored by the publication of research strategies by English Heritage, in its Exploring our Past (English Heritage 1991), Frameworks for our Past (Olivier 1996) and Exploring our Past 1998 (English Heritage 1998) documents. Nevertheless, they go on to say that:

...the use of this volume...is designed not to specify any particular research agenda, but rather to encourage those with an interest in Roman Britain to think broadly about how future research might develop (James and Millett 2001: 1).

Although the volume they have produced is an eclectic and erudite one, I would argue that it needs a much clearer focus, and a much more explicit framework for future research. The chapters are based on individual conference papers delivered to a session at the Roman Archaeology Conference in Durham in April 1999. I will return to discussion of this format later. Firstly however, I wish to briefly discuss the individual papers into which the book is divided.
The contributions

After the very brief introduction by Simon James and Martin Millett, John Creighton contributes a chapter on the Iron Age-Roman transition. Rather than discussing some of the recent ideas emerging from postcolonial theory however, he chooses to concentrate on the role of client kingdoms and obsides within the Roman empire. The latter refers to the tradition of friendly or client kingdoms sending their children (often the eldest son) to Rome to be educated and brought up in a Roman manner. Interestingly, many of these obsides went on to defy Rome. In a deliberately speculative and provocative manner, Creighton then considers two case studies from southern England (Gosbecks and Fishbourne). Here, instead of traditional narratives suggesting that the earliest Roman-style features dated to the conquest of 43 AD or its immediate aftermath, the evidence could also support the notion that these were pre-invasion sites, where Roman-style troops or even Roman legionaries or auxiliaries themselves were garrisoned. This could have been to support client rulers before the actual invasion itself, and might reflect cross-Channel contact some years or even decades before the conquest. Such ideas would need to be tested through comprehensive fieldwork, but they are certainly intriguing, and show how traditional, linear culture-history narratives might actually be more complex, and how we should re-examine many previous assumptions about Roman Britain.

J.D. Hill then contributes an article on recent theoretical approaches to identity in Roman Britain, specifically looking at ‘Romanisation’, gender and class. There are many excellent ideas in this section, which for me at least must be one of the strongest contributions in the book. It clearly demonstrates the exciting possibilities offered by more self-critical, theorised approaches to the data. In many cases, this work has been strongly influenced by exciting developments on postcolonial theory and material culture studies, and this has also been frequently on the agenda of TRAC (the Theoretical Roman Archaeology Conference). However, although this
paper is a good summary of current thinking, I feel that it also conceals some hidden tensions and disputes. The main focus of J.D. Hill’s research is in the British Iron Age. Was he brought in as an agent provocateur, to examine Romano-British studies as a self-confessed outsider? This certainly brought some interesting and fresh perspectives, but does this imply that some Romano-British specialists are still unwilling to engage with social theory? Good though his section was, I nevertheless felt that it contained some very negative remarks:

While the language of these debates is strange and alien to many of us, and their full working through is yet to come, at their heart they are all stressing the need to understand the social realities of different peoples’ lives in Roman Britain (Hill 2001: 12).

This half-apologetic statement hardly furthers the cause for the use of self-critical social theory within Romano-British studies. True, the language employed in these debates often requires intense concentration, and may indeed seem ‘strange and alien’ to some, but this is because such words have very specific meanings. It is now widely recognised that we have to be much more careful with how we employ language within archaeology. I accept that some very theoretical discussions may appear somewhat dense to those without knowledge of many of the terms used, and there is no doubt that some theoretical discussions should be explained in more everyday language to make it more accessible to all. However, we all have theories about the past, and one thing such discussions do is to make us critically re-examine our own beliefs and preconceptions. All archaeology could be accused of employing strange and alien language. It always amuses me when I hear colleagues moaning about the use of theoretical jargon, that the same people will then often go on to talk about SMRs, APs, EH, geophys, dendro, hi-macs, drotts, JaCoBs, RSJs, acro-props, stiffs, boss strat and ‘big matrix pleasure’ without batting an eye. Other people in pubs must wonder what the hell we are on about most of the time, and also why the letters ‘IFA’ seem to cause so
many outbreaks of anger or mirth.

