occasionally wondered about the wisdom of some of the terminology which M. deploys. While right to emphasize the importance of a more complex bureaucracy in allowing the business of government to continue irrespective of who was emperor, referring to this development as 'professionalization' (117, 306, 318), giving rise to a 'bureaucratic machine' (129), risks importing unhelpful anachronistic assumptions of efficiency into a context where emperors had an interest in encouraging a degree of duplication and conflict between administrative rôles (cf. C. M. Kelly in *CAH* XIII (1998), 169–71). I much preferred the less value-laden formulation of 'a self-sustaining entity' (117). Similarly, describing the strategies by which Stilicho secured his position as a 'system' (194, 250, 318) is in danger of suggesting too formalized an arrangement.

Of course, the eastern half of the Empire also had its child emperors during a substantial part of the same period, which would ideally form an integral part of an attempt to understand the phenomenon. However, the realities are that UK doctoral theses have relatively tight word limits and young would-be academics face considerable pressure to publish their first monograph without too lengthy a delay — circumstances which mean that M. has had to defer treatment of eastern child emperors to a follow-on project. This is unfortunate, but in the meantime a range of recent perspectives on one of their number can be found in C. M. Kelly (ed.), *Theodosius II: Rethinking the Roman Empire in Late Antiquity* (2013).

Overall, M. is to be congratulated on producing a fine, clearly-written study which significantly advances our understanding of the exercise of political power during an important phase of late Roman history.

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A. S. ESMONDE CLEARY, *THE ROMAN WEST, AD 200–500: AN ARCHAEOLOGICAL SURVEY*. Cambridge: Cambridge University Press, 2013. Pp. xiv + 533, illus. ISBN 9780521196499. £75.00/US$120.00.

Simon Esmonde Cleary offers us here a detailed archaeological survey, combined with an essay in periodization. The title of his book provides an entirely accurate description of its contents in two respects, but is inaccurate in a third. Firstly, this is indeed an explicitly archaeological survey that puts the material evidence to the fore and rejects the temptation to make this fit with what we know from textual sources; and, secondly, E.C. is firmly wedded to his date range, the whole of the third through the fifth century A.D. A major aim of his book is to suggest that these three centuries should be considered together as the 'late Roman' period, eschewing a traditional chronology that favours a break around the time of the political and military crises of the 250s to 270s. However, to entitle his book 'The Roman West' is inaccurate, since North Africa, Italy, and the western provinces of Illyricum are not treated at all, and Britain is only mentioned on occasion. 'The Roman North-West' would have been a more accurate title, since what E.C. covers (admirably) is Roman Spain and Gaul, from the Atlantic to the Rhine.

E.C.'s survey of the archaeological evidence is extensive and detailed, presenting a balanced and sensible overview of a mass of material, much of which (particularly that from Iberia) is unfamiliar in anglophone scholarship. The central chapters of the book are arranged thematically. Ch. 2 covers the military response to the breakdown of the frontiers in the third century: the building of fortresses, and, very importantly, the militarization of the aristocracy of north-eastern Gaul. Ch. 3 describes what happened to cities: some shrinking dramatically, like Arles; others showing little change, like St-Bertrand-de-Comminges; yet others booming in the fourth century, such as Trier; all, however, over time losing their traditional panoply of Roman secular monuments (fora, bath-buildings, amphitheatres etc.). Ch. 4 deals with the archaeology of religious change — above all the progressive build-up of Christian churches, but also the unexpected survival, and even renewal, of pagan shrines in the fourth century. Ch. 5 examines the archaeology of the élite, above all the imperial buildings of cities such as Trier and the richly decorated villas of the rich, known particularly from Aquitaine, but also from the Iberian peninsula, with spectacular (and enigmatic) examples at La Cercadilla and at Centcelles. In chs 6 and 7, E.C. examines the economic base of everything in the preceding chapters, looking first at rural settlement and production (ch. 6) and then at the manufacture and trade of other goods (ch. 7). Finally, in ch. 8 he sets out the

archaeological evidence for new peoples entering the Western Empire, above all Goths into Spain and Franks into Gaul. All of this is done thoroughly and well, with different interpretations presented and no attempt to force the evidence into too rigid a straightjacket.

