applications of the Keegan method to antiquity have so far dealt not with individual battles, but more broadly with the warfare of particular societies, for example Hanson's work on Greek hoplite warfare or Goldsworthy's own *The Roman Army at War, 100 BC–AD 200* (1996). The two works under review both apply the method to a single conflict, perhaps the most celebrated and discussed of all ancient battles. Though there is considerable agreement between them, each makes a distinctive contribution, and both are fine demonstrations of how much can be achieved by applying the Keegan model.

Polybius provides almost all of our usable information on the Battle of Cannae, since most of the variant material in Livy and later writers is clearly unreliable. Polybius' narrative has evident limitations (well brought out by Daly, 18ff.) — political prejudices, a tendency to schematize and simplify, emphasis on Cannae as the nadir of Roman fortunes, which perhaps accounts for the exaggerated casualty figures. Both D. and G. draw extensively on the warfare of Rome and other societies to build up a much richer account than Polybius provides of what the battle may have been like.

G.'s work, in Cassell's 'Fields of Battle' series, is fluently written and excellently presented and illustrated (though the river Aufidus is consistently misspelt 'Aufidius'). G. organizes his material in a broadly chronological fashion, but D., whose careful discussion is a revised version of his Dublin doctoral dissertation, adopts a multi-faceted approach. His opening chapters sketch the events leading up to Cannae and concisely survey the traditional issues of scholarly debate about the battle; the next two chapters examine the Roman and Carthaginian armies of the period; and in the final two chapters D. turns to reconstructing the experience of battle, first for the commanders and then for the soldiers. While D.'s work is in some respects more wide-ranging than G.'s, its organization has perhaps led him to be somewhat too succinct in his treatment of some topics, for example the commanders' battle plans. Here G. is fuller (102–13), with perceptive remarks on how the Roman commanders planned to win the battle and how far Hannibal may have modified his plan in response to the Romans' troop deployments. Both D. and G. recognize that the battle must have taken place on the right bank of the Aufidus and that the course of the river has changed over time. G. (86–93) argues powerfully for Connolly's view that in 216 B.C. the river ran some way to the west of its present course and the battle took place between the river and the Cannae ridge: as he points out, this site would have had the advantage for the Romans of providing natural protection for each flank. It would, however, be cramped, and D. (32–5) may be right to opt for Kromayer's location on the slope east of Cannae hill, with the river on approximately its present-day course.

Troop numbers and deployment must remain the nub of any reconstruction of the battle. Like most recent enquirers, D. and G. both accept Polybius' claim that the Romans fielded eight legions, rejecting the variant four-legion tradition reported by Livy and followed by De Sanctis and Brunt, above all because they believe that the Romans must have sought numerical superiority in infantry after their earlier defeats. Both explain Polybius' statements on the Carthaginian infantry deployment by supposing that the Libyans on the flanks were deployed in column (perhaps concealed, as G. suggests (111)) and that, despite their great superiority in numbers, the width of the Roman infantry line was no greater than that of Hannibal's Gallic and Spanish infantry. D. (157–66) conjectures that each line was 840 men wide, with the Romans 58 men deep and even the comparatively thinly deployed Gauls and Spanish an average of 26 men deep, while G. (99) thinks that the Romans may have been deployed as many as 74 ranks deep. Such reconstructions may be the best way of making sense of Polybius' account. However, in some respects they go beyond what he says (e.g. the postulated Libyan columns), and one may wonder about the plausibility of such extraordinarily compact deployments. Admirable as these attempts to recover the combatants' experience are, they continue to depend on assuming the essential accuracy of Polybius' schematic account.

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D. HOYOS, *HANNIBAL'S DYNASTY: POWER AND POLITICS IN THE WESTERN MEDITERRANEAN, 247–183 B.C.* London: Routledge, 2003. Pp. vii + 304, illus. ISBN 0–415–29911–x. £50.00/US$96.95/€75.00.

