4961 (223) Bestätigte Kopie einer Bittschrift an den Präfekten, ed. J. David Thomas.

Es folgen die üblichen Indices. – Leider ist die farbliche Wiedergabe der Papyri aufgegeben.

Halle (Saale)        Wolfgang Luppe


This is the third and final volume of Enrico Flores’ critical edition of Lucretius’ De Rerum Natura, following the earlier instalments of Books 1–3 (2002) and 4 (2005). It contains a brief introduction (11–19), a supplementary bibliography (20–5), a summary of manuscript sigla (27–9), addenda and corrigenda to earlier volumes (31), followed by a Latin text of Books 5–6 with a facing Italian translation (32–249). As the edition’s closing volume, it ends with a survey of the so-called Lucretian fragments (251–3) and the capitula that divide the transmitted text of the poem (253–66). Despite the lapse of seven years since the first volume, and the major advances in research that have been made since Flores (F.) began his work on Lucretius in the 1960s, the edition continues firmly on the principles adopted by the editor over thirty years ago. As a result, although this edition of the last two books of De Rerum Natura succeeds in recording many previously unpublished readings and emendations from Renaissance mss, it suffers from the same methodological defects evidenced in earlier volumes and can only be used with great caution.

The dust-jacket continues to display the contentious and misleading advertisement that this is the «prima edizione critica con un apparato esaustivo che dia integralmente conto della tradizione manoscritta umanistica, a partire dalla ricostruzione della copia, commissionata da Poggio Bracciolini nel 1417 ad un amanuense tedesco, del codice da lui scoperto nell’abbazia di Murbach e, probabilmente, di VIII secolo.» Unfortunately, F.’s introduction does not revisit in any detail the major questions of the interrelationship between Lucretian mss. One must presume, therefore, that F. continues to posit four branches to the Lucretian tradition: 1) the ninth-century Codex Oblongus (O, Leiden Voss. Lat. F 30); 2) the branch witnessed by the ninth-century Codex Quadratus (Q, Leiden Voss. Lat. Q 94) and

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the three fragmentary Schedae Haunienses et Vindobonenses (G, Copenhagen Gl. Kgl. S. 211 2, and VU, Vienna Lat. 107 ff. 9–18); 3) a reconstructed manuscript that he terms the Codex Murbacensis (M), the ancient manuscript he supposes Poggio discovered at Murbach (for which contention there is no secure evidence) and duly had copied (= the Poggianus (P)), from which the Italian tradition (of 54 corrected or defective Roman mss) descends; and 4) O, the first sovereign hand in O. As reviewers of previous volumes have noted, most fully Michael Reeve 2 and Marcus Deufert, 3 F. therefore rejects the most obvious, and now very secure, reconstruction of the Lucretian stemma, first advanced by Hermann Diels and subsequently refined by Richard Heinze, that the Poggianus (which I term π) descends from O.

F.’s own arguments against this natural conclusion were made in an article of 1978, reprinted in 1980; 4 F. therefore has not issued a formal and detailed response to the major researches of Reeve 1, Deufert, Konrad Mueller 4 and Gianfranco Cini 5, beyond a brief riposte to three reviews of the first volume of his Lucretius that contains more that is ad hominem than ad rem. 8 My own doctoral thesis 9 has provided further evidence to support this theory and its modification by Müller, namely that the manuscript Poggio discovered in 1417/18 was an apograph of O copied before a later stratum of corrections had been added into Book 1 of that ms: my own assignation of over 1,500 corrections in O has demonstrated that this intermediary ms, which I term χ, was copied from O between the work of O’, whose corrections are probably to be dated to the late ninth century, and those of O, a hand of the late tenth or early eleventh century whose textual alterations and glosses occur in O up to c. 827. 10 It is therefore very likely that χ was the exemplar of π. The inherent probability of this thesis has been obscured to date for two reasons. 1) Most scholars have based their assignations of correcting hands upon the testimony of Diels’ edition (Berlin 1923), which, despite its thoroughness, is incorrect in many places: F.’s own autopsy has not sufficiently improved the defective bibliography. 2) Critics have failed to give due weight to the fact that several corrections in O’ (whom F. is reluctant to regard as Dungal, despite the considerable probability of this identification, first made by Bischoff in 1965) not drawn from Ω, as well as all of the

