EDITING LUCRETIUS*

1. After Lachmann

Cautious scholars, to adapt a phrase of A. E. Housman's, do not edit Lucretius.[1] That is not, as in the case of Lucilius (the subject of Housman's original comment), because of the difficulty of dealing with fragments that have no context; the problems facing the editor of Lucretius are more complex and more daunting. In the first place, the text of Lucretius was badly preserved in a single strand of textual transmission from late antiquity to the early Carolingian period, the date of the earliest important manuscripts: it is marked by a great number of textual errors, and it is also marked by a significant but disputed number of spurious verses interpolated into the text at a stage before the latest common ancestor of the extant manuscripts. Any editor of Lucretius must be willing frequently to emend or obelize, delete verses or transpose them or identify places where lines have been lost. S/he must also constantly place his or her own judgment on display before an audience many of whom have quite strong opinions about the quality of the text: there are few authors I can think of (Propertius is one) where scholars diverge more radically in their understanding of the relationship between the surviving text and what the author actually intended.[2]

In the second place, no modern editor can ignore the shadow cast by the towering figure of Karl Lachmann, whose magisterial 1850 edition remains a

[1] A. E. Housman, "Luciliana," in *Collected Papers*, Cambridge 1972, 2: 662-84 (the quotation is from 663): "Cautious men do not edit Lucilius; they leave him to be edited by bold and devoted men, whose heroism they admire with that mixture of pity and self-congratulation which a Roman may be supposed to have felt as he saw Curtius descend into the gulf, or an Israelite as he watched the departure of the scapegoat into the wilderness." I wish to apologize to Professor Deufert and the readers of *Exemplaria Classica* for any gaps in my argument or annotation caused by lack of access to libraries during the current pandemic, and to thank Katharina Volk for improvements to an earlier draft of this review and Sander Goldberg and David Sedley for information and offprints.

[2] J. S. Phillimore's comment in the preface to his 1901 Oxford Text of Propertius is well known: *Quot editores, tot Propertii.*

strong and vivid presence, not only in the editorial history of Lucretius but in the history of textual criticism. The opening sentences of few commentaries can match the Olympian self-assurance with which Lachmann described in detail a manuscript that nobody had seen for centuries: "Ante hos mille annos in quadam regni Francici parte unum supererat Lucretiani carminis exemplar antiquum, e quo cetera, quorum post illa tempora memoria fuit, deducta sunt. . . Id exemplar ceterorum ARCHETYPON (ita appellare soleo) constitit paginis CCCII, quarum non tantum prima et ultima, sed praeterea centesima nonagesima, quae erat post finem libri quarti, conscriptae non fuerunt. . ."[3] And he went on to state that each page had twenty-six verses on it, that it was written in rustic capital script, and more. No matter that Lachmann was wrong in his description of the lost archetype (he conflated characteristics of a late-antique manuscript and its pre-Carolingian descendant); no matter that he had not, as he implies, invented the term "archetype"; and no matter that he was extremely patronizing about the article on the transmission of Lucretius published three years earlier by Jacob Bernays which anticipated much of his reconstruction—Lachmann's tone in itself, which continues in the clipped and peremptory entries in his commentary, has made him remain a figure to be reckoned with.[4]

And deservedly so: Lachmann's Lucretius was something new, not just in its tone, but in the single-minded concentration of the commentary on textual matters: on *DRN* 1.1 he discusses not the substance of the proem, but whether one should write *genetrix* or *genitrix*; on 3.1 he scornfully rejects—in a note only four lines long—the text most editors now accept, *O tenebris tantis…* as *valde ineptum* in favor of Marullus' *E tenebris tantis*. Lachmann was extraordinarily learned and his notes contain "a mine of information on Latin language, grammar, metre, and usage," but that information is not always correct; what is more, his "arrogant insistence on the principles of Latin usage, which subsequent criticism has concluded that Lucretius did not observe" and his lack of interest in Lucretius' philosophy distorted his judgment and thus the text he produced.[5] But Lachmann can not be ignored, not just because he so often speaks in an oracular voice that brooks no dissent, but because he is always intelligent and very often right. Even if his solution to a problem may no longer seem correct, the problems he recognized were very often genuine.

Marcus Deufert shares many of Lachmann's virtues: he is a careful and thorough Latin philologist who deploys his wide learning with great intelligence and to very good effect. What is more, not only does he in many respects take Lachmann

---

[3] C. Lachmann, *In T. Lucretii Cari De Rerum Natura Libros Commentarius*, ed. 2, Berlin 1855, 3.

[4] The fundamental treatment of Lachmann is that of S. Timpanaro, *The Genesis of Lachmann's Method*, transl. Glenn W. Most, Chicago 2005; his summary of what Lachmann's contribution to critical method actually was (115-18) includes the judgment that "he was a great simplifier, with all the virtues and vices this brings with it."

[5] The two quotations are from C. Bailey ed., *Titi Lucreti Cari De Rerum Natura Libri Sex*, Oxford 1947, 46. For Lachmann's "oracular tone" see Timpanaro, *Genesis*, 117.

as his model, but he also has the self-confidence that permits him to come to grips with—and often disagree with—Lachmann's powerful voice. Perhaps even more important, D. is able to question not only other scholars' judgment (including Lachmann's), but his own: I can think of few scholars who could reject their own earlier arguments as "nichtig" (*Kommentar* 412, on 6.565-567). The three volumes reviewed here as a group provide an impressive demonstration of D.'s abilities and mark the culmination of his long engagement with the text of Lucretius. *Prolegomena* deals with the manuscript tradition, including discussion of the *tituli* and *indices* (as well as some well-chosen photographs of the manuscripts to illustrate D.'s arguments about correcting hands), together with a detailed discussion of orthographic questions, while *Kommentar* is a textual commentary on *De rerum natura*, more discursive and far more helpful than Lachmann's but very clearly indebted to his, discussing many (but by no means all) of the textual choices D. has made. *Prolegomena* and *Kommentar* surround and support the central volume, D.'s Teubner text of Lucretius: *Prolegomena* for the most part explains D.'s apparatus criticus, and many of its conclusions are set out more briefly in the preface to his edition, while *Kommentar* explains the text itself.[6] Throughout, D.'s work is distinguished not only by his expertise as a philologist, but by his ability to give patient and lucid (and often quite lengthy) explanations of his choices; that lucidity makes *Komm.* in particular valuable for all Latinists. D.'s statements about philological and editorial method, moreover, are clearly programmatic: he has very strong views about what an editor and a philologist should do and what the purpose and audience of a work like his—and I take the three volumes as a single unit in this respect—ought to be. As will become clear over the course of this review, however, my own views on those subjects are rather different from D.'s, and I fear that his work, while outstanding, is made less valuable by some of his choices.

D.'s edition deserves more detailed description before turning to closer examination of his apparatus and his textual choices. The preface (written at some length in very clear Latin) describes the manuscripts, in large part summarizing the first two sections of *Prol.*; so too the sections on the *tituli* and on D.'s approach to orthography are largely based on the other two parts of *Prol.*, except that D. has modified his discussion of the *tituli* to take more account of humanistic emendations. He also offers explanations of his apparatus and of his edition of the fragments.

Three aspects of the edition deserve comment. One is D.'s careful articulation of the apparatus into three sections: the first apparatus reports testimonia, from antiquity up to the time of the earliest manuscripts in the ninth century; it is followed by an extremely useful apparatus of parallel passages and repetitions within *De rerum natura*; the third apparatus is the apparatus criticus itself. The last is of course the most important, and it is both very clear and relatively spare: it is largely positive (and D. explains the format in the preface), and D. reports

---

[6] I note that henceforth I will refer to these three works as *Prol., Komm.*, and *BT* respectively. I refer to his earlier *Pseudo-Lukrezisches im Lukrez*, Berlin 1996, as *Ps.-L.*

relatively few conjectures unless he thinks they are correct; that is a subject to which I will return later.

Beyond that, what is striking about this edition is that it includes not only the critical edition of the text of Lucretius, but also a separate edition of the subscriptions and *tituli* as well as the fullest text I know of the fragments ascribed to Lucretius. In including the former as a separate text, D. follows Lachmann; in including the latter, he goes beyond him. The *tituli* are of considerable importance for the history of the text, and D. discusses them in detail in *Prol.* (177-203): they appear throughout the text of O and in GVU (when extant); they are also occasionally found in Q. Additionally, they appear as indices for each of the last three books in both O and Q. The subscriptions (explicit at the end of each book, incipit at the start of each book except Book 1) probably belong to the same stage of the history of the text as the *tituli*, although that is not necessary or certain; what is clear is that, probably in late antiquity, a set of what were originally brief marginal pointers to the subject of a section of the poem were included within the text—and, as preserved in O particularly, written in a capital script and rubricated—as headings to each section. They are, in some cases, of considerable use in analyzing textual problems; D. has argued elsewhere that they sometimes reflect late-antique critical signs as indicators of interpolation.[7] In any case, Deufert has provided the fullest and most careful edition that these texts have ever had. The same is true for the fragments: David Butterfield in 2013 argued that none of the sixteen fragments he knew was genuine (Diels had thirteen), making some very tendentious arguments;[8] D. gives twenty-four fragments arranged alphabetically by source from Charisius to Varro, with a full and careful apparatus and brief comments; he marks eight of them as doubtful (using one asterisk) and eleven as spurious (using two asterisks), leaving five as genuine; for each of those, he suggests a possible location (in a lacuna or a corrupt passage) and refers the reader to fuller discussion in *Komm.*, while for the eight which he regards as doubtful he briefly supplies reasons for his suspicions.

I dwell on these apparently minor elements of D.'s edition of Lucretius, as I could dwell (but will not) on his exemplary discussion in *Prol.* of the very difficult problems of representing Lucretian orthography, for several reasons. One is that his treatment of the fragments and *tituli* shows that they are not in fact minor elements, but have important contributions to make to the history of the text of Lucretius and deserve to be studied seriously. The second reason is that D. does in fact take them seriously, offering, either in the edition or in the subsidiary volumes (*Prol.* for the *tituli*, *Komm.* for the fragments) often extended discussions of text, origin, and authenticity. Indeed, throughout this three-volume opus, one of the most admirable features of D.'s work is that he

---

[7] Cf. Deufert, "Overlooked Manuscript Evidence for Interpolations in Lucretius?: The Rubricated Lines," in Richard Hunter and S. P. Oakley, *Latin literature and its transmission: papers in honour of Michael Reeve*, Cambridge 2016, 68-87. He employs this evidence to good effect in his discussion of *DRN* 4.216-29 in *Komm.*

[8] D. Butterfield, *The Early Textual History of Lucretius' De rerum natura*, Cambridge 2013, 101-35.

rarely skimps on anything; to read through *Komm.* is to watch a scholar who respects the problems which he addresses: he studies them and, above all, takes the trouble to explain them. In that, he is a far better commentator and editor than Lachmann himself. Such problems, of course, are minor in comparison with the two great tasks of the editor: deciding what to put in the text, and deciding how to present in the apparatus the evidence on which editorial choices are based. The remainder of this review will be concerned with those central issues, beginning with the manuscripts and D.'s construction of the apparatus criticus, then considering the emendations and choices made in the text itself, and finally dealing with the large and thorny problem of interpolation and authenticity, the area in which I believe D.'s treatment to be seriously inadequate and misleading.

2. Recensio

The manuscript tradition of Lucretius falls into two very distinct sections. On the one hand, there are the major manuscripts, written and (at least in the case of the oldest and most important manuscript, O) heavily corrected in France in the Carolingian period; it is on these that reconstruction of the archetypal text depends. On the other hand, there are more than fifty humanistic manuscripts written in Italy in the fifteenth century; these are now generally believed to descend from a copy of O made for Poggio in 1417 and thus to have no independent stemmatic value, but it has often been argued that Poggio's manuscript derives from a source independent of the extant Carolingian manuscripts—and thus that what D. (like most students of the subject nowadays) believes to be humanistic emendations may in part be genuine tradition.

