De mundo (pp. 161–8). The fifth and final chapter (pp. 169–89) is devoted largely to the Elder Pliny (pp. 172–89), who adopted an encyclopedic approach to meteorology.

Parts of chapters betray signs of having evolved from lectures. Otherwise, it is difficult to account for so many repetitions that seem unnecessary on the printed page. For instance, to single out just a few of many examples, do we need to be told twice in the space of less than five pages (pp. 47, 51) that Cicero and Germanicus Caesar translated Aratus—each time as if it were new information—or on facing pages (pp. 82, 83) that in the *Meteorology* Aristotle confines his discussion to material and efficient (not final) causes? Occasionally repetition takes the form of presenting more than one English translation of an ancient text, one version in the body of the work and another in an endnote. While T. has good reason to give us George Goold's modern prose translation of Manilius 2.60–76 to help clarify the 1697 verse translation by Creech (pp. 138, 220), one has to wonder why we need two translations of a straightforward passage of Lucretius (6.703–11: pp. 125, 138). By contrast, the repetition in Chapter 5 of material previously covered in Chapter 2 (*parapegmata* and wind-roses) is easier to justify since Chapter 5 is thereby made more or less free-standing. Similar judicious repetition in other chapters permits the reader to plunge into any aspect of the subject that is of immediate interest.

Gaps in T.'s knowledge of her subject are few. She does, however, seem to know John Lydus only at second hand through the summary of Bandy (p. 204 n. 212), and so she fails to convey the richness of that source on the subject of how comets supposedly affect weather. To give one other example, T. cites (p. 57) a stale note in Fairclough's 1916 Loeb edition of Virgil's *Georgics* as her sole piece of modern scholarship on the unusual weather conditions that seem to have prevailed in 44 B.C., the year of Caesar's murder. For a thorough study of the evidence, see Ramsey-Licht, *Comet of 44 BC* (Atlanta, 1997), pp. 99–107.

The book is beautifully produced on fine paper, with fifteen back-and-white illustrations and diagrams, appropriately captioned. It is a pity, however, that the nearly forty pages of endnotes are not equipped with running heads having the form 'Notes to Pages 00–00', but this aid to the reader is lacking in most Routledge books (though not all, e.g. *Rhetoric at Rome* by M. L. Clarke). Typos are few (e.g. p. 195 n. 30 read 'in' for 'on'). In sum, this is a splendid collection of material, most of it not new or original, but usefully collected in one place where the reader can gain insight into a fascinating and important subject.

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A VEGETABLE PASSION

J. HENDERSON: *The Roman Book of Gardening*. Pp. x + 152. London and New York: Routledge, 2004. Paper, £17.99. ISBN: 0-415-32450-5 (0-415-32449-1 hbk).

Scribonia Attice cannot have had any time for gardening. I suspect that that this wealthy resident of Portus or Ostia (interred at Isola Sacra) was far too busy practicing her lucrative profession as a midwife. Nor could her husband, M. Ulpius Amerimnus, have had much time for gardening either. He was a physician and

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perhaps a part-time trader. No doubt this wealthy pair of busy freed folk had a nice garden at home, but that was surely tended by a slave. Perhaps the expensive family tomb that Scribonia Attice has left for us to admire (Isola Sacra tomb 100—there is here a titulus inscription and two reliefs depicting Scribonia's profession) also had a garden. But this would have been tended and protected by a resident slave, not by this medical couple. I doubt that Scribonia or her husband would have done anything more than admire her gardens. Gardening was a banausic task. Romans like Scribonia—Romans in general—simply did not do it. Their leisure would have been spent in other ways. Scribonia would have had little use, I suspect, for *The Roman Book of Gardening*, which she would have seen as a primer for slaves. Herein resides the puzzle of H.'s book on Roman gardening. It is written for gardening enthusiasts (those who do it themselves, Roman slave-like, 'smearing dung on seeds, dangling cucumbers, shaking out caterpillars', p. 27), but about a people who compelled others to do it for them. How does *The Roman Book of Gardening* reconcile the slave-owning Scribonia Attices with the gardening John Hendersons?

Before I attempt to answer that question, here is an outline of the book's contents. H.'s anthology provides translations for a series of unleisurely texts. The translated material included is as follows: 'Introduction: Reserving a Plot: from Virgil, Georgic Four [116–48]', followed by (Chapter 1) 'Produce in Prose: from Columella 11', (Chapter 2) 'Flowery Verse: Columella 10', (Chapter 3) 'Nature's Miracles in Pliny's Encyclopaedia', and (Chapter 4) 'A Year in the Garden with Palladius'. All of the translations are produced with interpretative or exegetical notes. The book begins with a long preface ('Preparing the Ground') which outlines H.'s understanding of the significance of the various texts. The book concludes with notes, further reading, a date chart, and three indexes (with plant names, English and Latin). As is evident, the book mixes genres (didactic poetry in Virgil and Columella 10 with encyclopedic writing in Pliny), modes (prose and poetry), periods (late Republic and late antiquity), gardening types (flowers and vegetables), farming and gardening (Palladius and Columella 10), and makes no attempt to periodize. His justification for this *mélange*, as we shall see, is theoretical. H.'s collection is a fascinating one and, like all else that he writes, stimulates and irritates in equal measure.