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1 F. continues to regard these as fragments from two separate manuscripts (GV; U), contrary to the conclusions of Bernhard Bischoff and Birger Munk Olsen.
5 Reeve’s 2005 Aevum article followed his initial groundbreaking survey (The Italian tradition of Lucretius, IMU 23, 1980, 27–48) and was succeeded by an article that tied up many loose ends (Lucretius from the 1460s to the 17th century: seven questions of attribution, Aevum 80, 2006, 165–84). The great majority of my statements about the interrelationship of Italian manuscripts are based upon Reeve’s articles.
7 G.F. Cini, La posizione degli «Italici» nello stisma lucreziano, AATC 41, 1976, 115–69. The two pages in Le scoperte (as n.4, 58–60) in no way suffice as a rebuttal.
10 F.’s first volume (which F. kindly sent me) failed to document accurately the 71 glosses and 76 corrections from this hand.
corrections of O’ (which he continues to attribute against all probability to Otloh of St Em-meran), were made entirely ope ingenii. Their clear presence in the Italian mss, when false as well as true, is of great significance.1 These assertions cannot be proven in extenso in a re-
view, but inspection of the Lucretian apparatus (as reconstructed from Martin’s Teubner,
Büchner [Wiesbaden 1966], Müller, and Flores) in the following places will prove enlighten-
ing: 1.137, 486, 646, 758, 1061, 2.397, 267, 278, 326, 3.52, 1132, 839, 4.77, 79, 166, 277, 735,
5.406, 581, 884, 995, 1330, 6.1, 128, 512, 523, 653, 590, 1028, 1045, 1078, 1139. Over 100 fur-
ther instances could be cited that point to the Poggianus’ (indirect) descent from O. The
inescapable fact that Lachmann was correct to recreate the archetype (Ω) of OQGVU as a 26-
line codex, which very probably was written in the eighth century, causes a problem that
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In the wake of the advances made in the study of Lucretius’ transmission over the last four decades, it is both regrettable and puzzling that F. was satisfied not to argue his own controversial contentions in further detail. The result is a text that
misplaces great faith in the authority of 15th-century Italian manuscripts:3 the present reviewer can state confidently that when F. relies on a Renaissance manu-
script he is simply supporting an Italian reader’s guesswork, even if it be correct,
with very few exceptions.4

For Books 5–6 of De Rerum Natura F. provides, as before, a dense apparatus criticus that contains a relatively detailed account of the readings of OQ (and U for
5.928–79, 6.743–1286) and the six manuscripts that F. primarily uses to recon-
struct π, LPABFC. However, despite F.’s repeated claims to «faticose collazioni» of all mss he regards as authoritative, his citation of them is often incomplete and

1 See my thesis (as above p. 198 n.9) Chapter IV (The Correcting Hands of O) 148–202. That F. asserted in Vol. 1 (16) that O’, if his work were conjectural, «[s]arebbe stato uno dei massimi filologi della latinità» shows that he has not analysed the evidence with due care.
2 On the basis of the researches of Reeve (as n.2, 144–52; as n.5, 39–45), I have argued (as n.9, 191–99) that knowledge of χ came into the hands of the owner of ϕ (from which FC descend) and an annotator of S. Onofrio 83 (r, Rome Naz. Fondi Minori 437), and that knowledge of O entered the tradition (via collation) in the 1460s or ’70s, for which the annotations of Vat. Pat. 312 (o) are the most significant witness.
3 E.g., at 5.587, 679, 705, 736, 776, 809, 823, 901, 1064, 1431, 6.83, 103, 218, 281, 736, 743,
1132, 1136, 1281, 1282.
4 The few cases where π either did not contain an alteration made by O’/O’ or presented a
nonsensical reading that differs from O can be explained with ease by recourse to O’s
appearance. For instance, at 6.941, where L reads tundaque and O reads uninclaque (Dun-
gal’s correction of unitaque), the corrected letter d is so paired as to appear like a large
d; in χ or π this was easily misread as cundaque, which by the time of L was read as tunda-
que through another ubiquitous confusion.

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misleading. This is most evident in the case of the manuscript that all Lucretian scholars agree to be of major importance, the Oblongus (O).