That latter question makes a significant difference for the construction of a text of Lucretius and, even more, for construction of the apparatus criticus, and D. accordingly devotes more than a hundred pages of *Prol.* (66-176) to a close examination of the humanistic tradition—almost double the space he gives to the Carolingian manuscripts on which his edition is based (1-65). The reason for D.'s distribution of effort here is, presumably, because the humanistic tradition is less well known and harder to decipher, and because most of what is now known was discovered fairly recently—long after Lachmann's time, in any case—and D. in general does not linger over matters that are not, to his mind, subject to dispute. Thus, while he gives no description in the *Prolegomena* of the major manuscripts themselves (O, Q, and the three surviving portions of a third Carolingian witness, GVU) or any analysis of the relationships among them (except, to be discussed below, the role of correctors in establishing the archetypal reading), his discussion of the most important renaissance manuscripts and their relationships is very extensive and very detailed.[9]

Recent scholarship, particularly the work of Michael Reeve, has established the basic relationships among the most important manuscripts of the fifteenth century and the relationship between the entire fifteenth-century tradition and

---

[9] In the preface to *BT*, of course, D. devotes five pages of lucid Latin (VII-XII) to the description of the manuscripts, but by and large he does not repeat himself.

the earlier Carolingian manuscripts; D. builds on that.[10] He argues that Poggio's copy (π) was taken directly from O after O had been corrected; the fact that in general the Poggianus seems to have taken over the corrections of O² and O^D but not those of O³—a feature of the Poggianus that has led other scholars to posit a lost intermediary copy of O made after O² and O^D had done their work but before O³ got his hands on O—Deufert explains as the result of an instruction by Poggio.[11] While speculative, that solution seems more plausible than the various alternatives, and it makes it clear, above all, that the source of Poggio's text was O itself, not a lost gemellus, and that nothing in the Italian tradition can be shown to derive from a source independent of the archetype Ω: all worthwhile readings (and of course some errors) are the result of conjecture, not collation. What follows, for most of the remainder of D.'s analysis of the fifteenth-century tradition, is an extraordinarily detailed consideration—including extensive collations—of the most important manuscripts (about a dozen) in order to determine the origin and, as closely as possible, authorship of those emendations that deserve to be printed as correct or at least to be reported in the apparatus as plausible. This culminates in D.'s elaborate reconstruction of the readings of φ, a now-lost manuscript that was almost certainly the work of Lorenzo Valla— although the certainty is not great enough for D. to print his name rather than a siglum in the apparatus—which contained nearly 500 conjectures that Deufert thinks worth either printing or including in the apparatus, and which he lists in full (*Prol.* 132-34). His comparison of the two sets of conjectures that can be reconstructed for φ and α (a mid-fifteenth-century anonymous scholar) in terms of their scholarly methods and abilities is itself a superb piece of scholarly synthesis, and should be read by anyone interested in the history of textual criticism (*Prol.* 136-40); anyone interested in the history of humanist criticism should read Deufert's concluding summary of the renaissance tradition of Lucretius (*Prol.* 174-76).

Throughout *Prol.*, D. keeps his focus firmly and narrowly on explaining the construction of his text and apparatus. In his discussion of the fifteenth-century manuscripts, his main goal is to identify as closely as possible the source of each correction and to give credit in the apparatus where credit is due—to such an extent that the detailed sigla in the apparatus identifying the manuscript sources for any given conjecture look as if they would be more at home in a textbook of algebra. A relatively simple example of this chosen at random is provided by the entry in the apparatus criticus for *adepta* at 2.998 (*quapropter merito maternum nomen adepta est*):

adepta Q²μ^{-Ja}φ : adempta Ω.

---

[10] M. D. Reeve, "The Italian Tradition of Lucretius," *IMU* 23, 1980, 27-48 and "The Italian Tradition of Lucretius revisited," *Aevum* 79, 2005, 115- 64.

[11] Deufert uses the siglum O^D for corrections made by Dungal, Charlemagne's court astronomer, in his highly distinctive hand; O² refers to other ninth-century corrections, and O³ to a later (tenth century) corrector and annotator who only got as far as line 824 of Book 1. For fuller discussion of the correcting hands in O, see below, p. 221-3.

By this is meant that the transmitted archetypal reading (erroneous) is *adempta*, but that the (correct) emendation *adepta* is shared by the renaissance corrector of Q (Q²), the lost manuscript μ (itself reconstructed from the manuscripts J_a, d, and the lost common source α, reconstructed from A, B, and R—except that in this case the reading is not shared by J_a, as indicated by the minus sign in the superscript), and the lost manuscript φ (probably the work of Lorenzo Valla, reconstructed from the four manuscripts F, C, e, and f). The corresponding entry in Bailey's OCT is simply:

<div style="text-align:center">adepta Q <i>corr.</i> : adempta OQ.</div>

In Martin's Teubner:

<div style="text-align:center">adepta Q²ABCF : adempta OQ¹VL.</div>

D.'s entry is unquestionably harder to read, but it conveys a large amount of information, and read together with the relevant discussions in *Prol.* makes it clear that at least three Renaissance critics came up with the correct emendation (and D. does not want to deny credit to any of them, as priority can not be determined), while Bailey simply ignores the renaissance evidence other than the corrector of Q (a manuscript he obviously had to use in any case), while Martin not only offers a string of individual manuscripts the affiliation of which he does not make clear (but D. does), but gives a list of individual sources for the error rather than the group siglum Ω.

D.'s approach to the humanistic tradition does not make for easy reading, but studied with care, it is immensely informative. Equally important is that, both in his discussion of the fifteenth-century manuscripts and, even more, in the discussion of the Carolingian evidence, D.'s desire to give full credit even to anonymous conjectures is matched by his equally strong desire to eliminate from the apparatus readings that are not stemmatic and merely clutter the page, as do in Martin's apparatus entry. In terms of the fifteenth-century manuscripts, that is not a major problem: D. is only interested in reporting worthwhile conjectures, and it can be assumed that any manuscript not reported shared the reading of O (generally as transmitted through Poggio's manuscript, although Valla too seems to have had some access to O) or had an error or less convincing emendation of its own. In the Carolingian tradition, the situation is somewhat more complex. The issue is not so much the reading of the first hands of OQGVU, but the nature and date of the corrections found in them.

D. addresses two problems in particular: in the case of O, where several early correcting hands have been distinguished, he wisely prefers to label only two as specific individuals. One of them, who corrected the manuscript extensively and perhaps even in conjunction with the original scribe, was Charlemagne's astronomer Dungal; D., like Butterfield, gives him the siglum O^D.[12] A later corrector (O³ for both D. and Butterfield), who only got as far as line 824 of

---

[12] Butterfield, *Textual History,* 204-20 is an extensive discussion of Dungal's work on Lucretius.

Book 1, corrected and glossed the text probably in the early tenth century. But where Butterfield attempted to assign to specific hands (which he labels O^2 and O^{ann} in addition to O^D) *every* correction made in the ninth century, D. wisely recognizes how difficult it is to identify the scribe of a punctuation mark, an expunction, or the addition of a single letter, and assigns all corrections about the scribe of which he is uncertain simply to O^2.[13] This leads not only to a simpler apparatus, but one that more accurately reflects the impossibility of precision in such matters.

What D. does in explaining the correctors of O is clarifying and sensible; in the case of Q, it is genuinely valuable. While earlier editors have seen the vast majority of corrections in Q as the work of a humanist, D. has isolated a significant number of corrections clearly made in the ninth century by a corrector or correctors whom he labels Q^a. The identification of these corrections makes a real contribution to answering the question that really interests D., namely the value of the early corrections for determining the archetypal reading, and thus defining what belongs in the apparatus criticus: Q's corrector, like the (at least) three early correctors of O, almost certainly had access to the manuscript from which O and Q were themselves copied—in other words, the archetype Ω.

D.'s desire to streamline the apparatus is laudable and largely successful. As a strict believer in stemmatic theory, he makes much use of group sigla, in particular for the reconstructed archetype (Ω) and the hyparchetype of QGVU (Γ), as these reconstructed manuscripts automatically eliminate the reporting of errors in individual members of the group. In most cases, the determination of what reading or readings deserve to be reported as possibly archetypal is straightforward: if O and Γ disagree, then the reading of either could be archetypal and thus both need to be reported; if O and any of Γ's constituents agree, then their shared reading is the only one of textual significance and no other manuscript readings need to be cited. It is equally clear also that if one of O's early correctors agrees with Γ against O^1 (the uncorrected reading of O), then that is the transmitted reading because, by and large, both O^D and O^2 correct by collation against Ω.

But the reality of manuscripts, as D. is well aware, is less tidy than a formal stemma suggests. There are, for instance, a few cases where O^1 has what is clearly the correct reading, while one of O's correctors agrees with Γ in error. Here, Deufert rightly believes that the error must have been in Ω, from which the corrector imported it into O—thus rejecting a true reading in the first hand of O. The same is true of the three cases where O^3 has imported a reading by collation, introducing it with the abbreviation for *aliter* or *alibi* ($a\hbar$): in each case, the imported reading is wrong, and the first hand of O is correct.[14] According to

---

[13] Deufert, *Prol.* 24-26.

[14] It is not clear to me exactly what the abbreviation stands for. Deufert (*Prol.* 91) opts for *alibi*, citing Lindsay, *Notae Latinae,* Cambridge 1915, 6, who seems to support that interpretation (although also mentioning *alius* and *aliter*); on the other hand, I have always taken it to stand for *aliter*, as did L. Traube, *Textgeschichte der Regula Benedicti*, ed. 2, Munich 1910, 123 and (e.g.) H. Gotoff, *The Transmission of the Text of Lucan in the Ninth*

strict stemmatics, in a closed tradition, every time the reconstructed Ω has an error and O^1 has the correct text, that correct reading in O^1 must be seen as itself a "peculiar error" in Maas's terms, that is, a deviation from its exemplar rather than something objectively wrong.[15] But if the transmitted reading is an error (in absolute terms), then how did O^1 come up with the correct text? At 4.334, *inde retro rursum redit et conuertit eodem*, only O^1 has the correct text; O^2Q (at this point, the only representative of Γ) have the unmetrical *retrorsum*. Butterfield's belief that O^2 and Q independently made the same unmetrical correction is highly implausible (see *Prol.* 33): O^1, according to D., must have written the correct text relying on his ear rather than his eye and perhaps on the presence of *rursum* in the previous verse. But to imagine that in a small but significant number of places O^1, relying on instinct, ear, or sheer conjecture came up with a correct text that was not in his exemplar is not entirely credible: it is one thing to believe that correctors sometimes made conjectures, but it is quite another to imagine that scribes writing manuscripts made corrections on the fly.

All this is, to a certain extent, a small, technical, and (in not the best sense) academic problem, and in terms of D.'s text and apparatus it makes very little difference, for several reasons. In the first place, it concerns only a small number of passages, and in most if not all of them the correct text is obvious, even if it was not in the archetype. In the second place, D. himself is willing to be inconsistent in reporting idiosyncratic readings even if this is more generous than strict method requires (*Prol.* 31). And third, and perhaps most important, is that D.'s evaluations of the text and the manuscripts are generally very good: one is, in most areas, happy to defer to his judgment. But D. is a strong believer in Method (something that will be discussed in other contexts below) and very much a believer in the Lachmann/Maas theory of stemmatics. Thus, if he is to keep his closed tradition intact, what D. simply can not accept is that *retro rursum* and *retrorsum* are derived from different manuscripts: "Nur eine solche Handschrift, die man als die einzige Vorlage geflissentlich wiedergibt, hat die Autorität, dass man ihr zuliebe einen sprachlich und metrisch richtigen Text in etwas Falsches verwandelt" (*Prol.* 33). That is wishful thinking, and I very much doubt whether Carolingian scribes had quite that attitude to authority.