The latest book concerning the life of Rome's great adversary Hannibal, by Dexter Hoyos, was highly anticipated for a number of reasons. It was primarily awaited as a follow-up to the author's 1998 work, *Unplanned Wars*, that breathed life into old, yet still relevant, scholarly

debates and postulated several new and provocative theories concerning the first two wars between Rome and Carthage. Moreover, *Hannibal's Dynasty* was anticipated by mid-Republican and military scholars alike because of its title; the book purports to be an analysis not just of Hannibal himself, but of his family, the Barcids, and their role in the power politics of the Western Mediterranean from the mid-third century B.C. onwards. The work presents a solid narrative concerning the history of Carthage beginning with the city's mythical founding in 814 B.C. and a brother of Dido's named Barca (21; Sil. 1.71–6; 15.745–8). This, however, is done with rapidity and the main narrative begins with the Truceless War of 241–237 B.C., and the majority of the work focuses on Hannibal and his exploits in the war against Rome. Here H. enters into some interesting discussions concerning politics at Carthage during the Second Punic War, and the Punic government's relationship with Hannibal while he was on campaign in Italy. These sections add substantially to many ongoing debates concerning the nature of the government and the internal politics of Carthage, and the Punic assembly's relationships with their generals.

The author interpolates a generous amount of source criticism into his work, examining the literary evidence, namely Polybios and Livy, in depth, and scrutinizing their accuracy on individual points concerning Hannibal and his predecessors. This type of analysis makes the work a worthy contribution to scholarship and a necessity for any researcher concerned with the period, as this source-based approach serves to bring forth several new historical interpretations. Nevertheless, *Hannibal's Dynasty* is nearly all narrative, and, despite its title and subtitle, the vast majority of the book deals only with Hannibal himself and his involvement in the Second Punic War.

An analysis of the relationship between the Barcid family and Spain is, at present, very necessary. While H.'s work goes part of the way towards this, its focus on narrative leaves several questions tantalizingly unanswered. Could Hannibal himself, a man who spent part of his childhood, all of his adolescent life, and over half of his adult life outside of his birthplace, even be considered a Carthaginian? He, of course, had lived for nearly two decades amongst the Spanish prior to setting out for Italy in 218 B.C. This is especially relevant considering how modern narrative histories of the Second Punic War often marvel over the fact that Hannibal was able to win all of his victories with such an ethnically diverse army. Too often his Spanish roots are forgotten in this context. His army, while certainly diverse, had at its core a very solid contingent of Iberians. Surely Spanish dialects as well as the culture of various Iberian peoples were second nature to him by that time. Questions therefore need to be addressed concerning the extent to which Punic lands in Spain constituted a Carthaginian Empire, as in reality they appear more to be an independent Barcid state that had less and less to do with Carthage over the years. To go even further, it could be postulated that the Iberian territories were hardly Punic at all, as by the late 220s B.C. they were ruled by men who had become Spaniards of Punic origin.

H.'s text, in this regard, exposes the need for an in-depth analysis of Hannibal's relations with the Iberian tribal aristocracies, as well as those with his fellow Carthaginians in Spain. Barcid lands in Spain could have been ruled by a Punic caste, but given the level of co-operation Hannibal received from many Spanish tribes this seems unlikely. Again, H. has provided us with a fine account of the life of Hannibal and analysis of the sources for the general's life, but it is a shame that the author stopped short of tackling so many pertinent questions facing Punic and mid-Republican scholars today.

The chapters concerning Carthage and Hannibal cause *Hannibal's Dynasty* to lean towards a specialist audience, as does the complete lack of maps in a work that mentions many places that would be obscure even to graduate students studying the third-century Western Mediterranean. Moreover, the amalgamation of straightforward narrative and specialist discussions is often not a particularly good fit. The narrative sections offer little to the scholarly reader, while the work's research pieces are often too complex for a more general audience. What this does however, is make the work ideal for the smaller market of advanced undergraduate students and entry-level postgraduates. As to the non-specialist sections, while there is certainly nothing whatsoever wrong with narrative books — they are, indeed, a very necessary part of historical research — we must ask, is another biographical account of Hannibal and the war with Rome really necessary? There are already over two dozen biographies of the Carthaginian general, with five in the last decade alone, of which the most notable is Lancel's, translated into English by Antonia Nevill in 1998. And, while H. does expand on many of Lancel's arguments, *Hannibal's Dynasty* does not go far enough to really supercede Lancel in importance to scholarship. Furthermore, while the