It is also very disingenuous of J.D. Hill to refer to theoretical debates in this way, when he was accused of exactly the same thing in the late 1980s when he was one of those bringing fresh, theoretically informed perspectives to Iron Age studies. This playing to the gallery or ‘dumbing down’ only reinforces the notion that theory will always remain bizarre, and is something to be added to interpretation, rather than it becoming an important and intrinsic part of our everyday archaeological practice. Theory is not just something that can be applied to sites or assemblages after they have been excavated. The fact that J.D. Hill felt that he had to write in such a negative and half-apologetic manner may show just how much resistance there still is to many of these ideas within Romano-British studies.

Lindsay Allason-Jones contributes a section on material culture and identity. As these form some of my research interests, I was particularly expectant about this chapter. Yet overall it is rather disappointing. Rather than presenting an overview of recent work in this field, she instead offers a single case study (Allason-Jones 2001: 21). Unfortunately, this work has already been published elsewhere (Allason-Jones 1988). She does however discuss some possible future research questions that finds studies of Roman military, civilian, religious and burial assemblages could explore. Whilst she welcomes some of the English Heritage statements on research frameworks, she also makes the very valid and important point about who is going to do the work on existing artefact collections, and study the new ones? There are currently very few undergraduates and postgraduates currently undertaking work in finds. This problem has been noted by many of my colleagues in both contract and academic archaeology, and by specialist finds organisations such as the Medieval Pottery Research Group.

As Allason-Jones states, there is likely to be a skills shortage in material culture studies in the years to come. To blame it on students ‘often preferring to do
their dissertation or theses on more theoretical topics’ (Allason-Jones 2001: 24) seems to be rather a trite comment though, and perpetuates a stereotypical theory: practice division that is divisive and extremely unhelpful (q.v. Pluciennik 2001). Surely these problems result more from students not wishing to embark on time-intensive analysis, due to the increasingly strict deadlines being applied to postgraduate work from universities and research councils, and for undergraduates the increasing lack of personal tuition from lecturers (which the training of people in finds work requires). This has occurred when rising student numbers have not been met by a concomitant increase in funding and staff. Most lecturing staff now find themselves barely able to cope with preparing and giving lectures and marking the assignments from them, let alone contemplate one-to-one tutorials. Many of these difficulties have been summarised succinctly elsewhere (Collis 2001b), and are certainly recognisable to my colleagues in many different universities, and myself.

Allason-Jones’ 1988 paper, and her chapter in Britons and Romans, are both marred by their uncritical notion of ‘small finds’ (although she is by no means alone in such assumptions, e.g. Spradley 2001). This concept brings with it a number of theoretical and practical problems. The designation of an object as a ‘small find’ gives it a reified and privileged status (explicitly or implicitly) over other objects from the same archaeological context. Furthermore, it may be attributing meanings to objects that reflect contemporary understandings and preoccupations, rather than the meanings material culture might have had in the past. This method of ascribing importance to particular objects is predicated on values derived from antiquarianism (Cumberpatch and Dunkley 1996: 7). Objects are considered as having importance in their own right, rather than their potential for facilitating interpretation of the context they have been recovered from, and the past human activity reflected in that context.

Yet the ‘small find’ may not offer any greater insights into past human activities or beliefs than the mass of other material from the context. Indeed, they may
have less ‘inference potential’ (q.v. Adams 1991). For example, a lost intaglio or a worn coin from a secondary pit deposit may tell us little about the nature of occupation on a particular site, but the pottery, animal bone and palaeo-environmental remains from the same context may provide much more information, and thus have more ‘inference potential’. There is also the practical problem of another layer of recording being necessary to link separate ‘small finds’ and sample numbers to the context number, and to have three strings of numbers for the material recovered. This increases the danger of mixed numbers and poorly numbered bags.