On the other hand, there is a fairly obvious solution to the problem, although it is not altogether comfortable for believers in the Method of Lachmann: not only O and Q, but Ω itself had corrections, and in a certain number of cases O chose the correct reading while Γ and one of O's correctors both chose the error. Correction, generally by collation against a manuscript's exemplar, was, as noted above, the norm in Carolingian manuscripts, and one corrector at least was quite explicit about the importance of proofreading. On fol 18ᵛ of BL Harley 2719 of Nonius Marcellus, there appears an annotation written in the third quarter of

---

Century, Cambridge MA 1971, 15. The use of such abbreviations (both ał and a capital A dotted on either side) is associated with Lupus of Ferrières, although it may be more widespread than that; for one list of such manuscripts, see E. A. Lowe, *Palaeographical Papers*, Oxford 1972, 320–22.

[15] P. Maas, *Textual Criticism*, transl. B. Flower, Oxford 1958, 3.

the ninth century, probably in Brittany (at least some of the other annotations by the same hand contain Breton glosses); it is found about a third of the way down the left-hand margin, adjacent to Nonius' lemma S*cripturarios ueteres* (38.1–2M):[16]

> Scriptores erant qui uenales codices faciebant, inde uictitabant eos
> (eis *Traube*) distractis. unde corrupti inueniuntur libri, quia non
> eos excurrebant (excutiebant *Traube*) nec recensebant cum aliis.

As Traube suggests, the author of this note (who, given the errors in his text, must be earlier than the note itself in its present form) is making a distinction between ancient scribes, who copied books for money and thus were not attentive, and modern (i.e. ecclesiastical) scribes, who are careful and collate their texts against other copies. That is clearly polemical, of course, but it does show that ninth-century scribes did think about collation and were proud of their carefulness; whether the ancient scribes were as inattentive as the author of the note says is much less certain. D. himself notes (*Prol.* 37 n.93) that "Eine Monographie zur mittelalterlichen Konjekturalkritik fehlt leider noch immer." None the less, much modern scholarship recognizes the heavy reliance of Carolingian correctors on the use of another manuscript rather than working *ope ingenii*. The practice of collation rather than conjecture, even when it imports nonsense, is evident in the Lucretius manuscripts as in other traditions, and Deufert recognizes it; but if Ω had variants, then those variants inevitably open a crack in D.'s Lachmannite ideal of a strictly closed tradition. Probably Ω was corrected against its own exemplar; but that (probably late antique) exemplar may itself have had variants—and they might possibly have come from a different strand of the tradition. Most of what D. eliminates from his apparatus is nonsense, and what is left is an excellent choice; what I emphasize here is that it is not altogether a mechanical choice purely on the basis of the Method, but relies on the judgment and (dare I say it?) instincts of an intelligent critic.

D. (*Prol.* 91) describes the editor's task in constructing the apparatus as separating the wheat from the chaff ("die Spreu vom Weizen zu trennen"), and that cuts two ways: on the one hand, he eliminates non-stemmatic readings and (unless they contain emendations) the readings of *codices descripti*, and on the other hand, he makes a serious effort to report and identify good or plausible conjectures. After the chaff/wheat metaphor, he continues (and the clarity of his statement is worth reporting verbatim): "Gleichzeitig ist es seine Pflicht, wenn möglich, den Erstentdecker oder die älteste Quelle für eine Konjektur zu bestimmen, einerseits um dessen Leistung zu ehren und andererseits um in Apparat die Fortschritte zu dokumentieren, welche bei der wissenschaftlichen Beschäftigung mit einem Text im Lauf der Zeit erzielt werden konnten" (*Prol.* 91). On the next page, he adds a corollary: that it is not

---

[16] These annotations were reported by W. M. Lindsay, "Spätlateinische Randglossen in Nonius," *ALL* 9, 1896, 598-99; this note was discussed and emended by Traube, *Regula Benedicti*, 77. I report it from my own inspection of the digitized copy,

http://www.bl.uk/manuscripts/FullDisplay.aspx?index=0&ref=Harley_MS_2719, accessed 25 May 2020.

important or worthwhile to report the name of a later critic, even a Bentley, who independently arrived at the same emendation as a humanist: "Damit ist für die Textkritik nichts gewonnen" (*Prol.* 92). And indeed D.'s apparatus largely follows these rules, and several elements of his practice are illustrated in the example from 2.998 discussed above. The negative side of it however, is severe: although he prints a great many conjectures in the text, including quite a lot of recent ones, there are relatively few he deems plausible enough to put in the apparatus.[17]

Brevity and clarity are worthwhile goals, particularly in an apparatus criticus, and it takes no small skill to compose one as lucid as D.'s. But brevity involves selectivity, and while I am not about to nominate alternative emendations that deserved to be cited, there is a real loss, and the assumptions D. makes in the sentences I quoted in the previous paragraph demonstrate part of what it is. The failure to name modern critics who independently discovered a conjecture made centuries earlier does not damage the text of Lucretius or our understanding of it, but if it is true that naming secondary emenders or citing interesting (if not always credible) conjectures made by scholars whom D. respects does nothing for *Textkritik*, it does a great deal for *Textgeschichte* and for our understanding of how the text came to be what it is. It helps to understand the value (to pick one of the most frequent names in the apparatus) of Lachmann's good emendations if one is able to see those of his that earlier editors accepted but D. does not. To suppress the names of seventeenth- or eighteenth-century editors because some of their emendations duplicate those of Marullus or Valla reduces our sense of the quality of their work and makes it more difficult to assess the worth of those conjectures that D. does report. To show that previous generations found a passage difficult and struggled to correct it is a part of our understanding of the difficulty and the meaning of the text itself.

Perhaps the most disturbing word in the passage I quoted above is "Fortschritte": D. presents the history of the text from its recovery in the ninth century up

---

[17] The treatment of modern scholarship in this respect is somewhat puzzling. Taking the first 500 lines of Book 2 as a sample, I found also-ran conjectures (excluding D.'s own) in 65 entries in the apparatus; in 49 of them a single alternative is given, and of those 49, 28 report conjectures made by Munro or earlier (9 of them by Lachmann himself) and only five have conjectures proposed after 1950. That is not the case with the few entries which contain many alternatives, and of the five entries including the greatest number of conjectures (on 2.42, 43, 88, 422, and 467) four concern obelized pasages and the fifth (2.88) concerns a passage that has attracted considerable modern attention: D. prints Courtney's *ad tergus ibi*, but (in addition to the old conjectures of I. Vossius and Goebel) cites conjectures made by D. Fowler, Butterfield, and himself. Every one of these five entries contains at least one recent conjecture, sometimes several. This suggests that unless he was overwhelmingly convinced of the truth of a modern conjecture, D. did not think many of them worth reporting as alternatives—very different from his treatment of Lachmann. D. also eliminates from the apparatus some conjectures that he thought serious enough to merit discussion in *Komm.*; I have noticed that in the case of Munro's *feruunt* at 4.608 and Butterfield's *blanda* at 5.1067 and I am fairly sure there are more examples, but I did not collect them systematically. My impression is that there are many recent conjectures that D. thought worth refuting in *Komm.* but not worth listing in the apparatus; conversely, there are also places where he puts in the apparatus conjectures (often older ones) that he did not think worth discussing in *Komm.*, including his own former deletion of 5.123 and Lachmann's of 6.608-38 or Muret's conjecture *labentis* at 5.989.

to the twenty-first century as an account of progress, and he eliminates those moments that he considers irrelevant to the onward march of Wissenschaft. It is obviously neither desirable nor possible to print every conjecture on every passage: the vast majority of the conjectures that have been made, at least over the past two centuries, deserve the oblivion to which D. consigns them. But D.'s apparatus sacrifices breadth for focus, and unsurprisingly his focus is on the text that he thinks best (his own), and on the kinds of emendations of which he approves. That kind of teleological approach is perhaps inevitable, but the result is that D.'s apparatus implies a narrative written backward, a history of criticism of Lucretius intended to explain and justify his own text. That kind of Whig history has long been discredited in other historical disciplines, and while the construction of any critical text and apparatus requires choices and limits, to pluck from the past those things that suit our own ideas and to ignore those that do not quite fit often leads to oversimplification.

Such a broad and censorious conclusion is perhaps too much to infer from a few words in *Prol.*: oversimplification is in certain respects a necessity, and any scholarly production, perhaps a critical edition most of all, requires a certain self-fulfilling back-to-front argumentation: we report scholarly opinions (or emendations) that coincide with our own ideas, and dismiss many others as irrelevant or wrong. And that is something no less true of D.'s work than of others' (including, I assume, my own), but it appears in a more emphatic form here than in most scholarship I know. In his comparison of the humanistic emendations that can be attributed to the two sources he labels α (probably in large part the work of Antonio Beccadelli) and φ (probably the work of Lorenzo Valla), D. makes it very clear which one he prefers, and why. φ he describes as a professional philologist (*Prol.* 140), emphasizing discipline and method: "In der Summe weist die energische, zielstrebige und zugleich behutsame, am Sinn und Sprachgebrauch des Lukrez orientierte Textkritik den hinter φ stehenden Gelehrten als einen scharfsinnigen, mit Lukrez gut vertrauten und vor allem methodisch diszipliniert arbeitenden Philologen aus." By contrast, in the case of α "denkt man tatsächlich eher an einen Dichter als an einen Philologen" (ibid.). Beccadelli was indeed a poet, and Valla was indeed one of the greatest scholars of his age. But it is the terms of praise and blame that are most interesting: a poetic sensibility has little place in emending a text, even a poetic text; method and philology are the framework for textual criticism.

3. *EMENDATIO*

I do not mean in the least to deny the importance of traditional philology in constructing an edition of a classical text; far from it. And D. is an excellent philologist, with a wide and precise knowledge not only of Latin meter, style, and diction, but of the entire tradition of Lucretian scholarship; and as a result, he has emended the text of Lucretius in a great many passages, using both the work of earlier scholars and his own abilities. As expressed in *Prol.*, D. genuinely believes in the importance of giving credit to the authors of successful conjectures; the result appears not only in *Prol.*, but in his citation of undervalued or forgotten

emendations made by earlier commentators. *Komm.* makes it abundantly clear that he has studied and thought about his predecessors. Lambinus in particular receives D.'s admiration: at 5.1353, adapting one of his conjectures, he refers to "Lambinus in seinem bewundernswert gelehrten Kommentar"; at 6.502, he follows Lambinus, whose deletion of the verse is "zu Unrecht in Vergessenheit geraten." Of Bentley's emendation at 6.674 of *fluuius qui uisus est maximus* to *fluuius quiuis est maximus* (generally printed until banished by the very conservative editor Johannes Vahlen), he calls it "diese unvergleichbar elegante Konjektur." He is not uncritical, however, even of his heroes: at 6.1204 he describes Bentley's tone ("Sed heus tu, an *totum corpus fluebat per nares*? ..." [D. quotes it at greater length]) as "an die Stimme eines Kindes erinnernden Empörung uber die verletzte Logik"; in his discussion of later critics' attention to the history of Lucretian criticism (*Prol.* 144), he praises Munro for his comments on his predecessors and contrasts them with the excessively nasty comments made by Lachmann himself about recent editors in his commentary, a polemic "die Lachmanns zeitlosem Werk wie wertloser Modesschmuck anhängt."

