Some methodological questions can be raised. Is the focus of the study too wide? Are the criteria for the inclusion of material rigorous enough? Although R. points to changes over time (e.g. p. 226), is it a good idea to treat the fifth and fourth centuries as a block, and thus reach general conclusions about classical Greek theory which cover Greek *poleis* from before the Peloponnesian War and Alexander's Macedonians? Should evidence for a practice be drawn indiscriminately from sieges, naval war, and open battle (e.g. p. 5 n. 6)? The dynamics of siege warfare demanded information (the technology of the defences, supplies, and motivation of the defenders) in ways open battle did not, especially the ritualized complicity of a hoplite battle (W. R. Connor, 'Early Greek Land Warfare as Symbolic Expression', *Past and Present* 119 [1988], 3–29).

Taking a construct from one culture and looking for it in another can be fruitful. New patterns can emerge as sources are read in new ways. But it can also lead to sources being read in odd and predetermined ways, giving a false centrality to the construct. R. is aware of the dangers (e.g. p. 5), but might be considered to sometimes fail to avoid them. To say that Xenophon 'alone went so far as to study the gathering and evaluating of information as a field in its own right' (p. 6) gives a strange impression of the contents of the *Cavalry Commander*, let alone the *Education of Cyrus* and the *Memorabilia*. R.'s admirable knowledge of modern intelligence gathering seems at times to induce him to systematize the *ad hoc* arrangements of the Greeks. For example, he claims that 'reconnaissance units in interpolis warfare were relatively small and did not engage the enemy. They typically numbered two or three for covert operations, around thirty for general duties' (pp. 17–18). The evidence produced for these 'typical' figures turns out to consist of one, perhaps two historical examples from the period for 'thirty', and none at all for 'two or three' (p. 18 n. 26; references for the latter consist of Homer, Plutarch's *Aratus*, and Arrian's *Against the Alans*).

The reader might have gained a clearer picture had a sharper distinction been drawn between information gathering from other Greeks and from other peoples. Alexander wanted to know about the nature of Scythian territory, their numbers, arms, and customs (p. 127). How relevant would any of this be when, say, the Eleans fought the Arcadians (Xen. *Hell.* 7.4.28–32)?

A more certain judgement on the importance of information gathering for Greek states might have been obtained if R. had used his skills in comparative history to produce a systematic comparison of Greek practice and theory with those of another pre-industrial polity, possibly Rome (R. uses N. J. E. Austin, N. B. Rankov, *Exploratio* [London and New York, 1995] here and there) or traditional China (for which R. D. Sawyer, *The Tao of Spycraft* [1999] is now available).

This is a scholarly and thought-provoking work, but whether its revisionist line will win general acceptance remains to be seen. Some might still prefer to regard as typical the attitude to intelligence exhibited by the Thirty at Athens in the events which led up to their overthrow (Xen. *Hell.* 2.4.1–7; cf. R. p. 213). The Thirty failed to notice, or failed to do anything about the muster of armed Athenian exiles in Boeotia, even though the Thebans had passed a decree of clear relevance to the position (Dinarchus, *Against Demosthenes* 25; Plutarch, *Pelopidas* 6.4). Although the area was notorious as the venue for raids from Boeotia into Attika (Aristophanes, *Acharnians* 1023), the exiles were able to occupy the fort at Phylae. The first expedition sent by the Thirty in midwinter obviously lacked equipment for inclement weather, and had to return when it snowed. The second, sent out with the express purpose of watching the exiles, was surprised and routed by a dawn attack on its camp.

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J. BLEICKEN: *Cicero und die Ritter.* (Abhandlungen der Akademie der Wissenschaften in Göttingen, philologisch-historische Klasse, dritte Folge, 213.) Pp. 128. Göttingen: Vandenhoeck & Ruprecht, 1995. Paper, DM 42. ISBN: 3-525-82602-8.

The aim of this book is to use the evidence of Cicero to refute the view—chiefly associated with the name of Claude Nicolet—that the definition of an *eques Romanus* was that he was (or had been) mounted on a horse at public expense, *eques equo publico*: (p. 72) 'it is highly likely that C. Gracchus . . . equated membership of the equestrian *ordo* only with wealth'. B. makes a number of powerful points: (p. 43) that Cicero has almost nothing to say of the centuries of *equites equo publico*; (p. 49 n. 87) that the usage of Cicero himself implies that in the period before he became a senator he was not an *eques* in the sense that Nicolet defines. A long discussion of the Roman concept of an *ordo* rightly emphasizes the importance to it in the age of Cicero of a certain kind of activity, almost 'career'. B. is in my view right to argue that, for Cicero, at any rate on most days, an *eques* was anyone with a property of over 400,000 sesterces.

