

ESSAYS ON ROME AND ITALY

BERNARD (S.), MIGNONE (L.M.), PADILLA PERALTA (D.) (edd.) *Making the Middle Republic. New Approaches to Rome and Italy, c. 400–200 BCE*. Pp. xx + 334, b/w & colour figs, b/w & colour ills, b/w & colour maps. Cambridge: Cambridge University Press, 2023. Cased, £100, US\$130. ISBN: 978-1-009-32798-5.

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This is a wide-ranging collection of essays, in more than one sense. It is something of an assortment – but in a good way, with variety and coverage of different topics – while regions around Rome receive diverse and detailed discussion too. The title may cause a few problems, thanks to the expectations it is bound to create. One gap that follows is noted in the book (there is no chapter on religion), while the Republic – if that word is understood to mean Rome’s system of government – is taken for granted. Old hat though the topic may be, this is unfortunate. Several contributions have some bearing on the question of state formation. And that, in turn, has significance for another old hat topic: the value of the literary evidence. Optimistic claims are made in several places, although there may be a suspicion that some are trying to have their cake and eat it (but that is certainly not a new approach). The decision to use one of the contributions as a conclusion to the book also has its consequences. The last chapter seeks to develop a case of its own, and that almost inevitably makes it less effective as a conclusion to the volume as a whole.

The editors’ introduction and the conclusion aside, the book’s chapters are arranged in three parts, according to the main type of evidence each is concerned with (‘Historical Sources’, ‘Material Sources’ and ‘Architecture and Art’). The first historical source to be considered is the consular *fasti*. P. Wright and N. Terrenato are interested in the ‘foreign’ names that appear in the *fasti*, which they take, as many have done before, to be evidence of migration. Since a number of plebeian leaders had ‘foreign’ names, Wright and Terrenato propose that the Conflict of the Orders should be reassessed. The conflict may not have been concerned only with vertical mobility of ‘local’ plebeian families, but also with horizontal mobility of migrant families. There may be something to be said for this, but there are uncertainties too, not least because the *fasti* provide evidence of office-holding, but not arrival. Rome had long been open to others, and various patrician families were ‘foreign’ too, but most families only become visible when they hold office. If a Licinius (a member of a ‘foreign’ family) was tribune in 493 BCE, would it really make sense to see the Licinio-Sextian legislation of more than a century later as concerned with horizontal mobility, because a Licinius was involved? And what about the Roman people?

In the following chapter J. Tan reconsiders the system of *tributum* and *stipendium*, which he sees as operating in a decentralised fashion (if ‘decentralised’ is the right word to use) and not through the state treasury. The system, he proposes, was run by a considerable number – perhaps many hundreds – of wealthy *tribuni aerarii*, for the most part plebeian, who managed the finances themselves, at a local level, and with some autonomy. This, in turn, empowered those involved. It is a significant proposal, also for what it implies about the nature and development of the Roman state (which is why ‘decentralised’ may not be the right word), as well as later knowledge and understanding of the early state and its workings.

Following an assessment of the economic demands of war, N. Rosenstein reconsiders the settlement after the Latin War and the role of citizenship without the vote. Such

citizenship, he argues, was not about accessing manpower – hence the comparatively limited recruitment of these citizens – but instead about raising tax revenue to fund Rome’s increasingly costly wars. In the final chapter in this section W. Scheidel draws on comparative evidence to explore different practices of slavery, to make a case about the possible nature and scale of early slavery at Rome. This is another matter on which the historical sources have little to say, although, if Scheidel’s comparative material is anything to go by, that is not because there was little to say. It becomes a question of interest and ignorance. Scheidel’s discussion also has implications for Rome’s labour force and for the number of people who might have been available for other activities, such as warfare.

The section on material sources begins with L.M. Yarrow’s study of early Roman bronze coinage. Yarrow suggests that the *aes grave*, despite variation in the weight of extant specimens, functioned as money, in a symbolic way, so that the variation was unimportant for its value. The proposed combination of symbolic value and lack of concern to produce uniform issues is potentially evidence of significant intellectual and political developments and has all sorts of implications, including, as Yarrow discusses, for the relative chronologies of the issues, which become impossible to establish by weight alone.