D. himself is, for the most part, very respectful of his predecessors; he obviously rejects some conjectures, but the harshest criticism I have found of any individual's suggestion is "schulmeisterlich" of Büchner and Butterfield at 6.490, and there are occasional comments about other scholars' pedantry. One of the most admirable features of D.'s scholarship (for obvious reasons more visible in *Komm.* than in the text itself) is one's constant sense that he is reading his predecessors with care and sympathy, looking for what is good rather than (in the manner of a Housman or Bentley) seeking objects for ridicule, and frequently giving long and careful explanations of the philological issues involved in a textual question. The text of Lucretius is seriously corrupt, and as noted above, D. has printed hundreds of the conjectures made in φ (Valla); he has also printed in the text no small number (I have not counted) of other emendations. Of the conjectures of other critics that he places in the text, the following seem to me most likely to be right:[18]

> 1.175 *uuas* (Pontanus) for transmitted *uites*; 1.555 *florem* (Marullus) for *finis* or *fine* (but in the same line Lachmann's *ad* is preferable to Castiglioni's *in*); 1.744 *rorem* (Christ) for transmitted *solem*; 1.977 *officiatque* (*ed. Basil.*) for transmitted *efficiatque*; 2.219 *deflectere* (Watt) for *depellere*; 2.250 *sensu* (Giussani) for *sese*; 2.515 *gelidas iter usque* (Lachmann) for *gelidas hiemisque*; 2.734 *induta* (Lambinus) for *imbuta*; 2.1029 *mittant* (Lachmann) for *minuant*; 3.84 *fundo* (Lambinus) for *suadet*; 3.492 *agens spumas animam* (Zwierlein) for *agens animam spumas* (with the consequent change of the necessary supplement that follows from <ut> to

[18] I include in this list no conjecture which is printed in either Bailey's OCT or Martin's Teubner. It will be obvious that my list is both incomplete and subjective. I have not given arguments here, as by and large I was convinced by D.'s; see *Komm.* D.'s own conjectures are considered separately below.

<quasi>); 3.852 *ut* (Susemihl; ascribed to Heinze by Martin) for *et* or *te*; 4.179: *momine* (probably Marullus) for *numine*;[19] 4.532 *expleta his* (K. Müller) for *expletis*; 4.568 *accidit* (Lambinus) for *incidit*; 4.608 *feriunt* (Lachmann) for *fuerunt*; 4.632 *ualidum* (A) for *umidum*; 4.878 *quareue* (Merrill) for *uareque*; 4.1096 *mentem spe raptant* (K. Müller following Bentley's *mentem spe captant*) for *vento spes raptat*; 5.600 *conlectus* (unnamed friends of Lambinus) for *coniectus*; 5.1273 *tum* (Lachmann) for *nam*; 6.11 *posset* (Lachmann) for *possent*; 6.475 *ollis* (Lachmann) for *omnis*; 6.490 *nimbis* (Lachmann) for *montis*; 6.800 *ex epulis* (Brieger) for *efflueris*; 6.1262-63 *aestu / confectos* (Susius) for *aestus / confertos*.

Several of D.'s own emendations, moreover, are probably correct:

At 1.105, where the transmitted text has *uitae rationes uertere*, D. observes both that *ratio* is always singular when applied to a way of life and that while other poets use *uertere* for *euertere*, Lucretius never does; he emends to *rationem euertere*. At 2.474, *umor dulcis, ubi per terras crebrius idem / percolatur*, he rightly draws attention to the peculiarity of the contorted construction (and punctuation) proposed by Munro; following Gifanius and Lambinus, D. sees that a verb of some kind is needed, but rather than emending *dulcis* to *dulcet* (Lambinus), he deletes *umor* as a gloss and emends to *nam fit dulcis*, which at least gives the passage sense. At 3.288-9 *in iram / cum feruescit* for *in ira* (following a suggestion of Kenney on 3.295) makes more sense, as does *cum summo de corpore* for *de summo cum corpore* at 4.84. At 4.875, the manuscripts read *sic igitur tibi anhela sitis de corpore nostro / abluitur*; not only is the combination of *tibi* and *nostro* odd, but D. points out the repetition of *nostro* from the end of 872, and emends 875 to *corpore toto*. At 4.1037, the transmitted *sollicitatur id nobis* is metrically defective, and the fifteenth century supplement *id <in> nobis* is printed by Bailey and Martin; D.'s *item* makes the sentence more logical as well as metrical. At 5.1353 Lambinus realized that *insilia* meant nothing in the context of weaving implements (or perhaps anywhere else) and conjectured *insubula* on the basis of a passage in Isidore; D. alters this to *insubla* on the grounds of prosody, as the u is apparently long. Several of D.'s other conjectures are plausible, but not so immediately convincing: *horae consistere* for *hora sistere* (or the commonly printed emendation *horai sistere*) at 1.1016; *quae quantast* for *quamquam* at 2.181; *hoc tibi si sumas* for *sumant oculi* at 2.547; *quacumque id mente uolutat* (or, and I think more appealing, Deufert's alternative suggestion *uolutas*) for *quaedam quae mente uolutat* at 3.240; and *interque* for *saeuosque* at 3.306.

[19] Note however the error *momine* for *numine* in the middle of *Komm.* 215: there are too many small typos in *Komm.*, but this is the only one I have found that affects D.'s meaning seriously.

D.'s argument in favor of the last of these is more ingenious than convincing; I suspect that it, like the emendation at 3.240, belongs in the apparatus with the note *fort. recte.*

On the other hand, some of his emendations are simply unnecessary or at best unlikely:

At 1.752, D.'s supplement *certum* is no better than the standard *rebus*, and at 3.1061 *recedit* is no better than the older conjecture *reuertit.* At 5.568, D. prefers the word order *nil his illa* to Bernays's perfectly reasonable *nil illa his* on fairly specious palaeographical grounds; at 5.1270 his rearrangement of the transmitted *quam ualidi primum* to *primum quam ualidi* is unnecessary. Some of D.'s conjectures are somewhat pedantic: reversing *terra* and *caelo* at 1.161-2 is ingenious, but excessively rationalistic, and at 2.462 *laedens sed rarum* is perhaps a good diagnostic conjecture but has no place in the text, while at 4.582 D.'s *montiuago* for the transmitted *noctiuago* is completely unnecessary,[20] and at 5.514 D.'s *inferni* for the transmitted *aeterni* is very unlikely. D. objects to the "unexampled poetic liberty" in using *aeterni* to describe a world in which nothing (other than atoms and void) is eternal, but Lucretius is in fact a poet, and if any change is needed, Merrill's *nocturni* is distinctly preferable. At 3.632 *anima* in the manuscripts is corrupt; a dative is necessary and most editors print *animae*; D., however, chooses to emend to the plural *animis* because the word is embedded in a nest of plural organs. Those plurals are natural pairs (eyes, nostrils, ears), and a singular hand and tongue are also found in the same context. Why more than one soul? As D. himself says, *animis* is "no less probable" than *animae*, but that is not a good reason to emend. At 3.514, D.'s emendation of *hilum* to *hili* merely substitutes a morphological anomaly for a syntactical one; at the end of 4.989 the transmitted *saepe quiete* is clearly wrong (it also appears at the end of the next verse, where it makes sense), but *membra ciere* (a joint conjecture of D. and his colleague K. Sier) is no more likely than, e.g., Büchner's *corpus ciere.* So too at 4.633 the transmitted *ut uideamus* is corrupt, and D. is right that the word *aptus* belongs somewhere in this space, but the conjectures he proposes (*aptus et almus* in the text, *ut fuat almus/aptus* in the apparatus) look plausible only in comparison with some other proposed emendations, such as Bailey's *atque uenenum.* At 1.453 and 3.962 D. would have done better to obelize than emend.

In at least four places, however, D. leaves in the apparatus conjectures of his own that might well be placed in the text: at 4.77 *sublata* for the transmitted *uolgata* is a very good idea; at 5.947 for the corrupt *claricitatiate* D. print's Zwierlein's *clarus agit late*, but *ago* is the wrong verb here, and D.'s own *claru'*

[20] For this conjecture cf. Deufert, "Zu den gegenwärtigen Aufgaben der Lukrezkritik," *Hermes* 138, 2010, 67-69.

ciet late (modifying a suggestion of Forbiger) is much better. At 5.257, D. follows Brieger and K. Müller in positing a lacuna after the line, but D.'s own suggestion of *praeterea* for *sed terrae* is a far better solution and belongs in the text, as does his proposal of *quod fieri* for *conloquium* at 4.598, where D. discusses, but does not emend, the mixture of senses that appears in the combination *conloquium . . . uidemus*. He seems to prefer to emend *uidemus* to *ubi habemus* (modifying Lachmann's *ubi demus*), which he also leaves in the apparatus, but that is clearly less effective.

I could continue with lists of emendations good, bad, and indifferent that D. has accepted or rejected; just as with his own conjectures, he prints in the text some emendations made by other scholars that were best forgotten and leaves in the apparatus some conjectures that would be better placed in the text. But although D.'s textual choices in his edition obviously matter, his great merit as an editor lies in the arguments that he makes in *Komm.* about his choices. For while D. is a good textual critic, he is an excellent exegete. Thus, while he prints a great many emendations in the text, he also does a superb job of defending the transmitted text; I noted in *Komm.* about forty passages (and there are more) in which he fends off unnecessary emendation, often with excellent discussions of the linguistic or stylistic problems that provoked it.[21] A sample: a very careful discussion of apparently solecistic changes of gender in mid-sentence (1.190, to which D. refers back several times later); a discussion of *efficio* plus the subjunctive without *ut* (2.1005); a defense of *suauis* in connection with fainting, including a report of a consultation with a medical friend (3.173), a good companion to his citation not much later (3.198) of a "schöne Anmerkung" by Merrill about the commercial value of poppy seeds. He also engagingly cites Merrill at 2.356 for the latter's conversation with a California cowherd about cows looking for their calves. At times, D. uses unimportant variants or unlikely conjectures as pegs on which to hang careful and valuable discussions of prosody, syntax, or style: on the prosody of *ubi* (3.728) and *semota* (4.270); on the syntax of *fama* (1.68) and *fit ubi* (6.145); on the usage of *quamuis* (2.177-81); on the use of *-ue* for *-que* in some questions (2.1099); on postponed *-que* (5.1205) and the position of *enim* (6.1277); on nouns with singular and plural of different genders (6.483).

At the same time, however, the very clarity of D.'s discussions in *Komm.* sometimes makes a certain weakness and rigidity in his argumentation apparent, and it reveals some of the limits of what he thinks relevant to the examination of the text. Thus, between 3.860 and 861 D. posits a lacuna, because *enim* in 861 ought to introduce an explanation of what comes immediately before in 854–860 and that therefore something must be missing. The difficulty, as D. says, has generally been recognized; but both Bailey and Heinze, while acknowledging

---

[21] Less common, but also important, is D.'s close attention to readings in the indirect tradition: at 1.66 he rightly prints *tendere* (Nonius) rather than the manuscripts' *tollere*; also from Nonius are *candenti sole* for *dispansae in sole* at 1.306 (so also Butterfield) and *lauit* for *pauit* at 2.376. D. follows Macrobius in reading *tecta* rather than *animum* at 2.28 and *torrens* for *torret* at 5.215, while rejecting Macrobius' *uirentes* for *uigentes* at 2.361. At 1.932 he prints *animos* (Lactantius) rather than *animum*, and at 6.233 he accepts Martianus' *uasi* for *uasis*.

the problem, both give reasonable explanations of a broader use of *enim* looking back to a larger range of argument rather than just to its immediate context. No lacuna is in fact needed; but for D. a rule is a rule. That is something made remarkably clear at 5.977, where the transmitted text is *a peruis quod enim consuerant cernere semper*. D., like Bailey and Martin (and many other editors) prints *a paruis*, a correction made by the fifteenth-century corrector of Q, but D. also resurrects an alternative humanist emendation, *a pueris*, only to reject it because there is a clear parallel to *a paruis* in Clarke's emendation, which D. accepts, of *puri* to *parui* at 4.1026.[22] D. refers to this argument as "das Prinzip textkritischer Ökonomie." Does that mean that because Lucretius (perhaps) refers to children as *parui* in one passage, he must refer to them the same way in another?