These central seven thematic chapters are framed by three that are essentially chronological. In ch. 1, E.C. examines the archaeology of the third-century crisis, in particular to question conventional chronologies that attribute all destruction and all cultural change to the disastrous events of the 250s to 270s. He sensibly does not try to argue away all evidence of barbarian devastation, but 'a central contention of this book [is] that the threshold of "accelerated change" in the archaeological record lies not in the mid to late third century, but the better part of a hundred years earlier, from the later second century' (23). Then, towards the end of the book, in ch. 9, E. C. examines the archaeological evidence for the fifth century and argues that this century saw such marked changes (for instance in the abandonment of villas and networks of exchange) that by around A.D. 500 one has entered a different world, both in terms of the end of Roman systems and of the beginning of new non-Roman ones. Finally, in ch. 10, E.C. argues for seeing A.D. 200 to 500 as a coherent period that can reasonably be termed 'late Roman'.

Does his argument for an archaeological late Roman period from *c.* 200 to *c.* 500 work? Most archaeologists would probably agree with his end date, though personally I would push it back a bit, to around A.D. 450. Historians, using textual evidence, are prone to extend 'late antique' Gaul at least to the time of Gregory of Tours at the end of the sixth century, but the archaeological evidence, with which E.C. is working, unequivocally shows very substantial economic and social change by A.D. 500. I am less well qualified to assess his start date of A.D. 200, but E.C. himself is happy to admit that this 'threshold' (as he terms it) was a subtle one — when two centuries of dramatic development under the 'High Empire' began to stall and, in places, to fall into reverse. Certainly, E.C. is entirely convincing when he argues that events in the latter half of the third century cannot explain everything that changed in late Roman times; there are too many regional and chronological differences to support such a simplistic explanation.

This is a book aimed at scholars and students at graduate or advanced undergraduate level; it is too long and too detailed for a wider readership. I suspect it will be consulted primarily in bits for its very useful, and up-to-date, syntheses of the state of scholarship — for instance, in ch. 8, E.C. is admirably full, clear and balanced on the complex issue of whether or not one can equate grave goods and ethnicity. But his book also deserves to be taken seriously for its discussion of periodization. A great deal of ink has been consumed considering how late in time one should extend 'Late Antiquity'; but comparatively little scholarship has been dedicated to examining this fashionable period's origins. E.C.'s book is a serious attempt to do just that, through painstaking analysis of the archaeology of Roman Spain and Gaul.

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F. RIESS, *NARBONNE AND ITS TERRITORY IN LATE ANTIQUITY: FROM THE VISIGOTHS TO THE ARABS*. Farnham/Burlington, VT: Ashgate Publishing, 2013. Pp. xvi + 288, maps, pls. ISBN 9781409455349 (bound); 9781472408273 (e-book). £75.00.

Frank Riess writes with great knowledge, engagement and passion, and offers much to be grateful for, for example in his study of the geology and hydrography of the port of Narbonne (19–32). However, there is also much to take issue with. His title is deceptive. This is not a standard regional survey, but the presentation of a particular historical argument: that the stormy relationship between Narbonne and the Visigothic kingdom of Spain, with its capital at Toledo, during the sixth and seventh centuries A.D. resulted from the city's strong self-identity which went back to its foundation as a Roman colony in 118 B.C. This feeling of difference found ultimate expression in Paulus' short-lived secessionist 'Eastern Kingdom' of 673 (189–90). R. complains bitterly that Narbonese separatism has been neglected by Spanish, French and Catalan historians, happy to adopt the 'Toledo-centred' (133) bias of the extant Visigothic texts, in order to present the inexorable rise of their own, exclusive, national identities.

So far, so good. Separatist feelings have been identified in other periods and areas of Gallo-French history, so it is not impossible that they existed in the case of Narbonne. Problems arise out of R.'s