Despite the repeated announcements of his careful autopsy and close analysis of O (cf. p.[11]), a major shortcoming of F.’s apparatus is his citation of its corrections. Given that F. regards the readings of O’ (Dungal) as a separate branch of his tradition (and thus of the same stemmatic importance as the whole Italian tradition), he is regretfully negligent in recording them: in Book 5 he omits 48 correct alterations made by this hand (excluding several via erasure), and in Book 6 no fewer than 133 (including even the marginal addition of locus at 6.1117): that Dungal’s correct alterations duly pervaded the Italian no doubt obscured these cases for F. In addition, he fails to cite 28 incorrect alterations made by O in Book 5, and 41 in Book 6. Of the second major correcting hand (O’) he has failed to record 29 corrections in Book 5 (disregarding merely orthographical changes) and 37 in Book 6. More significantly, F. has neglected to record alterations (correct and incorrect) made by O or O’ that are not to be found in Q(GVU), and typically not Ω, but that duly appear in L and other Itali (and doubtless Ω): in Book 5 he fails to record 18 such cases for the corrections of O’ and 7 for O’; in Book 6 the total rises to 27 instances for O and 7 for O’. In several instances F.’s apparatus therefore hides evidence that should be presented clearly to the unwary reader. At 5.406 Q initially read quod procul a verna munus est ratione repulsum (with variable word division); Dungal attempted to emend what he read as hos Itali: in Book 5 (disregarding merely orthographical changes) and 37 in Book 6. More significantly, F. therefore read the impossible a verna animis. At 6.1 Q’ read frugiparos, which is certainly correct, but Dungal altered the word to the commoner frugiferos, which adjective thereby occurs throughout the Itali. At the close of 6.208 the reading of OQ (colos et splendidus olis) required only the doubling of one letter (ollis) to restore the true reading; Dungal, however, made the unmetrical and incorrect changes to color et splendidus solis, which appears in L and several other Itali. At 6.364 the correct frigus (QQ) was wrongly expanded by Dungal to the meaningless friguius to repair metre, which mistaken correction dominates the Italian tradition. In not one of these cases does this information appear in F.’s apparatus. One could cite scores of further such omissions, but among the most significant are: 5.502 (auris OQ: auras O’), 727 (chaldeum O’Q: chaldeam Ω), 1253 (horribili O Q: horrendi O’), 6.166 (fulget O’Q: fulger Ω), 296 (incidit O’Q: incidit Ω), 547 (discerpunt O’Q: discerpret O’), 661 (partim O’Q: paritem Ω), 718 (manare O’Q: manare Ω), 799 (laevaris O’Q: laevars Ω), 823 (deriguit O’Q: dirigit Ω, ΩQ: inde Ω). To omit material of this significance is to fail as an editor. Even with readings of the original scribe(s) of O, F. is worryingly remiss, neglecting to mention, e.g., 6.40 radii Q: radius Ω, 383 utram se ex 6.87: utram Q: utram Q, 486 quantique Q: quantae Ω, 816 hos ΩQ: nos Ω, 1228 uti QU: om. Ω. Nevertheless, the frequency of the collocation «O(F)» in F.’s apparatus remains tellingly high. When F. does cite the corrections of O, he regularly ascribes them to the wrong hand: 40 are incorrect in Book 5, and 77 in Book 6, typically through the ascription of Ω corrections to O’ (after Diius) or of Ω corrections to F.’s O’, the original scribe(s). Even with this original hand, F. sometimes seems insufficiently at home. For instance, at 518 he wrongly reports O as plicamentur; rather, the scribe wrote praemunitur (= premunitur) with a pre-Caroline open α. More seriously, in his discussion of the confused state of the capitula in the index before Book 5 (p.[262]), F. states of the heading to 5.144 that O reads dis compositione verumque finitum in eo sunt, whereas LP read stittutum: Quod demonstrat, F. concludes, is his reading of O by autopsy, one must...
have grave concerns about his familiarity with Carolingian minuscule. In several cases he misattributes to the original text of O later corrections. At 6.1139 F. attributes to O cecropius, which is rather Dungal’s correction of cecropi (O’ QU) that duly entered π; cf. also 5.681, 833 and the strangely hesitant «O’ fort.» of the unquestionably Insular correction to 5.633. F. also attributes to O the reading minimum at 5.581 (which corrects mi, also found in Q), when it is clearly from O’ by virtue of palaeography and the fact that it must follow Dungal’s work;¹ that O’ made corrections without access to another ms, and that this conjecture is manifestly wrong (Bentley’s minos is generally accepted), render it most significant that minimum stood in π; cf. also 5.428, where O’s correction of omnigenus (Q) to −os is ignored. At p. 256 F. assigns the marginal note capitulum to O rather than the hand of the Glossator (whom I term O’), who worked some two centuries after O. Even with the most important Lucretian manuscript, therefore, F. gives neither complete nor accurate information.

We return to F.’s selection of codices. Beyond OQLPABFC, he limits his regular citation of mss (to use his sigla) to Pierpont Morgan M 482 (p), Vat. Lat. 3275 (c), Vat. Ottob. Lat. 1136 (f), 1954 (j) and 2834 (q), Vat. Urb. Lat. 640 (h), Vat. Ross. 502 (a) and 884 (Da), Vat. Pat. 312 (o), Basil. F VIII 14 (b), along with the correcting hands in Laur. 35.25 (D) and Monac. Lat. XII 69 (l); less occasionally cited are Marc. Lat. XII 69 (M), Neap. IV F 51 (N), Cesena Malatest. S 20.4 (x), Vat. Lat. 1569 (z), Vat. Reg. Lat. 1726 (Aa), BL Harl. 2612 (Ba) and Berlin Staatsbibl. MS Lat. fol. 544 (Ka). Several elements of this selection are strange: b j Ka are descendants of N, (which for this part of the poem) is itself a descendant of o, as q is; P is very probably a descendant of M, which clearly descends from L, and h may well descend from F for Books 5–6; Da has no systematic import. Such codices descripsi clutter the apparatus, which lacks citations of other mss of greater significance for reconstructing π. Not only should x z and Aa be cited throughout these books, but F. should have recorded careful collations of three other manuscripts: Madrid 2885 (Ja) and Laur. Conv. Sopp. 453 (d), which are not cited despite their independence of L, and Ambros. I 29 sup. (t), a descendant of o that can give further insight into the φ group, which probably descends from o but was able to draw readings from a collation of χ.² Important evidence for contamination of the

¹ An anonymous hand has made 177 marginal marks in O, which flag up almost exclusively perceived difficulties in metre or Latinity. These annotations can be shown to postdate Dungal but predate O; that such a mark occurs alongside 5.581 shows that the metrical defect of mi was not healed when this annotator worked (cf. my thesis [as above p. 598 n.9] 162–9).