There is another set of rules regarding the preferability of one reading over another, and that involves palaeographical probability, the principle that the closer an emendation is to the letters of the transmitted text and the easier the confusion that would lead from the correct (emended) text to the mistaken (transmitted) text, the likelier the emendation is to be correct. For D. that seems to be a fairly important principle. Thus, at 5.1160 the transmitted text *et celata in medium* is clearly corrupt, and D. himself has proposed two different emendations. In *BT*, he prints *celatam <uim> in medium*, while in 1996 he conjectured *celata sibi* (*Ps.-L.* 257). His earlier conjecture is better, as *uim* is too precise in the context; but better than either of D.'s suggestions is Lachmann's *celata mala*. D. rejects it because he sees it as not precise enough, but he also describes it as "ein wenig willkürlich," apparently because it is not close enough to the transmitted text. Here his language about what is wrong with Lachmann's conjecture is vague; it is more precise elsewhere. Thus, at 6.899 the transmitted text, with the participle *tenentes* describing the seeds of fire concealed in the wood of torches, is clearly (despite Lambinus) wrong, and D. rightly accepts Bernays' conjecture *latentis*.[23] But because he finds the (also clearly wrong) conjecture *natantes* closer to the paradosis ("paläographisch leichter"), he feels compelled to invent a remarkably strained palaeographical explanation for the change from *latentis* to *tenentes*.[24]

There are other examples. At 6.1064 *magnesia flumina saxa* in the transmitted text is supposed to describe the emanations from magnetic stones, but something is wrong. Lambinus suggested *Magnesi flumina saxi* and Bentley, importing a fairly violent enallage, proposed *Magnesia flumina saxi*, which D. prints. And yet, as D. himself points out, Bentley believed that

---

[22] M. L. Clarke, "Lucretius 4.1026," *CQ* 34, 1984, 240. Clarke is right, as others before had seen, that *puri* is hard to interpret as "children," which must be the sense, but he rejects Lambinus' much better *pusi* on the grounds that it is only attested in an epigram of Papinius quoted by Varro, *LL* 7.28. But there are many instances of the diminutive *pusillus*, and Cicero refers to Clodius sleeping with his big sister as *pusio* (*Cael.* 36). I distrust rules, but *lectio difficilior* surely applies here.

[23] So too Bailey in the OCT, reverting to *tenentes* in his larger edition.

[24] D. himself was more skeptical of palaeographical arguments in 1996: see *Ps.-L.* 11-12.

Magnesia flumina saxi was attested in manuscripts (*Quidam Codd.*), and the reading he was correcting was *Magnesi semina saxi*—and his reading is certainly better than that. But Lambinus' emendation is far better, and D.'s preference for Bentley's conjecture because it is closer palaeographically is very weak: Bentley's reading changes one letter, Lambinus' two. So too at 6.600 D. supports Lachmann's *atque* as an emendation for the transmitted *idque*, and he rejects Butterfield's *seque* (which he says he too once considered), despite its being "inhaltlich gut," because it is "ohne äußere Wahrscheinlichkeit" while Lachmann's is sufficient. This external probability is clearly the argument from palaeography, but Butterfield is certainly right here, and D.'s overemphasis on palaeography gets in the way of sense, as it does at 6.972, where D. prints Lachmann's *quod amarius frondeat esca* because it is palaeographically more plausible (the transmitted text is *frondeac ex(s)tet)*) than D.'s own *frondibus exstet*—which is, in fact, distinctly preferable and belongs in the text. As noted previously, at 5.257 D., following K. Müller, thinks there is a lacuna, but D.'s own emendation of *praeterea* to *sed terrae* is a more elegant solution, even if, as he says, it has "keine äußerliche Wahrscheinlichkeit"—again, palaeography trumps sense.

The possible confusion of letter forms is certainly one justification, and a fairly frequent one, for preferring one conjecture to another, but it is hardly the only reason that mistakes are made, and thus while it is a useful secondary argument it is not in itself a particularly strong argument in favor of an emendation: as Havet rightly said in his (too rarely consulted) *Manuel de critique verbale*, "le souci de la vraisemblance graphique ne doit pas faire oublier les autres critères."[25] Similarly Maas: "The main business, that of determining what is either tolerable or absolutely required from the point of view of style or content, will not be materially advanced by our perceiving what errors are more or less probable."[26] D. himself gives a fine example of what is wrong with this kind of mechanical argument at 6.1012. There, the manuscripts give *quod dicitur ex elementis*; D. corrects this by accepting Lachmann's *quo ducitur ex elementis*. But D. himself rightly observes in *Komm.* that *ex elementis* may well have been taken by a scribe from the end of 6.1009 just above (*<ex> elementis*), "so dass auch die Paläographie keinen Anhaltspunkt bei der Heilung zu geben braucht." Even with Lachmann's emendation, the phrase is in context very forced and quite unnecessary, and D. discusses various emendations that improve the sense but are not palaeographically plausible, e.g. Lambinus' *quod paulo diximus ante* or (preserving *quod dicitur*, which is probably a good idea) Diels's *quod dicitur angere multos*. Neither of these is perfect, but there is no reason for D. to conclude his long (3 pages) note by contradicting his own earlier statement that palaeography might not be helpful here, by saying that such conjectures reject *ex elementis* "ohne echten Grund," and deciding to print Lachmann's conjecture even if "eine letzte Unsicherheit nicht ausgeräumt weden konnte." Throughout,

[25] L. Havet, *Manuel de critique verbale appliquée aux textes latins*, Paris 1911, 157.
[26] Maas, *Textual Criticism*, 13.

D. is concerned with methodological purity, which means following certain rules even though he knows perfectly well that at times (as at 6.1012) they are not only useless, but harmful.[27] The great Homeric scholar Karl Lehrs composed a set of ten commandments for philologists, of which the third and fourth are relevant here: "Du sollst nicht vor Hanschriften niederfallen" and "Du sollst den Namen Methode nicht unnütz im Munde führen."[28] A. E. Housman put it more trenchantly at the end of his lecture on "The Application of Thought to Textual Criticism": "Knowledge is good, method is good, but one thing beyond all others is necessary; and that is to have a head, not a pumpkin, on your shoulders, and brains, not pudding, in your head."[29]

Methods, in the abstract, are of course good: they provide a framework for structuring our research, and they give us sets of rules which we both follow and reject: stemmatics, as I suggested above in discussing D.'s apparatus, does not always lead to a single answer, nor does palaeography always supply suitable grounds for choosing an emendation. And D., although he tends to follow rules, is too good a critic to ignore the limitations of scholarship-by-algorithm. What is more damaging than some of his less than ideal textual choices described above, however, is that he privileges one set of parameters—the rules of formal philology—over others that are equally, or perhaps even more, important in editing an author like Lucretius. Consider, for instance, D.'s treatment of 1.412, which he prints as *usque adeo largis haustos e fontibus amnes / lingua meo suauis diti de pectore fundet.* This is Bentley's emendation for what is transmitted as *largos haustus e fontibus amnes* (or *magnis*).[30] The image of the poet's tongue pouring *rivers* from his chest that he has sucked up from large springs is, to put it mildly, unappealing. D.'s preference for *amnes* over *magnis* is justified in part by a peremptory comment that *magni fontes* does not fit with the Callimachean *Bildersprache* that Lucretius uses (he also believes that while *magnus* refers only to size, *largus* has a broader meaning). But although D. recognizes a Callimachean background, he does not explore it at all. Those *magni fontes* reappear in Propertius in a highly Callimachean passage which D. does not cite, *paruaque tam magnis admoram fontibus ora / unde pater sitiens Ennius ante bibit* (3.3.5-6), and (as both Munro and Bailey note) the combination *meo . . . diti de pectore* in the next line of Lucretius is an Ennian archaism. R. D. Brown rightly described Lucretius' large springs, as "notably un-Callimachean": Lucretius' language suggests that we are in the middle of a literary polemic involving both Ennius and Callimachus, and that is surely relevant to any determination of the correct text.[31]

---

[27] Again, D. was less rigid in 1996, quoting with approval another dictum of Housman (A. E. Housman, ed., *M. Manilii Astronomicon Liber Primus*, London 1903, liii-liv): "An emendator with one method is as foolish a sight as a doctor with one drug" (*Ps.-L.* 25 n.119).

[28] I cite Lehrs from W. M. Calder, "Karl Lehrs' Ten Commandments for Classical Philologists," *CW* 74, 1980-81, 227-8.

[29] A. E. Housman, *Selected Prose*, ed. J. Carter. Cambridge 1961, 150.

[30] The first hand of O reads *magnes*, an early corrector *magnis*, while Γ reads *amnes* and O³ *amnis*. Both Bailey and Martin print *usque adeo largos haustus e fontibus magnis*.

[31] R. D. Brown, "Lucretius and Callimachus," *ICS* 7, 1982, 94 n.32.

Just as important for determining the text as the literary context, indeed perhaps more so, is the philosophical context. At 1.469, Lucretius is talking about time being an accident of the elements rather than a primary element itself and discusses the status of historical events such as the Trojan war; the transmitted text reads *namque aliud terris, aliud regionibus ipsis / euentum dici poterit quodcumque erit actum.* That there is something wrong here has long been suspected: *terris* and *regionibus ipsis* mean much the same thing. Munro recognized that whatever the two terms in 1.469 are, they are particular paraphrases for the more general terms *materies* and *locus ac spatium* used in 1.471-72. But Munro's conjecture *Teucris* for *terris*, which D. adopts, is truly misguided: while *regio* is an adequate substitute for *locus*, time is certainly not an accident of people who, as the two previous lines make perfectly clear, are themselves no longer extant. *Terris*, therefore, must conceal a word for matter. I have no idea what it is—none of the obvious words is metrically possible— and perhaps *terris* should be obelized. But a little attention to the Epicurean theory of time might have helped Deufert at least recognize a conjecture that is philosophically inadequate. So too, at 4.43 the transmitted text reads *de cortice eorum* while D. prints *de corpore rerum* without discussion other than a cross-reference to his treatment of the uses of *rerum* at 1.190. Not only (as he does not say at 1.190) is *rerum* at 4.43 an unnecessary emendation by Lachmann, but there are good philosophical reasons, of which D. makes no mention, for preferring *cortice* to *corpore*.[32]

One final type of textual problem deserves mention here, one that is of much more concern to Deufert than matters of literary history or Epicureanism: prosody and meter. What is striking here, however, is that D., perhaps because he seems truly expert in this area, seems less sure of the validity of rules and more inclined to admit authorial variation. Thus, at 6.652, *nec tota pars, homo terrai quota totius unus*, D. is troubled by the quantity of the final syllable of *homo*, a word which is normally iambic rather than pyrrhic. One solution, of a kind D. favors, is to transpose words within the line in order to eliminate the irregularity: *quota homo terrai totius unus.* Here, however, he leaves that in the apparatus and in *Komm.* addresses primarily the prosodic problem. Final -o in Lucretius is only shortened (iambic shortening) in pronouns and adverbs, not in the first-person singular of verbs or in other nouns ending in -o,[33] and iambic

---

[32] So D. Sedley, *Lucretius and the Transformation of Greek Wisdom.* Cambridge 1998, 40-1. Another philosophical puzzle which D. does not discuss in *Komm.* (and which I mention only briefly here, as I am no philosopher) is the transposition (which goes back to the fifteenth century) of 1.434 and 435; D. in the apparatus gives a peremptory reference to Lachmann's solution of the problem. Lachmann could solve the grammatical problems of the passage well enough, but D. ought to have at least looked at the philosophical aspect of the lines, which has to do with the nature of the void and how one talks about it. Sedley has suggested that *augmine* in 433 means not "size" but "extension," and if so that 434 and 435 are in the proper order in the manuscripts. See D. Sedley, "Two Conceptions of Vacuum," *Phronesis* 27, 1982, 189-90.