It may be more productive to follow the approach pioneered by Professor John Collis on his excavations on Iron Age and Gallo-Roman sites in central France (e.g. Collis 2001a; Loughton 2000). This was subsequently adopted and developed by several British contract field units such as the South Yorkshire Archaeology Unit (now sadly defunct). Here, although objects can be bagged separately from bulk finds according to their individual conservation needs, they are merely assigned different bag numbers within the overall context number (Collis 2001a: 84-86; Cumberpatch and Dunkley 1996: 7). This restores the individual context as the primary unit of recording. Rather than having separate sample numbers, sample bags or tubs too can also have bag numbers attributed to them, with the context number still paramount. Finds, samples and context information thus remain more closely linked, and communication between specialists during post-excavation is also facilitated. I am surprised that a finds specialist like Lindsay Allason-Jones has not come across such self-critical and valuable work. It is high time that ‘small finds’ became a redundant category within British archaeology.

Lindsay Allason-Jones also does not really engage with any of the recent exciting developments in Roman material culture studies, perhaps because there was no time for her to talk about these during her conference paper. However, this is a shame, for
it is in the field of material culture studies that some of the most interesting and dynamic work on Roman studies and the Romano-British period has taken place. To take just a few examples, there is the work of Karen Meadows, who has looked at the social context of eating and drinking in the very late Iron Age and Romano-British periods. She has linked finds, animal bone and archaeobotanical assemblages in her discussions of identity, consumption, material culture and changing social practice (e.g. Meadows 1994, 1997, 1999). There is also Penelope Allison’s innovative work on Roman household archaeology in Pompeii (Allison 1997a, b, 1999), employing more contextual and holistic approaches to material culture studies that many archaeologists would do well to pursue in Britain.

There are studies by Patricia Baker on Roman civilian and military medicine and medical instruments, and Andrew Gardner’s work on the material culture of the late Roman army, which is used to examine questions of context, social practice, identity and gender (Baker 1999, 2001; Gardner 1999, 2001). The work of J.D. Hill (1997) and Gilly Carr (2001) who have used toilet instruments to discuss issues of ‘Romanisation’ and the body must also be mentioned. There have also been initial considerations of placed deposits in Romano-British contexts (e.g. Aitchison 1987; Clarke 2000; Millett 1994; Reece 1988; Fulford 2001). There is also the work of Steve Willis or Jeremy Evans (e.g. Evans 1995; Willis 1997). I am aware that I have ‘cherry picked’ here, and have selected just a few of the articles that I have read in the past ten years that have particularly excited me. There are many other innovative material culture studies out there, and is a shame that Lindsay Allason-Jones does not consider them.

Jeremy Evans’ contribution shows that, using established techniques of artefactual and statistical analysis, it is nonetheless possible to ask new questions of the data, and to come up with new ideas for discussion. He also mentioned the problem that few assemblages are ever published in their entirety, and outlined some of the many biases in the
information we are able to retrieve. Nevertheless, I felt that he also failed to mention much of the exciting recent work in Romano-British artefact studies. His ideas may indeed be useful for characterising site types, and should certainly be part of the initial analysis of finds assemblages, but how close do these ideas take us to understanding the everyday lives and social practices of people in the Romano-British period? We need to answer some of the ‘big’ questions certainly, about what a site was and what its ‘function’ might have been, but we should not allow ourselves to think that this is where our analysis ends. Archaeology is about the study of past human lives, and I believe that we should start to recognise that we have an ethical commitment to writing histories for these long-dead people.