² In his addenda and corrigenda (31), F. states, without any reference to Reeve’s work, that O shows knowledge of O’s readings. This is clearly true, but need not mean anything
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tradition independently of π could have been given by citation of the annotations in r (cf. n.12), an apograph of o whose text survives to 5.338 (e.g., 5.33 acerbantius, 527 ali).

Given that F. has never drawn a stemma of Lucretius, or stated clearly how all of the manuscripts that he cites relate to one another, vertically or horizontally, it is difficult to interpret what principles determined his cherry-picking of mss. One presumes that his silence regarding the 23 remaining codices listed in his conspectus siglorum whose text could have been cited for these two books means that he regards all of them as descripti.1 We have seen that this is not the case with d, but several other manuscripts – BL Harl. 2694 (Z), Estensis Lat. 97 (c), Marc. Lat. XII 166 (g), Patavinus Capit. C 71 and 76 (u w), Florent. Naz. Panciat. 176 (y) – have not yet been shown to be copied entirely from another extant manuscript. These and others are ignored by F., despite their appearance in his conspectus (27–9).2

This list still contains some ambiguous references, e.g., «Leidensis Vossianus F 30» and «Cantabrigiensis 2.40».3 F. gives the siglum Fa to a manuscript he describes as «Collection of Major J. R. Abbey»: since it is was bought from the Abbey collection by a private buyer on 24/1/1975, reference should instead be made to its catalogue entry. If F. is to record mss which he has not seen and other scholars cannot currently see, he should have recorded the codex once owned by C.F. Bishop. The recently rediscovered Zaragoza ms (BCMZ Arm. 11 619 16) is not mentioned.4 The minor witnesses for excerpts of 3 to 14 lines from the poem (Ga Ha Ia) should be listed separately, and the three Vatican excerpts recorded by Reeve (as n.2, 125) added to them; the two sets of supposed fragments from Herculaneum (PHerc. 395, 1829–31) should now very probably be disregarded as non-Lucretian.5

It seems evident that F. has chosen to cite more individual manuscripts than in his earlier volumes, although in practical terms this has changed the constitution of his text very little. Somewhat oddly, the Itali are cited over twice as regularly in Book 6 as in Book 5, no doubt owing to the aid of Mariantonietta Paladini in collation (cf. p.19), with Book 5 suffering a strange downturn in citations between

other than that the text is a primitive witness to π; there is no evidence that O has ever crossed the Alps.

1 E H R S T Y Z c d g i k l m n r s u w y Ea Fa Ja.
2 F. uses O’ and Q’ to denote «correctiones recentissimae post saec. XV», although I can find no such emendations in his apparatus. The initials at 6.840, 8.48 and 887 should have been attributed to Q’, who added aestuar to 6.892. If F. wishes to amuse himself by recording irrelevancies (e.g., «veni sic! propter menstrua ha» at 5.764), one would expect him to report Q’s large Laus deo at the foot of 64.
3 For ‘t’ «Ambrosianus I 290» should be «Ambrosianus 1290»; the variation from «Harleianus 2694 British Lib.» (Z) to «British Library Harl.» (Ba Ca) seems unmotivated. The Latin «c. annis», used in the date of five mss, is curious. Sigla are not always employed clearly in the apparatus: often the pairing «Ba» or «Ca» appears where F. means not the Harleian manuscripts but rather two mss in each case (see, e.g., 5.699, 1011 and 1131). I do not know what «f-l» means at 6.447, and «B haud male a» at 1.1431 is odd.
4 The manuscript, which probably descends from o, is discussed by A. Traver Vera in the forthcoming volume of Exemplaria Classica (2011).
5 See further B. Beer, Lukrez in Herculaneum? – Beitrag zu einer Edition von PHerc. 395, ZPE 119, 2009, 71–9. It is presumably because of these ‘fragments’ that Bibliopolis included Lucretius in their series ‘La Scuola di Epicuro’, described as «collezione di testi ercolanesi» on p[4]. Reference to PHerc. in this volume is made only ad 5.1301, without consequence.
vv.431 and 605. The citation of some mss seems entirely arbitrary: for instance Ka is cited only at 5.34–45 and 6.1136–1287, and M only at 5.34–100 and 6.1–120, although both are regularly recorded in these brief passages. If F. albeit mistakenly believes these manuscripts to be independent of his main witnesses, they should be cited throughout.