[33] Although there is an obvious difference between *homo, hominis* and D.'s other examples, *leo* and *draco*, which both have long -o- in the stem.

---

homo is found at 1.66 and 6.676; pyrrhic *homo* is attested in colloquial contexts in Catullus, but does not fit the loftier style of this passage of Lucretius. And yet there is one relevant passage that suggests that Lucretius knew and used pyrrhic *homo*: at 3.925 *homo* is elided into *ex*, and Lucretius strenuously avoids the elision of iambic words (other than pronouns, where the final syllable is regularly variable). What is striking here is that D. confronts a conflict between two rules: the prosodic rule that final -o in nouns and verbs is not shortened, and the metrical rule (promulgated by Lachmann) that iambic words in Lucretius are never elided. Of course the transmitted text of Lucretius provides some, very few, examples of elision of iambic words; Lachmann solved that problem by simply emending the exceptions out of existence.[34]

There is one instance of iambic elision, however, that D. finds harder to dispose of, and he refers to it in his discussion of *homo* at 6.652. This time, the problem is not a man, but a horse (in Lucretius' discussion of the impossibility of centaurs): at 4.741, the transmitted text gives *uerum ubi equi atque hominis casu conuenit imago*, requiring the elision of the iambic word *equi*. Lachmann's solution is to transpose the words to *uerum ubi equi casu atque hominis*, but D. (like Bailey and Martin) rejects that and instead accepts Munro's suggestion that what he calls "the tangled sound" is meant to reflect the entangled condition of the simulacra creating the image of a centaur: "Die metrische Außergewöhnlichkeit ist also durch den Sinnzusammenhang gerechtfertigt," as he also accepts the prosodic anomaly at 6.652—which also allows him (if I understand his somewhat elliptical sentence) to avoid having to decide which rule the elision of *homo* at 3.925 violates.[35]

The use of pyrrhic *homo* at 6.652, however, has a larger context of prosodic anomaly which D. recognizes but does not emphasize. At 6.1135 the transmitted text has the word *corumptum* at the end of the line; that is easily corrected to *coruptum*, but the use of a short first syllable in the word normally spelled *corruptum* is, to say the least, unusual. D. points out the accumulation of prosodic oddities in this part of Book 6—which includes the equally odd *Brittannis* at 6.1106.[36] In fact, Book 6 as a whole contains a remarkable

---

[34] Munro (on 4.741) is rightly critical of Lachmann's tendency to remove metrical anomalies in Lucretius by editing them out. Against Lachmann's anomaly-removing conjecture *sua* for *suo* at 4.472 (printed by D. and many others), see Sedley, *Lucretius*, 47.

[35] Stylistic judgments as tools for textual criticism are fairly rare in D. He decides against seeing 4.598 as an interpolation (Lambinus did), because "die pointierte Formulierung spricht eher für den Dichter selbst als für einen Interpolator"; by contrast, he deletes 4.788-93 despite the poetic quality of the verses, while at 6.260 he refrains from deleting a line which he says does not fit the imitation of Homer in the surrounding lines, because the verse "an sich tadellos und schön ist."

[36] Both *coruptum* and *Brittannis* are cited as anomalous by one of the versions of the grammatical text ascribed to "Sergius". I should have included this text in my list of Sergii at Zetzel, *Critics, Compilers, and Commentators*, Oxford 2018, 321 (Servius #5) and I look forward to reading it in the edition of L. Munzi, "Spigolature grammaticali in una silloge scolastica carolingia," *BollClass* 14, 1993, 103-32 (from which D. cites the text), once libraries reopen.

---

collection of such anomalies:[37] it includes pyrrhic *homo*, the anomalous initial short syllable of *Cecropis* at 1139, and the scansion of *aqua* either with a long first syllable or as trisyllabic at 6.552 and 6.1072.[38] Although D. does not group these four instances with *coruptum* and *Brittannis*, they belong together, and D. (speaking only of the two examples he mentions) is quite sure that only the poet himself, not an interpolator, would indulge in two prosodic pecularities so near to one another.

The concentration of prosodic and morphological irregularities in the latter half of Book 6, which D. describes as "Nachlässigkeit" lends itself to another interpretation, but it is one which D. is very much unwilling to consider: that it is not inattention, but incompleteness, that leads to the difficulties of the end of the poem. D., like other critics, sees how disordered the last portion of the poem is. He recognizes that 6.1225, *incomitata rapi certabant funera uasta*, does not belong where it is, at the conclusion of a section on the deaths of animals; he makes a long and complex argument about the structure of the section and moves 1225 33 lines further on, to follow 1258. Displaced lines, as D. knows, rarely migrate that far away, and generally appear in the manuscripts after their proposed new location, not before it: D.'s argument does not make it plausible to move 6.1225 that far, and others have moved the line to follow 1234 (Munro) or 1246 (Lachmann), while K. Müller deleted it as an interpolation. That may be right; but it is also plausible, with Munro, to see it as an incomplete scrap. D. does better with his exposition of the problems surrounding 6.1247-51, which Bockemüller thought belonged at the very end of the poem, after 6.1286. D.'s explanation of textual problems and the structure of the argument is detailed and careful; but the need for such elaborate (not to say convoluted) explanations suggests that in fact the conclusion of the poem never had perfect logic and

---

[37] D. himself identifies a small region of anomalous prosody at 6.1065-89, which includes the peculiar *singlariter* at 1067, *colescere* at 1068, and *coplata* at 1088. For some reason here he does not mention *aquai* at 1072, and his other examples are outside the boundaries of this passage. Of the two explanations for the anomalies that he offers, he prefers "eine gewisse Nachlässigkeit" to mimetic stylization of some kind.

[38] D. prints *acuae* and *acuai*, treating -u- as syllabic and not as a semivocalic part of the normal -qu- combination; in this he follows Lachmann, up to whose time it was customary to treat the first vowel of *aqua* in these passages as artificially lengthened rather than make -u- syllabic. The third possible instance of this peculiarity also comes in Book 6, at 6.868, where the manuscripts read *laticis tactum atque uaporem*, but the grammarian Audax (7.329.6K) and, drawing on Audax (or, less probably, one of Audax's sources), Bede, *De arte metrica* 7.253.19 quote it with *aquae* rather than *laticis*. The grammarians scan *aquae* as a spondee, arguing that in such instances -qu- is treated as a double consonant lengthening the first syllable. D. brushes aside the reason for Audax's quotation and chooses to treat *aquae* as trisyllabic, against all evidence; and he also chooses to print it (as *acuae*) rather than *laticis*, which he views as an interpolation even though, as Bailey notes, "*laticis* is a most unlikely gloss on *aquae*." A stronger argument—although not one that D. would be likely to use—for *aquae* (either anapaestic or spondaic) rather than *laticis* is that it is followed at 6.872 and 874 by *aquai* with the properly short first syllable, just as at its first occurrence at 6.552 it is followed in 6.554 by iambic *aquae* (unfortunately, there is no such convenient pattern at the third occurrence). This suggests a kind of learned game played by the *poeta doctus*.

structure; along with the prosodic peculiarities, it might just imply that *De rerum natura* was never quite finished.

4. Incompleteness and interpolation

The question of whether Lucretius actually completed *De rerum natura* is not unimportant, even though it is not definitively answerable either: if there are passages as rough as the concluding section of Book 6 is generally felt to be, then in some cases the validity of emendation becomes problematic because the text itself may have been problematic from the outset. In the case of Lucretius there is not an indication of incompleteness as powerful as the incomplete verses of the *Aeneid*,[39] but for many critics the cumulative impression conveyed by repetitions, awkwardnesses of argument, and—perhaps the single most striking evidence—Lucretius' unfulfilled promise at 5.155 to discourse at length on the nature of the gods is enough to demonstrate incompletness fairly conclusively. D., however, is not one of those critics, and so far as I can see he never allows the word "incomplete" to appear in the same sentence as Lucretius or *De rerum natura* in any of the three volumes under discussion, for one crucial reason. The approach to the textual criticism of Lucretius with which D. is most closely identified, going back to his 1996 dissertation, is the belief that a large number of passages—repetitions, intrusive elements, awkwardnesses of argument— were written and inserted into the poem by one or more interpolators. D. has modified his belief with respect to some particular passages. According to his own reckoning, in *Ps.-L.* D. proposed to condemn 368 verses distributed over 92 interpolations, but he has subsequently come to regard his youthful judgment as "überkritisch" and reprieved about 150 verses;[40] on the other hand, he has now excised an additional dozen passages comprising 25 more verses.

D. is certainly right that there are interpolations in the text of Lucretius, as there are in the text of virtually every Latin author, including Virgil, Cicero, and even lyric and elegiac texts, but not all interpolations are alike, nor do they necessarily arise from similar causes or at similar periods in the transmission of texts. By my count (which is probably slightly off), there are in D.'s text some 62 or 63 passages marked as interpolations; I am a conservative critic, but even I think that 24 of them should indeed be deleted, and another 10 (including 1.44-49) should probably go as well. But of the 24 that seem definite to me, fully ten involve repetitions or very close variations of lines in the immediate vicinity, and are thus likely to be scribal errors rather than deliberate interpolations; two of them have the look of rubrics—related to the *tituli* that appear throughout the text—that have wrongly been interpreted as part of the text. That leaves a dozen that are deliberate interpolations, annotations, or something of the kind, and only one of that dozen is longer than a single verse. Of the ten further plausible interpolations, other than the intrusive description of

---

[39] Cf. *Ps.-L.* 20.

[40] *Ps.-L.* 305, *Komm.* V. After much counting, I am still unable to match D.'s numbers exactly. I presume that his are correct, but by my count he has reprieved 29 passages and some (but not all) verses in 6 more; the total number of reprieved verses (again, by my count) is 139.

the gods at 1.44-49 (which, however, many critics think genuine, although I do not), two (2.760-62, 5,210-12) seem to me doublets of adjacent sets of lines which, again in my subjective opinion, look more like interpolations than authorial second thoughts, and of the remainder all but one are single lines.

What my list of correct or plausible deletions does not contain, however, is any passage longer than six verses, and very few longer than two. D.'s hit list contains eight passages of ten lines or more and seven of between four and nine lines; but it is also worth noting that of the 29 passages he has reprieved, fifteen—more than half—are of four or more lines. One has the impression that D. too has begun to have doubts about longer interpolations: he no longer thinks that repetition is almost inevitably a marker of interpolation; he has been convinced by others' arguments that certain passages fit coherently where Lucretius (and not an interpolator) put them; he believes that he was, as I quoted him earlier, "hyperkritisch." And he is right.

D.'s own changes of mind are in fact a strong argument against finding wholesale interpolation in Lucretius: why should we think that his reasoning about passages he still deletes is any better than he now thinks his reasoning was in 1996? A reversal of opinion in something like 30% of the passages D. excised in 1996 is not exactly reassuring and reinforces what should in any case be obvious: much of the time, arguments about authenticity are subjective, and arguments about interpolation in Lucretius are frequently circular. For my own part, I believe that the poem was left unfinished, and hence am willing to see imperfect passages as signs of incompleteness and doublets as signs of revision in progress, while D. believes that the poem was completed and that imperfect or inconsistent passages are the contributions of a later hand. Put slightly differently, the argument for eliminating verses often rests on the (at times unstated) assumption that the poem as it left the author's desk was perfect and thus that imperfections are a sign of interference rather than of incompleteness. I will not talk about D.'s alleged interpolations in as much detail as I discussed his choice of emendations; there, we start from at least similar premises, but in the case of interpolation that is not the case: even if we recognize the same signs of imperfection, we will inevitably interpret them differently. That way, therefore, lies an impasse, and the internal evidence of imperfection can never (or hardly ever) resolve the underlying issue of interpolation.