But B. does insufficient justice to Nicolet's view that the nature of the *equester ordo* was such that it could only be constituted by the censors; and it was already an essential part of Nicolet's case that the failure to complete a census after 70 B.C. in large measure explains the problems of definition in the period from which most of our evidence comes.

There are also problems with B.'s handling of the sources: first, the doubts hanging over the *Commentariolum petitionis* mean that no argument should depend on it (*contra* pp. 6, 43–4, 49); and the well-known ancient commentary on the Verrines is not by Asconius (*contra* p. 13); (p. 71) the Livian accounts of corruption in 213 B.C. and of the closure of the Macedonian mines after 167 B.C. are certainly filtered through the experiences of the late Republic; and the gold ring occurs in Cicero only at *Rosc. Am.* 144 (not cited by B.), where it is taken for granted that Sex. Roscius should have it, and in the Verrines, where Verres is accused of granting it improperly.

There are also problems of more fundamental kinds. Notoriously, at both the points in the Gracchan *lex repetundarum* at which the definition of a man eligible for jury service, an *eques*, is precisely defined, there is a lacuna in the text; it is, however, possible to go beyond what is said in the commentary in *Roman Statutes* (London, 1996), where the authors were content to cite the two supplements offered by Mommsen as exemplifying the two possibilities, the *equus publicus* or a census of 400,000 sesterces: *if* the definition was in terms of the census, there must also have been a specification that the man be a full citizen, to exclude *ciues sine suffragio*, something like *qui h[ac ceiuitate ceiuis optimo iure erit quique apud censorem HS CCCC professus erit d]um*. . . . But we now know within limits the length of the lacuna and there is simply not space; whereas *qui h[ac ceiuitate equo publico stipendia fecit fecerit d]um* . . . is the right length. (On the evidence of Pliny for this period, see also the article of Jean-Louis Ferrary cited below.)

Secondly, at pp. 83–4, B. rightly observes the at first sight surprising fact that *equites* could be compelled to serve as jurors: in the Gracchan *lex repetundarum*, the praetor simply chooses; and the practice is assumed by Cicero's argument at *Clu.* 148–60 and *Rab. Post*. 16–19. But B. fails to distinguish between what the majority of *equites* might want, namely control of or a share in the jury-courts, and the reluctance of an individual when actually forced to serve to run the risk of being punished for misconduct; or to appreciate the nature of Cicero's argument, shedding crocodile tears for the position of *equites vis-à-vis* senators, which grasps at any straw to justify the ludicrous proposition that citizens compelled to serve should by virtue of that fact not be liable to punishment for misdemeanours—it would be interesting to know what Cicero would have said of the proposition that a soldier should not be punished for desertion because he had been conscripted. It is significant that Cicero attempts to make the argument in two speeches where he perhaps more flagrantly than anywhere else seeks to defend the indefensible, in the *Pro Cluentio* on his own admission, in the *Pro Rabirio Postumo* in the judgement of B. and everyone else. It remains more probable that jury service was imposed on *equites equo publico* than on those with only the equestrian census.

Thirdly, the fact that men known as *tribuni aerarii* served as jurors after 70 B.C.: it is hard to identify these as anything other than in origin a group concerned with army finance, which again makes it more likely that the *equites* who served on juries were not simply a property class. B. never faces this issue: it is true that we can say almost nothing useful about the group; but it must have been definable in terms other than that of a census level for the purposes of Roman legislation; so also the 'equestrian' element of juries after 70 B.C.

But, as B. says, what he is investigating is not 'wie es eigentlich gewesen ist', not even what Cicero thought, but what Cicero wanted his audience to think that he thought (pp. 60ff., 103ff.). B. is perhaps, in dealing with different occasions, not sufficiently aware of the different expectations of different audiences; and it is precisely that dialectical relationship between speaker and audience that should have prevented him from saying (p. 106) that the essence of politics was personal relationships.

