Concluding Discussion

By Martin Millett*

The ending of Roman Britain is clearly a subject of enduring interest as witnessed by the papers in this volume. The contributors share a subject, but differ in their approaches to it, sometimes in fundamental ways. There is some inevitability about this because there are very well-established and differing traditions of study which depend, *inter alia*, on perspective (Romanist or Medievalist), regional focus (South and East versus North and West), or material interest (e.g. coin specialist against landscape historian). There are also subtle differences of approach to both textual and archaeological evidence, which might be characterised as cautious scepticism at one extreme, and optimism verging on credulity at the other. Add to this the increasing evidence that Roman Britain itself was highly regionally varied, even very locally differentiated, and we might think that the subject addressed by the authors here is certain to provide a multi-vocal narrative which, however rewarding to read in the post-modern world, does not carry us much further towards any consensus about the historical reality of fifth-century Britain. Whilst this first impression may not at first sight seem very positive, I would suggest that there are areas where we can see ways ahead in understanding this key period. Without seeking to summarise the papers, I would like briefly to draw out three topics worthy of consideration.

BRITAIN AND THE CONTINENT

It is entirely appropriate that a number of the papers presented lay stress on the importance of placing Britain in its proper Roman imperial context, thereby considering contacts with the Continent. I would argue that this is essential if we are to make best sense of the period, and too many past approaches have been too inward looking. However, such an approach must certainly move beyond simply labelling the period in Britain as Late Antiquity (a term which is surely appropriate in the terms that Peter Brown demonstrated as long ago as 1971). However, the thrust of recent work, as notably summarised in Simon Esmonde Cleary's book (2013), is that there are a variety of different local and regional trajectories in the archaeology and history of the near Continent, so we can neither view Late Antiquity as a uniform phenomenon nor thus reliably use parallels from any particular area as a model for events in Britain. This point is well illustrated by Peter Guest's paper which shows how patterns of hoarding appear to represent a particularly insular pattern but that this should nonetheless be placed within a broader European context (both within and beyond the Empire). Equally David Petts' paper which examines Christian contacts within a Gallic context is welcome and thought provoking. However, the contrasts with contemporary Mediterranean developments and an understanding of the complex local patterns of Christian archaeology across the Empire would repay further exploration within a more regionalised understanding of the late antique world.

With coinage we have important new evidence which is being enhanced by the study of both hoards and stray finds (principally through the success of the Portable Antiquities Scheme) as discussed by Sam Moorhead and Philippa Walton. This is providing a much sounder foundation for understanding, yet underlines the basic conclusion that bulk supplies of coinage to Britain ceased early in the fifth century, with limited numbers of coins continuing to arrive until about A.D. 430. It is important to appreciate how much the understanding of the anatomy of coin circulation patterns is made possible by an agreed international terminology. In contrast it seems extraordinary to me that those studying the imported pottery of the period persist in using the 1950s insular terminology to describe the pottery (e.g. Thorpe 2007), thus making it a struggle

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to relate their findings to the Continental literature! The study, and thus the evaluation of possible trading contacts it represents, is also bedevilled by a failure to place the absolute quantities of pottery found in Britain in proper perspective. The Tintagel material, for instance, amounts to a total of perhaps 150 amphorae and 80 fine ware vessels (Thorpe 2007, 246): certainly a fair haul but insignificant beside the quantities from innumerable Mediterranean sites, and thus difficult to see in terms of direct contact with Constantinople whether through trade or diplomacy as Ken Dark suggests in his paper. What seems clear if we place the finds into a proper European perspective is that they represent comparatively small-scale, possibly intermittent and most likely indirect contacts which although important in terms of the dynamics of indigenous society, seem unlikely to have been very important to those living in the Mediterranean. In this sense, the kind of careful consideration given to the Scottish material by Fraser Hunter might valuably be extended to other parts of the UK, especially the South-West of England.

THE NATURE OF THE OLD EVIDENCE AND THE EXTENT OF THE NEW

It is sometimes suggested that the period under consideration here is difficult to examine because of a lack of evidence. Whilst there is some truth in this with regard to the archaeological material — at least when compared with the earlier centuries of Roman engagement — it is actually far less true when we turn to the textual evidence. By comparison with other periods in Roman Britain there is quite a lot to go on. The problem has often been that as it is difficult to use, work has lacked proper critical evaluation and texts have too often simply been drawn upon to support particular, sometimes simplistic views. Whilst I detect some such cherry-picking of the sources to support rhetorical positions in this volume (for instance, Neil Faulkner's 'peasant revolt'), several of the papers here provide stronger evaluations of the written sources within their historical context, thus providing a corrective to over optimistic readings. However, in the absence of the discovery of a fifth-century source similar to the letters from Vindolanda, it seems unlikely that we will ever have much more textual evidence to consider. The same cannot be said for the archaeological material where not only is fresh evidence being produced all the time, but more importantly, careful and thoughtful analysis of that evidence is providing new insights which have considerable potential.

Aside from the work on coinage and the Scottish material to which I have already referred, I was struck by the papers by Hilary Cool and Ellen Swift on what used to be dismissed as 'small finds' and James Gerrard's thoughts on ceramic production. These careful analyses are important as much for the way that they treat the evidence as important and understandable in its own right, as in the conclusions that they draw. In this sense they provide useful models for the types of future work that will revolutionise our understanding of the period.

THE POTENTIAL OF NEW RESEARCH METHODS

The outstanding problem that we must face up to if we are going to move ahead with a better understanding of fifth-century Britain, is that our present chronological frameworks are hopelessly inadequate. We have been aware of this for more than a generation, knowing that the dating methods on which Roman archaeologists rely fall apart after the volume supply of coins dries up: a deposit with coins of A.D. 388–402 might date to long after that period, or very close to it. Despite excellent past work by people like James Gerrard and Hilary Cool identifying longer sequences into the fifth century, we are bedevilled by a lack of absolute chronology that attaches to these changes in assemblage composition or artefact typology. When we look at the important evidence from excavations like those at Vindolanda discussed here by Andrew Birley, the problem comes into very clear focus.

This volume shows that there is some hope for resolving this issue through the use of radiocarbon as demonstrated on a local scale in the paper about Elmet by Ian Roberts. However, it is equally clear if we stand back from the issue, that significant progress is not going to be made in this area on the small scale. We should learn from the spectacular work on Neolithic chronologies (Whittle *et al.* 2011) or those of the Saxon period (Bayliss *et al.* 2013) where large-scale, long-term collaborative projects have made it possible to provide refined absolute chronologies on a large-scale, thereby contributing to the resolution of historical problems. Anyone who has looked at the use of Bayesian statistics in these projects will be aware that the 'prior assumptions' fed into the analysis need to be very carefully vetted if they are not to produce a spurious framework based on dubious evidence. This would represent a key challenge in any project on the ending of Roman Britain. Nonetheless, with rigorous work to ensure that the 'prior assumptions' are soundly based, we cannot but welcome the prospect of a more precise dating framework. So while we might be inclined to celebrate post-modern diversity amongst the papers presented here, there is no escaping from the knowledge that if we had a less fuzzy chronology for the period, some of the ideas explored would certainly be shown to be less plausible than others. My plea would therefore be for a big collaborative research project to try to provide us with a better radiocarbon-based archaeological chronology for fifth-century Britain.

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