Certainly more deserved to be said of the Cesena ms (x, Malatest. S 20) than the mere sentence on p.[31] n.6: the manuscript’s readings give instructive insight into the Italian tradition and could well be one of the most important witnesses for the readings of π in its state after Poggio’s death. F.’s partial collations of the Cesena ms are often incorrect: a cursory inspection of x prompts the following corrections: 5.397 petonta rapax, 6.465 uitur samnior, 531 auentis, 550 esdiopius, 701 vertice ni, 710 concioitel, 818 loca malit. F. has failed to cite several readings from x, which disguises in most cases its proximity to π and O: e.g., 5.152 quod si, 342° et, 382 olis, 437 indue, 531° sit ech, 656 rosea natura, 727 babilonisa, 742° palliament aceres, 764 penabitur, 825 aeneque, 883 lactantia, 937 creatat, 962° uidgetat, 1267 dularet lewre ac, 1361 at, 1393 propterea quae, 1425° a nobis, 6.87° persuenerat, 103° popides, 113 petuianthius, 226° motibusque, 376° quàetier bida, 405° p molte premer, 427 descen-dat, 438° latica, 511° turbam in ore, 539° dirupes post rupe, 541 sum-meros ca, 629° morbis, 633° propositas, 767 certe, 799 cuntcre, 818 alitibus, 858 soclare, 912 qui neque, 914° semper, 915° cuuda (nisi ciuda), 922 mitis parque, 916 coorta, 964° extracta (summis), 972 maritis, 974 maritimum, 977° tumida, 1091° aetatem, 1132 calantibus, 1141 uenies, 1192° sapppremum, 1220 tua (asterisked readings differ from the other mss F. cites).

As it stands, F.’s apparatus thus conveys a large amount of information but in a regrettable form: even among the Itali the more important manuscripts are cited pell-mell amidst the chaff, as if codices numerandi non ponderandi essent. To the uninstructed, the plethora of readings may seem impressive; to the scholar seeking uerae lectiones, the sheer gallimaufry of material will be a barrier more often than an aid, obscuring readings of real worth and importance.

To turn back to F.’s six major Italian witnesses, AB (Vat. Lat. 3276; Barb. Lat. 114) and FC (Laur. 35.31; Cantabr. Nn II 40) are traditional pairings of genuine significance by virtue of their independence from L, although the latter pair is heavily affected by the conjectures of the g group. L (Laur. 33.30), the manuscript of Niccolo Niccoli, is very probably a direct descendant of the Poggianus, and in many respects the most important witness to its readings. In his introduction, F. makes the new assertion that «si deve pensare che (P) abbia piuttosto a base, se non essenzialmente, i soli LP» (17). It has been demonstrated, however, by Reeve that P (Paris. Lat. 1036) is a descendant of L via M and therefore bears no independent authority: that F. dates P «post 1429», L «c. anni 1430» and M «post 1450» is an attempt to obscure the facts:1 L, the earliest extant Renaissance ms, cannot be dated securely but was written in the last years of Niccoli’s life (†1437); P could have been written at any point after L before 1460; M cannot be dated more securely than between the 1450s and late’50s.

If F. wishes to fancy that LP are siblings, he is beholden to cite all of their singular readings as witnesses to his (P). Unfortunately, this is by no means achieved: to take the case of L, 91 readings peculiar to it are not cited in Book 6, with particular frequency after verse 805: e.g., 6.66 cernimus, 136 dant om., 138 perscindant, 143 grutterque item, 212 tegens, 228 saltare,

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1 F.'s perverse decision to omit mention of Reeve throughout the entire volume means that reference is not made to the correct explanation (as above p. 598 n.5, 31), and instead we find nonsense (e.g., 5.34, 833, 6.211).
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266 venientes om., 282 maturis cum, 307 non imitanda, 333 inter mortando, 374 non imitanda, 535 extectet, 622 largitus, 625 quom, 645 panda, 728 a serrantibus, 779 tactas, 822 corrupta, 836 xerati, 838 uaporem, 859 nix, 864 tibi, 866 nam (pro manu), 885 consurgent, 914 ili corno etc. A closer inspection would turn up several cases where F. has omitted readings that can be traced back to π (e.g., 6.367 fluctus et, 530 magni, 567 tantum (tantum' O'), 718 versusus, 825 factit, 836 lumine, 859 nix). In addition to these, F. omits a further 43 instances where O agrees with L (and several other Itali) against Q.

F. seems to persist in regarding AB as descendants of L, rather than as independent witnesses to π, which is infinitely more probable. He is therefore forced to explain the coincidence of their readings with O and Q' against L as the result of contamination from a lost copy or copies of an (hyper?) archetype of OQ (which Q' also used) that entered Italy before A was copied in 1442 (probably in Naples). This contorted explanation is unsatisfactory: so many of the readings found in the Italian tradition, including in AB against L, reflect the corrections and mistaken alterations of correctors specific to O that their source must be O itself or an apocalypse; the keen work of Q', by contrast, can be regarded without any difficulty as conjectural. F. however proceeds to revise his explanation of the division of readings often evident between pABF and LP: he now supposes that these reflect not double readings in π but rather that pABF somehow have readings independent of the tradition that reached the ninth century. But this only pushes the problem further back in time: if π derives from (M), and F. does not posit another manuscript alongside (M) that is independent of the archetype of OQGVU (Ω), are we to suppose that these double readings were preserved in (M)? All of these claims, couched deep in speculation, evaporate on a closer inspection of the tradition.