Keith Dobney’s article on the state of zooarchaeological research on Roman Britain outlines the principle trade and economic arguments, but also shows how animal bones can be used to discuss identity and ethnicity, and ritual and symbolic behaviour. This shows the possibilities of combining traditional approaches to the data with new research questions and theoretical ideas. This is also a feature of Jeremy Taylor’s paper on rural society in Roman Britain. I think that is was perhaps the strongest paper in the whole volume, but then I am biased as my own research is on rural communities. Nonetheless, he covers a large amount of ground, and incorporated everything from material culture and artefact assemblage studies to settlement patterns, field systems and land tenure, villas and social space within Romano-British buildings. I especially like his diagrammatic means of displaying data from excavated roundhouses, aisled halls and ‘developed’ aisled buildings. This builds on the earlier work of Hingley (1990), although this 1990 paper now seems rather essentialist and overly structuralist in the light of recent contributions from gender and feminist archaeology. It also acknowledges the contribution of studies of social space within Iron Age roundhouses (Fitzpatrick 1994; Oswald 1997; Giles and Parker Pearson 1999). Taylor also advocated the development of
studies and research initiatives based at a much more regional level. This paper was bursting with ideas, and is a very important contribution to Romano-British archaeology.

Martin Millett discussed approaches to urban societies, which was also the theme of another paper within the volume, the result of a CBA working party. Martin Millett’s paper proposed some very interesting areas for future research, including looking at social space within urban houses, and the construction of social space and movement within urban settlements as a whole. I would certainly applaud any attempts to bring in ideas such as Tim Ingold’s ‘taskscape’ (Ingold 1993; 2000) to develop what amount to ethnographies of urban places. This has been something that has been considered recently for other archaeological periods, or in other disciplines (e.g. Chadwick In prep; Croll 2000; Mayne and Lawrence 1999; Rendell, Penner and Borden 2000). There has been some really exciting, theory-driven multi-disciplinary work in urban studies. Not only could this benefit Romano-British urban studies immensely, but more importantly, Romano-British studies could contribute much to these wider inter-disciplinary debates regarding experiences of urban inhabitation.

The CBA working party consisted of Barry Burnham, John Collis, Colin Dobinson, Colin Hazelgrove and Michael Jones, and the results of their work were originally published on the Internet (http://www.britarch.ac.uk/research/urban1.html). Their inclusion here sits well with Martin Millett’s paper, but is to some extent at odds with much of the rest of the volume. This is not through any faults with the article. On the contrary, it is a very cogent and succinct summary of the present state of knowledge, although the unusual referencing is awkward and irritating. Most importantly though, this working party have proposed a series of strategies by which these research ideas could be implemented, ranging from improving on-site practice through to the development of local, regional and national research initiatives. This is a far clearer statement of research goals than is to be found in most of the other
Simon James contributes an article exploring military and civilian identities and interactions during the Romano-British period. He suggests some potentially productive questions for future research, but unlike the other papers, also proposes some real mechanisms by which these could be achieved. However, these fall down somewhat in relying on the largesse (or not) of English Heritage, and the Heritage Lottery Fund. Again, this is a weakness. I feel that it may be more pertinent to propose ways in which routine, developer-funded fieldwork can be collated and integrated into these sorts of research frameworks. This may not be possible under the current unregulated and unrestricted system of competitive tendering. He or other authors in the volume might have referred to ideas concerning a general Development Tax, or other recent proposals to end direct developer-funding, and to reinstate research back into the heart of archaeological practice (e.g. Carver 1989; Cumberpatch and Blinkhorn 2001; Graves-Brown 1997).

Finally, Simon Esmonde Cleary has a paper discussing the Roman to medieval transition. His paper is illustrated by some rather poorly put together artefact drawings, reproduced for the most part at inappropriately large scales. This detracts from what is a short but incisive paper, and one that actually does engage with developer-funded fieldwork and the opportunities it presents. I also cautiously welcome his idea that a Late Antiquity framework would less cover in a less divisive manner the period from the 4th century AD to the 7th century, and agree that this would re-integrate Britain within continental developments. However, in some parts of Britain such as Wales, this might have the effect of over-emphasising the difference
between the Roman and post-Roman periods, at a time when some recent research is suggesting that there might have been some more continuities than has been previously thought.

