

Editorial

Even as we approach the millennium there is still the tendency to characterise our achievements by decades – expansion of the 60s, recession in the 70s, and so on. Whether or not this is an appropriate way of measuring progress, a decade is a sufficient span of time to allow us to take stock. So what of the 90s for Roman Britain? Is the subject making headway? Does it remain an area where it is still exciting to do research? Where should we go next?

If pressed for a one-word characterisation of the 90s I would offer consolidation. In the autumn of last year, for example, we saw the publication of the last fascicule and index of *RIB* II, which completes the publication of inscriptions on materials other than stone. This is a notable achievement not matched by any other province of the Roman Empire and, like *RIB* I, it will become an indispensable source for Romano-British studies. Hard on its heels comes the completion of the publication of the *Corpus Signorum Imperii Romani* for the whole of Britain; only a very few areas in England now remain to be published. RCHM(E) produced its long awaited *Roman Camps in England*, but how much more of an impact if all the Commissions had combined to produce ‘*The Roman Camps of Britain*’!

At the same time the fruits of rescue archaeology from the 1960s onwards continue to be published. While much has been achieved, many more important results are still awaited from a swathe of towns and cities in England, and there can be few modern successors of the larger Roman towns where the publication of work pre-dating the mid-1980s is now complete. Inevitably cuts in public expenditure have slowed this process down, both by reducing the total number of post-excavation projects which can be supported at any one time and by the agonising scrutiny of the research designs of individual projects to achieve the most effective results by addressing those problems which we currently perceive to be the most pressing. In allowing such resource as there is for post-excavation to be spread more widely, rather than concentrated on the ‘exhaustive’ publication of fewer sites, it is inevitable that some sacrifices have been made, and we need to be constantly reminded of the scope of the archival, artefactual, and environmental material which remains to be researched. Nevertheless there is much to be done yet to complete the publication of pre-1990 rescue archaeology.

But what of the new? There are certainly projects which are attracting attention – the major project at Number 1 Poultry must rank as the great excavation of the mid-90s both in the quality of information informing us about Roman and medieval London, and in the astonishingly imaginative way that the excavation has been undertaken within the basement of the new building. While the excavation of the small town at Heybridge in Essex runs a close second, rescue projects on such a scale are few in number and likely to be even more of a rarity as pre-PPG 16 planning decisions cease to be an issue.

The success of the first Roman Archaeology Conference sponsored by the Society and held in Reading at the end of March 1995 demonstrates the underlying interest in Roman archaeology. For the first time in Britain delegates were able to draw from a menu of themes which concentrated both on continental European Roman archaeology and Roman Britain. Given the characterisation of the 90s as a period of consolidation the success of that conference appears almost paradoxical in the context of Romano-British studies generally, where new, field-based research is clearly becoming a rarity. In last year’s editorial we drew attention to the impact of PPG 16 on rescue archaeology and the

contribution of the latter to advancing our understanding of Roman Britain, but the decline in field projects goes far beyond rescue archaeology. Indeed the latter can be seen as a smokescreen concealing an inexorable decline in the resourcing of field research in British archaeology (here Roman Britain) since the beginning of World War II. If the post-War advance up to 1990 can be attributed to the results of rescue archaeology, pre-War developments can be associated with, *inter alia*, the long-term urban research projects such as Silchester, Caerwent, Wroxeter, Caistor-by-Norwich, and Verulamium. Nowadays such longer-term research projects are a rarity: the excavations at Caerwent by the National Museum of Wales and Cadw, which have been running since 1981, have now come to an end, while the emphasis of the new project at Wroxeter by the University of Birmingham is on survey of the rural hinterland and, by geophysics and other techniques of remote sensing, of the town itself. On the northern frontier work continues at South Shields and at Vindolanda where excavation has produced such a remarkable stream of documents (two of which are published in this volume) illustrative of life and work on the north-west frontier of the Empire. Even the Danebury project, the long-term investigation by the University of Oxford of an Iron Age hillfort of southern England in its landscape setting, and so important to our understanding of the native context of Roman Britain, has now come to an end. Such projects are undoubtedly expensive and the success of the more recent have owed much to the support of local authorities and national agencies like Cadw, English Heritage, and Historic Scotland. Reorganisation and further cuts have radically affected the contribution of such agencies to such research. This is to be much lamented: projects of this kind provide not only a focus, stimulus, and training-ground for the next generation, but also a context for addressing new issues of independent origin, or thrown up by the results of adventitious, rescue-driven research. This plea for more resource for major projects is not intended to be a criticism of small-scale field research. On the contrary, the slimming-down of research resources over the last two decades has led to the development of very effective and targeted research-projects operating on small budgets.

Nevertheless, the decline in field-based research is symptomatic of a much greater malaise which is also evidenced by the small number of full-time University-based research students engaged in studying the archaeology of Britain in the Roman period. This in turn will feed into the pattern of future recruitment into the staffing of archaeological units, heritage agencies, museums, and universities. While, superficially, all seems well, as the steady stream of publications seems to show, we can see that, as the millennium draws to a close, there are severe, structural difficulties at several levels which jeopardise the future of our subject. While the National Lottery may assist with specific projects, it can scarcely be appropriate to the addressing of these underlying problems.

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