F. repeats his belief ([11]) that Lucretius' transmission passed through an Insular phase. We find mention of English (5.412, 485), Anglosaxon (6.320), Irish and Scottish (6.1138) hands to defend several implausible confusions of letters, twice to defend F.'s own emendations (5.485, 6.320). Remarkably, he asserts at 6.1192 that the exemplar of U (shared with Q) was written in an Insular hand, despite the near certainty that the archetype was in Caroline minuscule; the basis of his assertion, a supposed confusion of the letters r and p «in scriptura minuscula insularis», arises from his mistaken collation of U, which reads suennum not suennum, as he asserts.1 Even more improbably, F. implies at 6.1138 that the source of mortiferentum (doubtless intended as one word) in Fhf is written in an Insular hand. In reality, there is no secure evidence for an Insular phase in Lucretius' transmission, and almost fifty years ago Virginia Brown firmly disposed of this romantic notion of several scholars of the nineteenth and early twentieth centuries.2 F.'s feeble counterarguments in Le Scoperte (as n. 4, 7–8 n.8) do not pass muster: there is no secure evidence for direct knowledge of Lucretius among writers of the British Isles at any point until around the year 1460, when John Tipoft, Earl of Worcester, secured a manuscript for himself (X, Bodl. Auct. F I 13).

We may now turn to F.'s apparatus, where he allows himself more room for editorial comment than is customary. Unfortunately, several notes are hard to understand.3 At 5.962 it is stated that L in the text and P in the margin read iungebat

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1 Other errors can be found in F.'s collation of U: read at 795 effulit it, 800 effueris, 1026 ergo it, 1038 mutas est (π) per, 1122 conturbus set, 1235 apicat.
3 In a number of cases F.'s Latin is ambiguous (e.g., 5.31, 201, 679, 1442, 6.179) and often questionable: ettam (5.199), sed (5.468), est (5.825), quod in cod. (P) fuisse significa-
(L.66 P read lugebat); to this F. adds the parenthesis «quod erat etiam in (P) et (M)». What logic allows F. to conclude that lungebat stood in (M) as well as π is entirely unclear: if it did stand in π, why could the correction not have been made by Poggio, Niccolì or another learned reader?

This same problem applies to several cases where F. argues for authorial variants in the tradition. Although he first waded into this field in 1965, it has not gained greater favour since. Significant divisions between two readings preserved in the tradition, which F. is strangely reluctant to regard as being the result of conjecture or error, he treats as Lucretius' own variants, both of which have somehow survived throughout the poem's transmission, despite their complete absence from OQGVU. Among Books 5-6 we find: 5.35 severa OQABFC: sonora L; 819 navibus mss: nibus schol. Bern ad Verg. Geo. 2.336 (according to Diels); 1294 approbrium mss: obscenum Macr. Sat. 6.1.61. Yet L's sonora is doubtless a conjecture made in π, perhaps made after μ (the source of d Ja and AB's exemplar) was written; if the Bernese schol. on Virgil do read nibus, it is doubtless an easy slip influenced by the preceding magnis. Macrobius had no direct access to Lucretius, so the strange reading obscenum is most likely an error of his own, his source or his transmission. F. also regards several verses in Book 5 as compositions that Lucretius would later have rejected, had he lived to give his final revisions to the poem. This is a sound theory, and undeniable examples exist elsewhere in the work (most famously the rejected programme of Diels); of the verses that F. puts between braces ($586$, 674) or double lines ($1327$, $1359$, $1388-9$), the first two are much more probably in need of emendation,$^1$ $1327$ is a less appealing candidate for deletion than $1342$, but both $1359$ and $1388-9$ might well have been deleted in due course.

The text itself of F.'s edition continues along the generally conservative principles of his earlier volumes, with few emendations accepted that are significantly removed from the paradosis as reconstructed from OQ(U) or whichever italics he favours. F. eschews implementation of the obelus entirely, and in some cases (e.g., $5.947$) declares that he prints a conjecture «quia aliqua melior non inventa est»: in this case his support of Forbiger's clarus citat late is unfortunate, as the conjecture is metrically unparalleled.$^2$ F. flatly records his dislike of violent emendation and Interpolationsforschung in his introduction ($12$). Yet each case needs to be taken on its own merits, and F.'s text certainly preserves readings that other scholars (myself included) have regarded as very improbable or impossible, e.g., $5.201$ asidam, $949$ debere, $948$ nota nugas, $1066$ nauigii, $1106$ igni, $1429$ numerum (gen. pl.), $6.650$ idque (scil. biatum neut.), $665$ morbi, $743$ remigii, $755$ opus efficit, $762$ pote eis, $1109$, $1277$ enim.

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$^1$ The text of $5.386$ I have discussed at Prometheus 35, 2009, 86–9; at $5.674$ the repetition of mollem may be a Perseverationsfehler, and the verse would gain greater purpose if (e.g.) primam were read.