▲ Some general thoughts

Apart from John Creighton and J.D. Hill’s contributions, I feel that there is a lack of discussion of processes of colonialism and reactions to it. There are also surprisingly few suggestions as to how material culture and landscape studies could be employed in these arguments. The latter in particular seem to be conspicuously absent from the volume, despite the fact that landscape and understanding human experiences of it has become a key issue across the social sciences and humanities. It is now becoming a concern in Romano-British studies (e.g. Petts 1998), but these approaches need to be more explicitly commented upon. It is mentioned by Jeremy Taylor’s paper, as was regionality, but these are barely touched upon elsewhere. All too often, many books and articles published on Roman Britain, even some written by quite prominent academics, convey the idea of a homogenous Roman culture being applied across the whole of the province. They fail to take into consideration the many local variations that already existed, or that developed through the complex, two-way interactions of colonialism, and the varied ethnic makeup of ‘Roman’ immigrants themselves. The study of field systems, archaeozoological and artefact assemblages could all be utilised to investigate this, and this should be another important area for future research.

Despite the cogent contribution from Jeremy Taylor regarding Romano-British rural settlement, there is little consideration in the volume as a whole of trying to develop archaeologies of inhabitation or experience for the period (q.v. Barrett 1999, 2001; Meskell 1996; Shanks 1992). Most archaeological accounts from all periods still produce universal bodies devoid of identity, who lack situated human engagement with the world, and who seem to experience items of material culture and landscape
features in isolation from one another. I believe that as archaeologists we need to be trying to circumvent these problems. Such archaeologies would engage more with the day-to-day social practices and routines of these communities, but would also allow for the multiple experiences and perspectives of the different occupants to be considered and explored. There is no outline of how we can go about imagining these different humanities, and how exciting recent theoretical ideas concerning such topics can be worked through our everyday archaeological practice. Yet rural Romano-British farmsteads and field systems are often the sites most frequently encountered during developer-funded fieldwork. How can routine, small-scale investigations such as evaluations contribute to wider research frameworks? What about advancing some suggestions concerning the dissemination or publication of these investigations? At the present time there seem to be real problems with these issues, particularly when so many sites end up as unpublished site reports.

There was also no clear discussion of the theoretical gauntlet laid down by TRAC (the Theoretical Roman Archaeology Conference), the many issues that have been discussed there, or the potentials, advantages and disadvantages of these approaches. In particular, many of the points that have been raised by John Barrett and Eleanor Scott have not been followed up (e.g. Barrett 1997a, 1997b; Scott 1993). This is despite the fact that TRAC, and more theorised approaches to Romano-British archaeology generally, are now quite clearly having the same enervating effect on the discipline that they have had on Iron Age archaeology during the past fifteen years. These may still sometimes be eclectic and not fully worked through ideas, but at least people are prepared to write and undertake imaginative and innovative approaches (cf. Hill 2001; Laurence 1999). Romano-British archaeology seems to be undergoing a schism. One half is now pursuing theoretically informed research on identity, colonialism, gender, class and material culture, whilst the other half steadfastly refuses to recognise or utilise any of this work, and continues to write the
same meta-narratives concerning military campaigns, trade and economy, and the development of urbanism. Although the latter are still worthy research questions, I feel that they need to be re-worked through understandings of the everyday lives of Romano-British peoples.

Britons and Romans also suffers unfavourably in comparison with a ‘sister’ volume - Understanding the British Iron Age: an Agenda for Action (Haselgrove et al. 2001), itself based on an earlier web document (Haselgrove et al. 2000). Rather than a collection of individual papers, this Iron Age volume represents a much more integrated summary of the state of present knowledge, and clear research aims designed to take our knowledge further. Crucially, it outlines those areas of Britain where there are research frameworks, those areas where information is unsorted, and those areas where little or no work has been done. It also identifies five strategic areas that will be central to future research on the British Iron Age, and suggests ways in which these goals can be implemented. It is an approach that Britons and Romans could well have done with following.