External evidence and intellectual history, however, provide at least a context within which to explore the pre-history of Lucretius' text. Philology is, after all, a historical discipline, and thus one of the most striking features of D.'s new trilogy is the contrast between the immense care he devotes to explaining the history of Lucretius' text from the Carolingians to the Renaissance and the complete absence of any attempt to investigate, or even to describe, the conditions of transmission that would have permitted one or more interpolators to insert hundreds of inauthentic lines into the text of *De rerum natura* and have them accepted by readers as genuine, in some cases until D. proscribed them in *Komm*. This absence deserves emphasis: although he has changed his mind significantly since 1996 about the extent of interpolation in *De rerum natura*,

D. in none of the three volumes under review has a single word to say about the posited process of interpolation or, indeed, about the history of the text in the eight centuries between Lucretius and the archetype. *Komm.* is marked by its gaps and silences: for 27 of the 62-odd passages he condemns as interpolations, he simply refers back to his discussion in 1996 without the addition of a single word, and for four more—bringing the total to exactly half—he only adds a reference to a more recent discussion of his own. In *Ps-L.*, D. at least devoted a few perfunctory pages (310-15) to suggesting a context (Herculaneum) and a time (before 79 CE) when massive rewriting of Lucretius' poem might have taken place.[41] That suggestion was, as will be discussed shortly, nonsensical, but at least it made a gesture towards recognizing the need for historical explanation of a remarkable phenomenon. But in *Komm.* and the other two volumes, I can not find so much as a cross-reference to D.'s earlier explanation.

To call the supposed presence of interpolations of up to 25 verses in the text of *De rerum natura* remarkable, as I did in the previous paragraph, is an understatement: if it were true, it would be unique in the history of the transmission of classical Latin literature. To give some context, it is worth comparing D.'s 60+ interpolations in Lucretius to the comparable history of the text of Ovid's *Metamorphoses*: D. in discussing the theory of interpolation in *Ps.-L.* took as a theoretical model the work of Richard Tarrant, whose Oxford Text of *Metamorphoses* marks more interpolations than any other recent edition.[42] Conveniently enough, Tarrant's edition marks some 66 interpolations (relying on my own count, which may again not be perfectly accurate), roughly equal to the number D. has identified in the considerably shorter text of *De rerum natura*. But the significant difference is that of Tarrant's 66 interpolations, not a single one is longer than six lines, only 24 extend beyond one verse, and only 8 have more than two. And of those eight longer interpolations three, including the two longest ones, reflect divisions in the manuscript tradition. D's interpolator finds no comfort in the transmision of the *Metamorphoses*.

On the other side, it is also worth considering where there are long interpolations in Latin poetry, and—outside of Plautus, where dramatic interpolation is a wholly different problem and the nature of the original is impossible to define—I can think of only three possible candidates, passages of dubious authenticity longer than ten lines. The most famous of these is the Helen Episode in the *Aeneid*, a brilliant fake composed in the first half of the first century CE; it appears in no reputable manuscript of the *Aeneid*.[43] Next is the so-called "second ending" of Terence's *Andria*, a happier and more explicit

[41] For a trenchant criticism of D.'s treatment of interpolation and its alleged context, see also E. J. Kenney, "Downsizing the *DRN*," *CR* 48, 1998, 26-7.

[42] R. J. Tarrant, "Towards a Typology of Interpolation in Latin Poetry," *TAPA* 117, 1987, 281-98; id., "The Reader as Author: Collaborative Interpolation in Latin Poetry," in J. N. Grant, *Editing Greek and Latin Texts*, New York 1989, 121-62; id., ed., *P. Ovidi Nasonis Metamorphoses*, Oxford 2004.

[43] The Helen Episode has been much discussed; the most recent treatment I know is Irene Peirano, *The Rhetoric of the Roman Fake*, Cambridge 2012, 242-63.

conclusion to the play that was probably composed in the second century CE: it was known, and known to be spurious, by the late antique commentators, and is not found in any of the major manuscripts of Terence.[44] And finally there are the so-called "Oxford verses" of Juvenal's sixth Satire: known to the late antique scholia, found in only a single Beneventan manuscript of the eleventh century—but unquestionably genuine, and thus not an interpolation.[45] Beyond those three, nothing. Whole poems were clearly faked, manuscripts were faked, spurious lines entered textual traditions, sometimes, as in the case of Lucretius or the *Metamorphoses*, with some frequency. But they found their way into the text singly or in pairs, and interpolations of entire cohorts of 20 or 25 lines are unexampled and highly unlikely.

How, then, did this extraordinary rewriting of *De rerum natura* in which D. clearly believes actually take place? As noted above, D. in none of the volumes under review says anything at all about it: he assumes it is possible because he believes it is true. But the story he told in *Ps.-L.* is, unfortunately, a fairy tale. D. believed (and presumably still believes) that somehow *De rerum natura* was used as a teaching text in the Epicurean school of Herculaneum. The evidence for that is non-existent: some papyrus scraps from the Villa of the Papyri have been identified as coming from a text of *De rerum natura*, but even if that is so, it means no more than that Philodemus or one of his friends read the poem, and not that it was on the syllabus as a text for Epicurean physics.[46] D. believes that the school of Herculaneum was responsible for the philosophical interpolations in *De rerum natura*: lines added to correct, clarify, or supplement the arguments of the poem. But several things militate against that. In the first place, the Epicureans who lived around the Bay of Naples—most notably Philodemus and Siro—were Greek, and Philodemus certainly wrote in Greek, both philosophy and poetry. Latin speakers are associated with these philosophers: not just L. Calpurnius Piso, who may have owned the villa where the papyri were found, but, conveniently enough, some remarkably important and skilled poets, notably Virgil himself and his close friend (and later executor) Varius. Both of them had indeed read Lucretius; and Virgil was clearly capable of writing very good imitations of Lucretius, in the *Georgics* and in Book 6 of the *Aeneid*. But the poets capable of writing Lucretian hexameters read Lucretius in a poetic context, not a school of philosophy, and are very unlikely candidates for Chief Interpolator. On the other side, it is now

---

[44] The basic treatment is still that of Otto Skutsch, "Der zweite Schluss der Andria," *RhMus* 100, 1957, 53-68, with modifications by B. Victor, "The 'alter exitus Andriae'," *Latomus* 48, 1989, 63-74. Yet another version of an alternate ending appears to be attested on papyrus: see G. Nocchi Macedo, "*P.Oxy.* 24.2401 and the History of Terence's Text in Antiquity," *BASP* 55, 2018, 71-117.

[45] See, for instance, E. Courtney, "The Transmission of Juvenal's Text," *BICS* 14, 1967, 38-50.

[46] So, rightly, Sedley, *Lucretius* 66. Scraps of Latin papyrus were claimed as fragments from *De rerum natura* by K. Kleve, "Lucretius in Herculaneum," *CronErc* 19, 1989, 5-27 and repeatedly thereafter, but few scholars nowadays believe in his Lucretian discoveries; see George W. Houston, *Inside Roman Libraries*, Chapel Hill 2014, 98 with references to recent discussions of the problem. Even Herculaneum has changed in the past 20 years.

quite clear that the major text on which the philosophical argument of *De rerum natura* was based was the first fifteen books of Epicurus' great work *On Nature*, without much, if any, attention to developments in Epicureanism after the death of the master: Lucretius was, as David Sedley has described him, an "Epicurean fundamentalist."[47] As such, *De rerum natura* would have been of remarkably little use to Philodemus and his friends, who spoke an Epicureanism that had been elaborated and modified over time and that paid a great deal of attention to refuting Stoic criticisms of Epicurus' beliefs; had *De rerum natura* been a set text in Herculaneum, the so-called interpolations would have reflected the changes in doctrine over the previous two centuries, but they do not. In the school of Philodemus, they make no sense at all.

There is a reason, of course, that D. needs Philodemus. He is well aware that Roman textual criticism was extremely conservative, and even the most radical critics such as Valerius Probus expressed their opinions in the margin rather the text, and they seem, to judge from the remains of ancient scholarship, to have preferred deletion to interpolation.[48] Only practical texts underwent significant alteration: theatrical texts might be expanded and revised for revival performances (as is the case with the extant text of Plautus' *Casina*); the cookbook of Apicius was enhanced with extra recipes; grammars and commentaries might keep an author's name attached to text quite different from what he had originally written. We know, moreover, that Lucretius was, at least in Jerome's day, a poet studied in schools equipped with a commentary: the marginal *tituli* could possibly be the remains of such a commentary, and D. himself has made a good case, using the *tituli*, that our archetype is descended from such a text.[49] But if literary texts, including Lucretius, were preserved within a tradition of highly conservative criticism, then the massive interpolations which D. finds in *De rerum natura* must have entered the text before it became a literary monument. Hence the suggestion that it was used as a textbook in an Epicurean school; hence Philodemus.

But if Philodemus & Co. were not the interpolators, it is remarkably hard to find any intellectual context in which wholesale and, if D. and his fellow interpolation-hunters were right, fairly clumsy interpolations could have made their way into the text of *De rerum natura*. Only one possibility comes to mind, and it is even more suspect than D.'s Herculaneum. In 1999, three years after the publication of *Ps.-L.*, D.'s mentor and friend Otto Zwierlein—to whom, along with D.'s Leipzig colleague Kurt Sier, *Komm.* is dedicated—published a remarkable book in which he claimed that hundreds of lines of the *Aeneid* were in fact composed by a mediocre minor poet named Julius Montanus sometime in the reign of Tiberius.[50] That was a mirage (to put it kindly) and required

---

[47] Sedley, *Lucretius* 62-93. The fundamental argument that Lucretius did not pay attention to developments in Hellenistic philosophy subsequent to Epicurus himself was made by D. J. Furley, "Lucretius and the Stoics," *BICS* 13, 1966, 13-33.

[48] On the conservatism of ancient criticism, see *Ps.-L.* 316-17.

[49] Deufert, "Overlooked Manuscript Evidence."

[50] Otto Zwierlein, *Die Ovid- und Vergil-Revision in tiberischer Zeit. Band I: Prolegomena*, Berlin 1999. My opinion of Zwierlein's argument may be found in greater detail

believing the idea that a text already widely read and studied could be replaced two generations after the author's death because of the alleged patronage of the emperor.

D. approaches the problem of interpolation more soberly than Zwierlein, and he wields his snickersnee with less abandon; that is perhaps because he is actually editing the text he carves up, something Zwierlein wisely refrained from doing. What is more, there are only two passages which (to my eye at least) clearly prove that the poem is incomplete rather than mangled by an Epicurean interpolator. One of them is 5.155, *quae tibi posterius largo sermone probabo*, in which the referent of *quae* is uncertain, but either it points ahead to some later passage or, if not such passage exits, it must be taken as a sign of incompleteness. As the text now stands, *quae* must refer to the abode and/or nature of the gods, described briefly in the previous lines; but D. in 1996 (*Ps.-L.* 302-4) argued that both those aspects of the gods had in fact been described, albeit briefly, and also that it was "unwahrscheinlich" that *quae* could refer as far back as the beginning of the preceding paragraph. He therefore proposed to move 5.155 thirty lines back, to follow 5.125, where *quae* could refer back to the previous lines about the mortality of the celestial bodies. He made that transposition easier by excising 5.126-45 for no good reason at all (a deletion still in effect in *BT*); and when Hans Gottschalk in reviewing *Ps.-L.* pointed out that he had made his own case more difficult by also deleting 6.419-31, a significant part of the argument that the transposed 5.155 might be thought to be talking about, D. conveniently decided in *Komm.* that those lines should no longer be deleted—but in *BT* 5.155 is in its proper place after 5.154; and D. has therefore destroyed his own argument against the belief that the line implies that the poem is not complete.[51]

Even more astonishing is D.'s treatment of 4.45-53, the so-called Second Syllabus of Book 4. As is well known, the opening of Book 4 is troubled, and there has been much dispute not only over the authenticity of the first 25 lines—D. thinks they are an interpolation—but about the passage that follows immediately: we first get a summary of what has come before (the discussion of the nature of the soul and its mortality in Book 3) and then a preview of what is to follow in Book 4 about perception and the simulacra. But then we start over, but this time, the summary of what has come before refers not to the contents of Book 3, but to the contents of Books 1 and 2 (4.45-48), while the preview of coming attractions has two lines that are identical to the first syllabus (4.49-50=4.29-30) but then uses language of the simulacra that is quite different from what has come before. For D. all this is nonsense, and all three sections of the Second Syllabus betray the hand of the Interpolator: the lines summarizing Books 1 and 2 are identical to 3.31-34, where they make sense; it would be quite wrong to say, as 4.49-50 do, that it is highly relevant to atomic theory that there

---

in my review, *Vergilius* 46, 2000, 181-91.