F.’s own conjectures are not typically compelling; disregarding the impossible (5.11) and those that have been preceded by earlier scholars or manuscripts, only summum at 5.144 is unobjectionable, although F. does not account for the transmitted text propter odores. His own transposition of 5.144-5 to after 125, on the ground that 5.128-40 (m.784-96) has engendered the mistake, is novel, but the verses read rather flatly after 124-5. He posits that the mistaken transposition of 144-5 to after 143 is an «error antiquissimus... or tus in ipso Lucretii exemplaria; yet if the error stood in Lucretius’ fair copy (or rough sheets), one wonders how it can be described as an error at all, rather than the poem’s ordering before its final revision. F. could have considered more carefully the merits of more emendations made after, rather than during, the 15th century. Several excellent conjectures are overlooked, and many he cites just to add ‘pessime’ or «nutiliter’. I limit myself to recording these ten: 5.122 distant (Madvig), 186 specimen (Pius), 468 pandit (K. Mueller), 737 it (pro et, Romanes), 969 pariles (Garrod), 6.116 at (pro enim, Kannengiesser), 244 deorum (Meurig-Davies), 386 notare (Orth), 899 natantes (Romanes), 972 quom (Ernout). F.’s attitude to Latin orthography is disappointingly inconsistent: occasional spellings of the Late Republic are printed (5.66 natuomique, 531 heic, 1082 quom, 6.298 and 444 vortex; cf. also the apparatus at 5.38, 1102, 1286, 1305, 6.1251) but F. adopts Early Imperial orthography beyond these isolated cases. The text itself is generally free from misprints, although amentia is wrongly bracketed at 6.509, an is printed for ab at 6.621, and ea for eo at 6.796. F. marks five lacunae (5.704, 1012, 6.47, 48, 954), none new. He correctly rejects the lacuna Lachmann posited after 6.839 but does not suggest where the six fragments whose veracity he accepts (pp.251-2, an appendix drawn largely from Büchner) could have stood in the poem.3

Insufficient use is made by F. of the indirect tradition: testimonia, an indispensable part of complete critical editions, are not recorded throughout his text. For Books 5-6, 84 indirect citations by ancient or early mediaeval authors have not been mentioned at all. Typically F. only cites such authors when they offer a correct reading (but see, e.g., Nomius ad 6.740-1). When the indirect tradition is cited, it is usually impossible to learn from his apparatus which lines were quoted.

Often F.’s omissions of readings from such authors are misleading: at 5.1221, he neglects to note that OQ’s murmure (O) is attested in Mico’s Opus Prosodiacum (s.IX); at 6.353 that Seneca the Younger (Epp. 110.6) supports caree (O’) and several Itali; at 6.725 that oppilare (O’) occurs in the Florilegum Sangallense; at 6.874 that radius (O’) is supported by Macrobius (Sat. 6.4.7); at 6.877 that dentitiat quis is recorded by Priscian (GLK 2 211,22); at 6.1135 that altro, which F. attributes to Avancius, can be found in a tenth-century hand in Sergius’ De syllabis (the same hand also records Britannus at 6.1105), which deserves no less than Ga Ha la to stand in F.’s conspectus siglorum.

To turn to bibliographical matters, F.’s knowledge of Lucretian scholarship is found wanting in a number of respects, a situation not helped by his reliance upon several later editions and secondary testimonies (or worse). It is understandable not to mention Orth’s 1961 Salamanca edition, García Calvo’s 1997 Zamora edition or...

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1 5.112 tripodi (D. Clay), 199 (O’), 312 (OQ), 6.298 (early edd.), 444 (several Itali, various edd.).
2 Remaining conjectures are 5.468 sparsit, 485 inertem, 652 appulis, 673 barba, 6.320 tan-tisper, all but the unmotivated barba at 5.673 being adopted in the text.
3 I have argued elsewhere (as above p. 598 n. 9, 60-88) that fragments I, IV and VI are spurious, and that II, III and V are probably misattributed to the poem. Boeck’s restoration of a hexameter for fr. III is reported despite its being unmetrical.
two partial commentaries of 2009, but there is no good reason why F. has not directly used Faber’s 1662 Saumur edition, Turnebus’ monumental Adversaria (1564–73), or several manuscript sources.