Furthermore, on a slightly more prosaic level, Understanding the British Iron Age is well illustrated with line drawings, tables and photographs, and costs less than £5. Britons and Romans has fewer illustrations, some of which do not seem particularly relevant, or at least over-large (e.g. pages 57, 94-95), and yet it weighs in at over double the length, and nearly four times the price of the Iron Age volume. The cost may be due to differences in the amount of grant aid from English Heritage and Historic Scotland available to each volume. Nevertheless, for the amount of money expended, readers should expect a more clear cut series of statements about the ‘state of the art’ in Romano-British studies than they have received.

Understanding the British Iron Age is a document that curatorial archaeologists can use readily to inform their project briefs, and one that contract field units can consult when preparing their research.
designs. Many curatorial archaeologists in particular do not seem to attend TAG (the annual Theoretical Archaeology Group conference) or more specialised period-based research meetings, and instead go to more general gatherings such as the IFA Archaeology in Britain conference. This is partly a result of their rising work commitments, often exacerbated by local government cuts. They therefore look to publications that can inform them about recent trends in interpretative ideas, and outline current understandings of available information.

This is a task that readers may find difficult, given the fractured nature of Britons and Romans. Many of the individual authors make some excellent points, and several of them reiterate certain key problems, such as the need for more material culture specialists for example. But there is no summary or integration of these ideas. Only the ‘Themes for urban research’ section was able to do this, and this is the result of a CBA working party. This is the result of the volume being a series of published conference papers, rather than articles written especially for it. Many of the contributors might have been able to write much better ‘state of the art’ summaries if they had been given the opportunity to do so, and a concluding series of key points would have been welcome. However, I also wonder if it was not possible to achieve any form of consensus. Compared to the audience attending TRAC (the Theoretical Roman Archaeology Conference), I have observed that some of those going to the Roman Archaeology Conference still seem to be engaged in very traditional archaeology, and are even reactionary to new ideas at times. J.D. Hill’s half-apologetic remarks concerning social theory and critical debate may reflect unease amongst some of the target readership regarding the need for new approaches to the evidence. Attempting to produce research agendas to suit all of these disparate elements in Romano-British studies may simply have proven too problematic.

By contrast, the Iron Age Research Seminars are fairly exclusive affairs. They are not well publicised,
and there is an air of ‘by invitation only’ about them. To my knowledge only one of these meetings, held in Sheffield in 1998, has attempted to widen the list of those invited (this being the only one I have attended!). Nevertheless, perhaps because these seminars are still dominated by a dozen or so key Iron Age researchers, they may have been able to produce a more focused document. Understanding the British Iron Age is also much less self-conscious about the use of theoretical ideas, and it offers clear means by which developer-funded fieldwork can contribute to wider research questions.

**Conclusion**

Overall, Britons and Romans: Advancing an Archaeological Agenda is an interesting and timely volume. To be fair, it only claims to be advancing a research agenda, rather than establishing one. Despite its many problems, it will form the basis for many discussions in the next few years. But it could be and indeed should have been so much better. In particular, no real framework is offered by which developer-funded fieldwork can be integrated into wider research goals (apart from the two ‘urban themes’ sections). No real mechanisms for implementing the research questions are proposed. A real opportunity has been missed to present a cohesive and clearly articulated set of ideas and areas for future research.

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**Adrian Chadwick**

Adrian Chadwick is a Lecturer in Archaeology and Prehistory at the University of Wales College Newport. He graduated from Sheffield University’s Department of Archaeology and Prehistory in 1990, and has worked for many British field archaeology units. In 1999 he finished a part-time MA in
Landscape Archaeology, again at Sheffield University. He is studying for a part-time PhD, on the Iron Age and Romano-British field systems and rural communities of Nottinghamshire, South Yorkshire and West Yorkshire. He can be contacted at: adrian.chadwick@newport.ac.uk