So why is it, as B. rightly says, that for Cicero the *equites* are essentially the *publicani*? (B. sensibly observes that one should not characterize Cicero as the 'spokesman' of the *equites.*) B. suggests, following Ferrary, that it is partly because the rôle of the *equites* in the jury-courts was, between Sulla and 70 B.C., non-existent and even after 70 B.C. limited, a fact reflected in the history of the *equites* offered by Pliny (*REL* 58 [1980], 313–37, 'Pline, *NH* XXXIII, 34, et les chevaliers romains sous la République'; it remains the case that Pliny offers a pretty idiosyncratic account: see P. A. Brunt, *The Fall of the Roman Republic* [1988], p. 515). It must also be the case, as Ferrary further observed, that the inclusion by Sulla of large numbers of individual *equites* in the senate gave prominence to the activities as *publicani* of those who remained. (It is a corollary of B.'s argument that senators were not heavily involved in investment in the activities of *publicani*, a view to which I am sympathetic.) But there is perhaps another factor: everything we know about Republican legislation suggests that the *Lex* of 67 B.C., reserving the front fourteen rows of the theatre for *equites*, will have been long and complex, whereas what we know of it consists of a few sentences. Who were the *equites* envisaged by the statute? In other words, was it expected that fourteen rows would provide for the likely take-up among *equites equo publico*, or among those with the equestrian census, as some of our sources state?

But the statute must in any case have played a major part in everyone's perceptions of what an *eques* was, leading to a further muddying of the waters (its importance was underlined by J. Linderski, *CP* 72 [1977], 55–60, reviewing Nicolet): was an *eques* a man with the public horse, a man with a certain level of wealth, or a man who sat in the fourteen rows?

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S. E. ALCOCK (ed.): *The Early Roman Empire in the East.* (Oxbow Monograph 95.) Pp. viii + 212. Oxford: Oxbow Books, 1997. Paper, £24. ISBN: 1-900188-52-X.

This is the twin to the very useful *The Early Roman Empire in the West* (T. Blagg and M. Millett 1990) and displays similar strengths and weaknesses, mostly beyond the control of any editor.

A concise preface explains the process of selection: contributions encompassing the entire range of evidence—not least archaeological, geographical coverage and topics of significance. Then follow three sections and an afterword. 'Urban Structures' has essays by Woolf on Roman urbanization in the East and one on Roman colonies in Achaia by Rizakis. Next four essays focus on 'Regional Studies'. Gawlikowski surveys the Syrian Desert, Tate looks more closely at the countryside of Syria, Hirschfeld tackles Jewish rural settlement in Judaea, and Potts goes beyond the frontier to Roman relations with the Persian Gulf and the rise of Spasinou Charax. In the final section, 'Images and Identities', Rose explores imperial imagery, Braund has an essay on Greeks, barbarians, and Hellenism around the Black Sea, Cormack one on funerary monuments in Asia Minor; Schmidt-Colinet looks at Romanization in the funerary architecture and decoration at Palmyra, and Elsner cultural resistance as expressed in pilgrimage, religion, and visual culture. Finally, Millett offers his Western perspective on the East.

Like its twin, the book contains a number of excellent essays, some of which will be explored a little more fully below. As Alcock acknowledges, however, the selection has had to omit a great deal. 'East' for her means the Greek East from the eastern Balkans. However, just as the western volume had nothing specific on North Africa, so too this one is silent on the same area; the former had nothing on the Danubian provinces (or Italy and the major western islands), while this one has nothing on Cyprus or Arabia.

Alcock notes that such volumes underscore the divide in research on the Roman Empire and looks forward to the day when books like this will bring the two parts together. That seems unlikely. Just as 70 years ago Collingwood found the sheer quantity of literature on Roman Britain had made the subject difficult of access even for the specialist, so too now the Roman East. Though a poor relation in terms of research, the East has yet been the subject of huge numbers of publications in the past generation alone. Even books like Fergus Millar's *The Roman Near East, 31 BC—AD 337* (Cambridge, MA, 1993), concerned with only a part of the East, had to focus on just the early period and exclude most archaeological evidence, and it still ran to almost 600 pages. Like the later Roman Empire, research seems fated to spawn manageable chunks—individual scholars mastering the evidence for a specific province or group (the Near East, Asia Minor, Egypt). But again like the Empire, these must also be held together—with great difficulty to be sure—by works such as those of Sartre (1991) encompassing the entire Greek East (and now his *D'Alexandre à Zénobie* [Paris, 2001]), by Mitchell on the Anatolian provinces (1993), Millar (1993) and now W. Ball, *Rome in the East* (London, 2000), on the written and archaeological evidence respectively for the 'Near East' (= Greater Syria), and these collections by Blagg and Millett, and Alcock. In short, just as we must all be grateful to those able and willing to synthesize the evidence for the Empire as a whole or large parts of it, so, too, the laborious task of producing edited works has an important rôle to play and needs regular repetition. It is then