Because of his reliance upon the Variantes Lectiones in the 1712 Tonson edition of Lucretius, F. misattributes conjectures of Gronovius to Susius at 6.118 and 747. More regrettable, F.’s dependence on Havercamp and Turnebus, two of the most slap-dash Lucretian editors to date, leads him to attribute non-existent readings to X (Bodl. Auct. F.I.13); at 5.182, dis is solely a conjecture of Isaac Vossius (who wrote ideo dis ad loc. in Leiden 757 G 25 and Camb. UL Hhh 1283, later copied into Oxford Bodl. D’Orville 324); X rather reads diius with most mss. At 4.897 (corrigenda p.31) X reads corporis not corpus, and at 4.968 (Vol. 2) uellum not duellum. At 6.103 F. cites ligna from the Tonson edition, apparently unaware that it occurs in Vat. Lat. 1569 (among other Itali) and the ed. Veronensis (1486). F. cites Vind. 170 (W) from Wakefield, not F.C. Alter’s edition (Vienna 1787) or via autopsy; W’s seque at 5.679 doubtless signifies consequae, not Lachmann’s conjectured adverb. F.’s sole citation of n at 6.520 is not supported by Wakefield’s statement. At 5.571 Turnebus’ filumque should be cited from his Adversaria (XIV.23), which appeared in the same year as Lambinus’ second edition; at 5.1120 Adv. XXX.22 should be cited; at 6.574 «Turnebus apud Lachmann» is misleading, for Turnebus (Adv. IV.17) did not discuss this Lucretian passage; at 6.862 rara should be attributed only to Lambinus, who made the suggestion a decade before Turnebus. At 5.514 F. alleges that the Tonson edition misattributed altermi to Faber, but F. should read p.362 of the latter. Had he consulted Isaac Vossius’ own copy of Faber’s edition (now Trinity Coll. Camb. Adv. b.13 3), discussed by Munro and Reeve (as n.5, 180–4), F. could have avoided several inaccuracies: emendations of Vossius can be found here ad 5.747, 1145, 6.523, 1124; his conjectures should read consanguinea est at 6.475, medoore at 6.641 and et at 6.715; at 6.934 Vossius did not defend caeli but merely added it through his collation of QQ (caeli). Careful inspection of this volume and of G.J. Vossius’ text of Lucretius (Leiden UB 757 G25) could have helped F. in distinguishing which made a given conjecture, a matter he often disregards. Avancius’ conjectures at 5.241, 1048, 1361 and 6.964 should be cited from the close of his 1502 Catullus; inspection of these notes would have revealed that at 6.952 odos is owed to Avancius, not Lambinus. References are desired at 5.881 to Cartault’s La Flexion dans Lucrèce, Paris 1898, 90 n.3, and at 6.642 to Heinius ad Ov. Met. 4.336; at 6.698 F. wrongly claims that no one has realised that Faber conjectured animam, but van der Valk himself did.

The metrical Italian translation, so far as I can appraise it, is generally accurate, and at times suitably vigorous. The quality of the printing, paper and binding is high. F. closes his Italian translation with «AMENs», imitating the final subscription of Ω. This bald statement of how he has discharged the task of editing the poem reflects much of his peremptory manner, yet Lucretius and scholars of his text are left frustrated. For all his claims to heal the faults of former editors, and to have collated all manuscripts to some degree (111–12 etc.), one cannot have confidence in either F.’s critical method or his editorial accuracy, which has as much of Wakefield as of Havercamp about it. Despite the smorgsobord of Renaissance readings, his three-volume edition and its «apparato esaustivo» is destined to have only tangential value in the field. Not only does the world still lack an apparatus that is

2 It is unfortunate that F. states of Grasberger’s dissertation (De Lucretii Cari carmine, Munich 1856) that «per lo più non vale il costo della stampa».

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both accurate and devoid of irrelevance but it is yet to see a full critical text edited on the understanding that no manuscript beyond OQGVU can bear independent authority for the direct transmission of De Rerum Natura.

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David Butterfield


Angesichts des Umstandes, daß die Römer ihr Gemeinwesen als res populia > res poplica > res publica, mithin als «the People's property» oder «the People's business» bezeichneten, scheint es ihm paradox «that modern historians of Rome regularly assume that the republic was always and necessarily an oligarchy» (1). Er mag nicht glauben, daß Rom von einer «innately conservative» (2) Führungselite regiert wurde, deren Mitgliedern es – mit Symes Worten – allein um «power, wealth, and glory» (16) ging, aber nicht um politische Inhalte. Daß der Gebrauch des Wortes «party» die Gefahr des Anachronismus in sich birgt, darf uns seiner Meinung nach nicht den Blick dafür verstellen, daß partes im Sinne von politisch-ideologischen Parteien (7) eine entscheidende Rolle in der Geschichte der römischen Republik spielten. Zumindest seit der Zeit der Gracchen gab es «two rival ideologies, two mutually incompatible understandings of what the republic was» (9), nämlich die der Optimaten, denen es um die Interessen der Oberschicht ging, und die der Popularen, die sich für die Interessen des Volkes einsetzten. In dem er sich gegen jenen «twentieth-century way of thinking about Roman politics with ideology taken out» (24) wendet, dessen Entfaltung in den bekannten Arbeiten Gelzers, Münzers, Strasburgers, Meiers, Symes und Ross Taylors er kurz skizziert, will er zeigen, welchen Gewinn es für unser Verständnis der politischen Geschichte der römischen Republik bringt, «if we 'put the ideology back', and take the concerns of the Roman people seriously» (33).


1 Die These einer von Geta als Konsul des Jahres 116 geförderten Aufführung einer C. Gracchus-Licinia-Tragödie (54–55) als Erklärung seiner Entfernung aus dem Senat im darauffolgenden Jahr ist freilich nicht mehr als eine geistreiche Spekulation.