[51] H. Gottschalk, review of Deufert *Ps.-L.*, *Mnemosyne* 52, 1999, 748-55, at 751-2. D.'s inadequate response to Gottschalk is at *Komm.* 302. To the extent that D. has not really changed his methods since then, this review echoes a great deal of what Gottschalk also commented on.

be simulacra;[52] and the words used in the final section, which the Interpolator has made up himself, are all wrong as terms for the simulacra. He simply rejects without serious discussion the possibility that there was, or was intended to be, a reversal of Books 3 and 4. All this is found in D.'s discussion of these lines in 1996 (*Ps.-L.* 160-64); his parallel discussion now in *Komm.* adds nothing to what he said then about 4.45-53 and merely adds reasons for seeing 4.44 as part of the interpolation. Neither then nor now does D. pay any attention to the argument of Mewaldt, made more than a century ago, that the Second Syllabus is a remnant of unfinished revision, that Book 4 was in fact originally intended to follow Book 2 and be followed in turn by what is now Book 3.[53]

The reason for considering Mewaldt's argument is, quite simply, that it is right; and if a detailed proof of its correctness was not available in 1996 (although it had certainly been discussed and believed by a great many scholars) it certainly has been since 1998: David Sedley showed, in overwhelming detail, that Lucretius' source for Epicurean physics was Epicurus' great work *On Nature*; and in *On Nature* the discussion of perception and the simulacra preceded the discussion of the soul and its mortality. When D. says that 4.45-48 only make sense as an introduction to the discussion of the soul in Book 3, he is contradicting Epicurus himself. D. is right that the language used in 4.51-53 is awkward; again, as Sedley has shown, it is a first attempt to translate literally the terminology used by Epicurus himself, and that later—as in the First Syllabus, he used a different method.[54]

D.'s description of what is wrong at 4.45-53 is remarkable for more reasons than one. In the first place, he attempted in 1996 to eliminate it without serious consideration of the theories of the incompleteness of the poem for which it has long been a primary piece of evidence: as far as D. is concerned, there is no point in discussing it—or even admitting that it has been an important topic of discussion for other scholars. In the second place, although Sedley's book—and I focus on it because it is a clear case, and a book that I know better than much other Lucretian scholarship—has been available for twenty years and is without question one of the major contributions to the subject in the past generation, D. has chosen not to say a word even now about the relevance of Epicurus and of Lucretius' Epicureanism. And finally, he creates an interpolator stupid enough to insert a Second Syllabus from the wrong book and attach it to a repetition of lines from the previous page and to a misuse of Lucretian terminology—and yet clever enough to write some pretty good passages of Lucretian verse. Interpolator here is a close relative of Zwierlein's Montanus: he is too dumb for his own good, but too smart to be caught out by most critics.

---

[52] I do not understand D.'s objection here. He does not seem to notice that the word *res* appears in three successive lines, and the existence of *rerum simulacra* does indeed have considerable bearing on the *res* created from atoms—it is how we perceive them, and the simulacra are themselves *res* of a sort in that they too are composed of atoms.
[53] J. Mewaldt, "Eine Dublette in Buch IV des Lukrez," *Hermes* 43, 1908, 286-95.
[54] See Sedley, *Lucretius*, 39-42.

5. *Lacunae*

The very idea of interpolation, particularly large-scale interpolation such as D. and other critics believe took place in the text of Lucretius, in fact entails a very peculiar combination of intelligence and incompetence. An interpolator of this sort has the acuteness and skill to recognize places where the text or the argument is incomplete or unsatisfactory—this obviously does not apply to all the interpolations D. identifies, but it is a necessary element of the larger ones and is relevant to some of the shorter ones as well—and at times he is sharp enough not merely to find incomplete arguments, but even to recognize and fill in genuine lacunae, places where some of the original Lucretian text has been lost.[55] On the other hand, sharp-witted though the interpolator must be, and although he can clearly write passable Lucretian verse, he is not good enough to escape notice: there are infelicities in meter or grammar in some cases, failures to link the insertion to the surrounding genuine text seamlessly, and redundancies and repetitions that are not recognized or not removed. And above all, he is repetitive: many of the interpolations D. identifies are either repetitions (verbatim or very close) of genuine passages of Lucretius or (to use D.'s word) centos composed of lines taken from all over *De rerum natura*.

In this description, however, there is one obvious problem: it applies only to the unsuccessful interpolator who is caught and expelled by sharp critics (to whom we might give the designation Interpol). And indeed there is a symbiotic and reciprocal relationship between Interpolator and Interpol: only if Interpol is very clever will the clumsiness of Interpolator be revealed; but if Interpolator is very clever, Interpol will never find him. Neither can exist without the other, and neither exists outside a theory of textual criticism that views both sides of this relationship as part of the history of the text itself: from the point of view of Interpolator, the text which he is modifying, although genuine, is flawed; from the point of view of Interpol, the text which he is modifying is flawed because it is no longer genuine. Interpol is also in some respects the modern successor, rather than the opponent, of Interpolator. D., for instance, is inclined to see something missing from the text with some frequency: after 1.681, 3.1002, 4.144, 6.49, and 6.954 he is probably right; a lacuna is less plausible after 3.860, 5.257, 5.879.[56] In a text the problems of which are so many and so severe the presence of lacunae is scarcely surprising; rather, what is worth noticing is the number of times that D. composes verses to fill the gaps he has detected.[57] At 4.289 and

---

[55] I have not noticed many places where an interpolation coincides with what modern editors recognize as a lacuna. There were a few in 1996, but they seem to have disappeared. Thus at 1.146-8, where D. recognizes an interpolation, he formerly thought there was also a lacuna after 1.148; similarly he thought in 1996 that there was a lacuna after the interpolation at 6.383-5, but instead he now deletes 6.386 as well. Cf. *Ps.-L.* 64 n.263: "Daß Interpolationen eine Lücke verkleiden, ist ein weitverbreitetes Phänomen."

[56] These lists are obviously not complete, nor is D. responsible for identifying all the lacunae he marks in the text (he follows K. Müller in many cases). Note also that the possibility of a lacuna is admitted after 4.146, 4.961, 6.83a (mid-verse), 6.698a (mid-verse) in the apparatus criticus.

[57] Many of these D. takes from earlier scholars (with full acknowledgment), sometimes with modification; his main source and model is K. Müller.

4.961 he supplies one verse (and at 4.216 the first verse of what he is sure was a longer lacuna); at 3.860, 4.146, and 5.257 two; at 6.954 he puts one supplementary verse in the apparatus, but offers a two-line supplement in *Komm.*; and at 6.698, where K. Müller had proposed a lacuna beginning after *penitus* and composed a possible supplement of three verses, D. proposes one of four. D., to be sure, leaves his compositions in the apparatus or *Komm.* where they belong, unlike K. Müller, who printed no fewer than 66 of his own verses in the text, albeit in italics.[58] But if D. is more restrained than Müller in his versifying, the two differ only in degree: both are themselves interpolators, feeling the need to add verses (which are generally far more leaden as poetry than the passages they eliminate as interpolations). They look for interpolations in the same spirit as they look for places to interpolate themselves: it takes one to know one.

I return briefly to the question of the Second Syllabus of Book 4, because it provides crucial evidence for the issue of interpolation, and D.'s treatment of it in turn provides very strong evidence for the gaps in his approach to the text. I see no reasonable alternative explanation for the existence of this passage other than that introduced by Mewaldt and now argued in detail by Sedley: that is is a sign of the incompleteness of *De rerum natura*, a mark of revision in progress, and a very important indication of Lucretius' changing ideas about how to present Epicureanism and how to organize his own poem. But although I see no alternative explanation that works, I would be very happy to learn of one—one that takes into account not just the immediate awkwardness of 2.45-53 (which is obvious) but also the possible reasons for it. But D. has made no effort to identify, explain, or come to terms with the (to me) very powerful reasons for interpreting it as a sign of incompleteness and revision in progress; he pays no attention to the Epicurean context and background for what Lucretius is saying; and he apparently has no particular interest in learning about them. He is unwilling to draw the obvious conclusion about interpolation in Lucretius: the main interpolator is Lucretius himself.

There are lacunae in *De rerum natura*, to be sure; but they are nothing at all in comparison to the chasm that yawns between D.'s approach to the textual criticism of Lucretius and those other approaches which he steadfastly refuses to recognize. As I hope I have made clear over the course of this (overly long) review, there is much that is admirable in D.'s scholarship, but his main contributions lie fairly strictly within the confines of a certain kind of philology: he is brilliant in sorting out the manuscripts; he has a great ability to clarify grammar, meter, and argument; he recognizes problems in these areas, and he advances solutions. But the fact that his sphere of reference is limited to philology, and in the text itself largely limited to relatively small passages, means that his textual improvements (both his own emendations and the emendations of others that he elevates to the text) are successful in similarly small ways: they improve grammar, meter, and the sense of short sections of the text. That is indeed not an inconsiderable accomplishment, and in terms of

---

[58] Owing to library closure, I have been unable to consult Müller's edition directly (Conradus Müller, ed., *T. Lucreti Cari De Rerum Natura Libri Sex*, Zurich 1975); I owe this information to the (scathing) review of M. F. Smith, *CR* 28, 1978, 31.

the *words* of the text my sense is that *BT* is now the best available edition. But even there, D. looks regularly to rules and algorithms to guide his choices, and those rules are entirely drawn from within the discipline of philology as he defines it.

But D.'s toolkit is—and I assume it is by his own choice—a small one, and it is one that has gotten smaller since 1996. Then, he at least made some reference to some fairly recent work on Lucretius as a philosopher and a poet, if only to disagree with it. But in *Komm.*, there is no sign of Furley or Gale (whose names do appear in the bibliography of *Ps.-L.*), not to mention Sedley or the large amount of relevant scholarship, philosophical and literary, that has appeared since 1996. D. cites occasionally from Epicurus, but only from texts long known (the letters and the *Principal Doctrines*, together with one fragment in Usener)—and never from the one major text which was clearly Lucretius' principal source, *On Nature*. He cites a few lines of Empedocles, but unless I am mistaken, none of them is from the new fragments that have so changed our understanding of Lucretius' relationship to him. To be sure, these works are not always relevant to textual questions—but even when they are, it is unfortunately clear that D. is not interested.

If I were writing about the text of Cicero's speeches, I do not think I would get very far if I limited myself to questions of rhetorical theory or periodic style and paid no attention to Roman law or Roman history. That is, I think, an example comparable to Deufert's approach. Neither textual criticism nor Latin philology stands on its own: we deal with texts that have historical and intellectual contexts, and we deal with texts that have intellectual content as well. However tidily we can organize the Latin, we also have to recognize that the Latin we are correcting is actually saying something about something. And Lucretius had an extraordinary amount to say, not just about Epicurean physics, but about the language and literary and philosophical traditions, both Greek and Roman, which allowed him to write as he did. To limit the analysis of his text to narrow questions of philology, however well studied and explained, diminishes not only Lucretius, but his editor as well.

James E. G. Zetzel
Columbia University in the City of New York
jez2@columbia.edu