The remaining chapters in this section are concerned with land, its employment and its produce. T. de Haas reviews the archaeological evidence for rural settlement and land use in central Italy, synthesising findings from the Suburbium Project, the South Etruria Survey and the Pontine Region Project. He discerns a general trend towards growth, in the later fourth and the third centuries, of small farms and larger estates with specialised production for urban markets. The period also saw expansion into areas not previously settled. There is accordingly evidence for modification of the landscape, to drain land, with some areas requiring immense investment of time, labour and revenue. As A. Trentacoste and L. Lodwick discuss in the following chapter, extensification of land use is also a way to increase production, alongside any intensification, so the implications of these costly works for arguments such as Rosenstein’s may be quite significant. All these trends, de Haas argues, reflect the more stable conditions following Rome’s expansion. Trentacoste and Lodwick, in the final contribution to this section, assess the archaeological evidence for agriculture and animal husbandry. Given the diversity of terrain and peoples, it is not a surprise that there is evidence of regional variation in crops and livestock. The evidence also suggests that, following early diversification and before the late Republican period, there was a period of general stability in what was produced. Rome’s expansion evidently had little impact in this regard, but that too is unsurprising. More noteworthy changes are found in the evidence for animals, in their size and numbers. Cows, sheep and goats got bigger, but pigs smaller, despite their importance as food and in rituals. Domestication may explain the decreasing size of pigs, but the growth of the others potentially has implications for the management of livestock.

Part 3, ‘Architecture and Art’, opens with two different studies of urban development. While the perception of Rome rose in the eyes of various Greeks during the course of the fourth and the third centuries, from an Etruscan city to a Greek one, and the city saw improvements in its organisation and infrastructure, D. Palombi argues that the urban model employed by the Romans was inspired by the cities of Latium. Thus, he suggests, the Romans did not ‘Romanise’ Latium; instead, they ‘Latinised’ Italy. The development of Rome in these same centuries is also discussed by P.J.E. Davies. She proposes that the buildings and works of the city (Rome’s ‘object-scape’), instead of being merely passive reflections of political measures, should also be seen as agents that effected change. It is a fascinating case to ponder, but a challenging one to develop, especially for this period. The extant literary sources, which come from much later times, naturally know nothing about

such matters and probably would not have been much interested; besides, objects are rarely just objects. How much can really be said about the agency of objects when considering, for instance, Sulla's work on the Senate house, regardless of whether or not the building was in need of repair, or his expansion of the *pomerium*?

Bernard, the only editor to contribute a chapter to the book, looks at a few historical scenes from Oscan Campanian tomb paintings. He notes, but ultimately passes over, attempts to identify these scenes, to focus instead on the basic point that they appear to refer to specific events. They are not generic, unlike the usual aristocratic scenes of returning warriors and banqueting. Bernard connects this interest in the specific with the rise of the state and its magistracies. But how far this can be cast as 'becoming historical' depends on what came before, the audiences involved, the nature of what was being conveyed and so on. After all, aristocrats who based their standing on ancestry, family status and individual exploits can hardly have been unconcerned about the past. Shifting values, from individual to civic, may have affected what mattered, not only in terms of what was commemorated (in tomb paintings, the specific event instead of the type), but also where and how (funeral speeches, for instance, instead of banquet songs). There is a risk of creating a false dichotomy.

The book concludes with C. Smith's chapter, 'Becoming Political: Middle Republican Quandaries', which is concerned with aspects of state formation. Smith approaches the issue with discussion of the Twelve Tables, land ownership and Latin rights. But, even while contemplating scenarios in which the literary evidence must be handled with scepticism, Smith is also much concerned with defending that evidence. He assesses the general treatment of the literary evidence in the book too, which he claims is 'a gathering of true believers'. Those who are wary of the idea of belief, and also of belief in what the sources say may, however, find much here that justifies their doubts, and not just about the sources. Few individual Romans appear in the book (they are outnumbered by bar graphs alone). One who does is Ap. Claudius Caecus, the censor of 312 BCE. If the index is any indication, he is the most frequently mentioned Roman in the book (Sulla is a contender, but no page numbers are listed for him). Caecus has often been viewed as a pivotal figure for this period, and Smith refers to his reforms as a 'potential watershed moment'. But what did he do? The question is important, not only for that potential watershed moment, but also because of the quandaries with the sources for it, and the serious methodological questions they raise.

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