What is the answer to the puzzle presented by the first paragraph? (H. is perfectly well aware that Scribonia Attice had her gardening done for her.) A clue is to hand in the summation of his discussion of Columella. H., placing great stress on the prose book of gardening instruction for the manager of Columella's Book 11, ingeniously argues that the previous verse Book 10 of Columella becomes, as it were, the flowering of the practical advice of Books 11 (and 12). The poetic artifact of Book 10 thus exemplifies the process it describes. H.'s anthology then bears an analogous relation to Roman gardening just as does the verse Columella 10 to the prose Columella 11. So H. self-referentially tells us (p. 12): 'So **Columella *Ten*** will grow plenty of explanatory notes [sc. H.'s text—see also postscript of p. 27: 'see how *The Roman Book of Gardening* . . . grows on you']. It will be a good idea for me to give an "advance" on the translation in store.' Thus the justification for the introductory photograph of the West Sussex potting shed and the various 'Hendersonian' quirks of language and arrangement in the translation. These all highlight the process of *The Roman Book of Gardening* rather than its apparent object of contemplation, gardening (and this goes a long way towards justifying the fact that H.'s voice tends to flatten the Latin). There, then, is the answer to the conundrum of my first paragraph. *The Roman Book of Gardening* is more interested in itself than in Roman gardening. This book therefore will no doubt enlighten and amuse Latinists of a deconstructive turn. How the

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modern-day Scribonia Attices of West Sussex (or the Roman ones of Ostia) will feel is harder to decide.

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THE BEGINNINGS OF CAVALRY

R. DREWS: *Early Riders. The Beginnings of Mounted Warfare in Asia and Europe*. Pp. xi + 218, maps, ills. New York and London: Routledge, 2004. Cased, £55. ISBN: 0-415-32624-9.

In this book Robert Drews takes four-square aim at the theories of Gimbutas and others that *c*. 4000 B.C. people on the Eurasian steppes began not only to ride on horses, but also to fight from horseback. D. opens his book with the statement that some will view his thesis (that mounted warriors did not become significant until the eighth century B.C.) as unlikely, whereas others will consider it self-evident and wonder why such a book as his should even have been necessary. I confess that I stand in the latter camp; but no matter: even self-evident truths bear occasional repeating.

That said, the earliest secure attestations of riders on the Eurasian steppes come from the late second millennium B.C.; such attestations, however, are legion in the first, as are references to mounted warriors. (D. dispatches [pp. 12–15] alleged earlier attestations, in particular the horse burial at Dereivka, now redated to the first millennium B.C.)

Extrapolating from known circumstances to unknown ones has its pitfalls. But positing that in unknown circumstances things develop *differently* from what one observes happening in known ones verges on lunacy. Point for point, the proponents of mounted warriors on the steppes in the fourth millennium B.C., however, do exactly that.

First, in the Near East and the Aegean region we find no attestation of anyone riding before the second millennium B.C.; and not until the first (or very late second) do we find anyone riding in a non-awkward fashion (e.g. not in a 'donkey-seat'—see e.g. Fig. 3.8 on p. 43). Secondly, in these regions cavalry does not appear until the opening centuries of the first millennium: i.e. mounted warriors were a late development which lagged far behind the advent of riding *per se*. Thirdly, comparable technological advances, such as the wheeled waggon, the spoked wheel, or the metal bit, on the basis of available evidence, seem to have appeared with rough contemporaneity both in the Near East and on the Eurasian steppes—i.e. wherever precisely such advances may have originated, they spread rapidly, and for the obvious reason that they conferred immediately apparent benefits on those who adopted them. Why, then, should we assume that in the case of riding and mounted warriors these technological advances appeared on the steppes two and three millennia respectively before we see them in the Near East—and not with rough contemporaneity in both regions as in the case of other comparable advances? Then, in the Near East, where we have abundant evidence, we see a long delay between the appearance of riding *per se* (however awkward) and the appearance of mounted warriors. Why, then, should we posit that riding and mounted warriors appeared simultaneously around 4000 B.C. on the steppes in a period for which we have no evidence?

In point of fact, what evidence we do have from the steppes (earliest attested rider from the late second millennium; and cavalry attested only in the first) actually

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