analysis of the Second Punic War is well-done in H.'s volume, it does not match Lazenby's *Hannibal's War* (1978) in terms of depth or comprehensiveness. It is therefore difficult to see where this work will fit into modern scholarship; certainly it is a necessary read for military and political scholars of both Carthage and mid-Republican Rome, but it is unlikely to make a substantial impact upon any scholarly debates beyond individual points of source criticism and discussions of the nature and structure of the government of Carthage.

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R. MORSTEIN-MARX, *MASS ORATORY AND POLITICAL POWER IN THE LATE ROMAN REPUBLIC*. Cambridge: Cambridge University Press, 2004. Pp. xiv + 313. ISBN 0–521–82327–7. £50.00.

After decades of relative neglect the Roman *contio* has now become the focus of an intense debate about the people's role in Republican politics. This new study by Morstein-Marx represents the fullest and most ambitious treatment of the institution so far. It is a provocative and stimulating work which offers important new insights into the nature of Roman politics. The scholarship is impressive and the analyses often profound. Divided into eight chapters the book covers a wide range of aspects: the location and physical context of *contiones*, their audiences and role in the political process, the formal structure of the meetings, the debates, and their ideological underpinning.

M.-M.'s vision of Roman politics does not lend itself easily to a brief summary. That is partly due to the complexity of his argument, which is rich in nuances and discursive detail, but partly also to the book's attempts to integrate two contrasting, some might say irreconcilable, models of Roman politics. The first half of the book deals with the mechanics of *contiones*, their structure, audiences, and political impact. The *contio* is here identified as the focal point of the entire political process, where vital communications between leaders and masses took place. In these chapters M.-M. comes very close to a democratic reading of Roman politics. The people are presented as a central player in the decision-making process, whose responses would determine the success or failure of proposals. The second half analyses the ideological 'construction' of the *contio*, the speeches and the debates. Here M.-M. argues that the élite's monopoly over the communication meant that the people were easily (mis)led and the *contio* therefore became an instrument of control rather than a vehicle for popular power. Thus, the popular influence, argued so strongly in the first half, turns out to be illusory.

Readers may wonder about the genesis of this paradox, and, despite the author's insistence on the unity of his thesis, the various parts struggle to form a fully integrated whole. The book seems to offer two distinct visions of Roman politics, and to this reviewer the second one remains the more persuasive. M.-M.'s analysis of the *contio* as an ideological construct is perceptive and illuminating. It goes beneath the surface and shows the split between ideal and practice. Public debates emerge as spectacles rather than free exchanges of views informing an audience. The symbolic importance of the *contio* meant that any crowd 'impersonated the *populus Romanus*' and any speaker addressing this crowd by definition became a *popularis* and presented his views in this ideological mould. The result was an 'ideological monotony', which reinforced the élite's domination of the public discourse.

The 'democratic' argument of the first half rests on the premise that the *contio* was a communication between leaders and masses, in which the 'Popular Will' manifested itself. Despite the fact that only a small fraction of the *plebs* could ever be present, M.-M. argues that the audience formed a representative cross-section of the urban populace, and that their experiences at the *contiones* would feed directly into the masses and shape their views on current issues. How this worked in practice is not entirely clear, and it is worth reminding ourselves that we have no reliable information about the composition of the audiences; as M.-M. convincingly shows, any contional crowd was automatically 'the *populus*'.

M.-M. suggests that *contiones* typically were dominated by shopkeepers from around the Forum. That raises obvious economic issues, and we might wonder what happened to the regular Forum crowd — the senior senators with their entourages, the candidates solliciting for support, the *boni* and *equites* doing business, shopping, and networking. Did these people, who had time, resources, and a direct stake in Roman politics, vanish from the Forum whenever a *contio* was called? According to M.-M. the audiences were variable and self-selecting, but many issues would have been of marginal interest to the working masses — how many *tabernarii* were so concerned