hausted his attempts turned out quite imperfectly, not only because of their inexperience and incomplete knowledge of the manuscript material but above all for the objective reason that even today makes it impossible in so many cases to trace a stemma codicum: contamination. Heyne—a philologist whose greatest originality certainly did not consist in textual criticism but who even as a textual critic is more valuable than is generally said—was well aware of this phenomenon of contamination; for the New Testament, Griesbach was too.

So too, the manuscript tradition of Homer was too contaminated to permit the fulfillment of Wolf's proposal that the manuscripts be organized "into classes and families." But thanks to his use of the Venetian scholia discovered by Villoison, Wolf was able to achieve something else in his Prolegomena: the history of a text in antiquity. In this way he prepared the way not for Lachmann, but rather for Jahn's and Wilamowitz's concept of Textgeschichte [history of a text] and for all the nineteenth- and twentieth-century studies on "ancient variants and ancient editions" (to repeat the title of one of Pasquale's chapters). For Wolf, the Homeric question itself was nothing more than the first phase, oral and popular, of the history of the text of the Iliad and Odyssey: in the Prolegomena it is only discussed in these terms and not as a problem in literary history.

44. Heyne 1817 (1753): xix-xxiii; but what Heyne provides is in the first instance a genealogy of the editions of Tibullus, and only secondarily one of the manuscripts. Schweighäuser 1798: preface: to classify the manuscripts, Schweighäuser relied on shared corr uptions less than on the reciprocal arrangement of the text of Epictetus's Manual and of Simplicius's commentary on it (cf. Timpanaro 1955: 70). Schweighäuser had more merits in the eliminatio codicum describentorum: see below, p. 99.

45. Heyne 1817 (1753): xv, xxxvii (where he speaks of "apographa, perhaps prepared with others, or made out of them"); Heyne's fame as a textual critic, and as a Classical philologist in general, was impaired by the scornful tone adopted toward him by his students Wolf and Lachmann (and, in a less technical field, Friedrich Schlegel), in great measure unjustly; for Lachmann in particular, see Lachmann 1876: 2106.

46. Griesbach 1796-1774: loc xviii: "The readings of the one recension have been introduced into the manuscripts of the other family," etc. Already Semler (1765) had often observed that in different passages the very same manuscripts belong to different "recensions."

47. F. A. Wolf 1895 (1795): 44. In his editions of other authors (Plato, Cicero, etc.), which in certain cases would have allowed an application of the genealogical method, Wolf limited himself to hasty recognizences in contrast with his principles.

48. Simon 1685 had already arrived at this concept of a "history of the text," but he had had no followers among Classical philologists. Cf., for new, Pfeiffer 1976: 250.


3

The First Phase of Lachmann's Activity as a Textual Critic

After the considerable progress achieved by the method of textual criticism during the eighteenth century, we witness a return to old positions in the first fifteen years of the nineteenth century. Gottfried Hermann and Immanuel Bekker, the two greatest textual critics of the generation after Wolf, differed greatly from one another in many ways, but both remained quite unaffected by the need for a systematic recensio as it had been adumbrated by the great New Testament critics and by Ernesti and Wolf. Hermann was an admirable expert on Greek language and style and supplied contributions of decisive importance to the study of meter, but he had no interest in manuscript tradition: his editions are based not on manuscripts but on preceding editions, and the improvements he contributed to the text of the Greek poets are the fruits of conjecture or, when he does choose between variants, are based solely on internal criteria. To be sure, he succeeded very often in resolving once and for all text difficulties that had remained unsolved until then; for after all, a thorough knowledge of an author's language and style always remains the first and essential condition for restoring his text. And yet his complete indifference with regard to the documentary foundation of the classical texts represents not only one aspect of his lack of understanding for the new Classical philology of Wolf and Boekh but also a step backward compared to the textual criticism of the eighteenth century.

Bekker, on the other hand, was an indefatigable explorer of manuscripts:

1. Cf. John 1849: 20. Sauppe 1841: 15; Sauppe 1866: 92, writes, "Whoever wishes to perform the art of criticism properly must first of all examine the manuscripts and seek out and investigate their characteristics as carefully as possible. I recall that you [i.e., G. Hermann] gave this advice very often." It remains uncertain whether Sauppe capitandae benevolentiae causa is attributing here to his teacher something that the latter had in reality never said, or whether Hermann really did give this advice to his students without going on to apply it himself; we have already noticed a similar contrast between theory and practice in Ernesti and in Wolf himself; cf. chap. 2, no. 39, 47. Cf. the addendum to this chapter.
we are indebted to him for the rediscovery or appreciation of first-rate textual sources such as the Urbina of Isocrates (Vatican, Urbini. gr. 111), the Parisinus manuscripts of Demosthenes (Paris. gr. 2934) and of Theognis (Paris. suppl. gr. 288), the Ravenna of Aristophanes (Raven. gr. 429), and many manuscripts of Plato. Yet despite the fact that Bekker was Wolf's favorite pupil, not even he ever thought of doing a systematic recensio as Wilmotowitz rightly notes, "in his choice of manuscripts and readings Bekker essentially relied upon his sense of language and style, which, for Artic prose, was certainly very experienced.

This step backward in the methodology of textual criticism at the beginning of the nineteenth century helps to explain the impression of great originality that Lachmann's first works make even when they do nothing more than reaffirm, sometimes with less refinement and caution, principles that were well known to the Classical philology of the eighteenth century.

In the field of the criticism of Classical texts (we shall refer to Germanic texts shortly), Lachmann began his activity with an edition of Propertius in 1816, followed by editions of Catullus and Tibullus and an editio minor of Propertius (all 1829). His review of Gottfried Hermann's edition of Sophocles' Ajax and two reviews concerning Tibullus are also important.

In this first phase of his thought, Lachmann claims, polemically and paradoxically, that the most urgent task is to supply rigorously diplomatic editions reproducing the manuscript tradition in the most ancient form we can attain, "without taking into the least consideration meaning or grammatical rules." Hence not only is conjectural criticism postponed until later; so too is even interpretation itself. Until that time, most critical editions had been simultaneously exegetical, even if some scholars like Heyne accorded the first rank to exegesis, others like Hermann to textual criticism; and all the Romantic theorists of Classical philology, from Friedrich Schlegel to Ast, to Schleiermacher, to Boeckh, had insisted, or would do so a little later, on the "critical and hermeneutic circle." Lachmann, instead, produces editions that are purely critical: in the preface to his Propertius (1816: iv) he refers the reader for everything regarding interpretation to the commentary on the poet that J. G. Hulschke was planning to publish, just as in the preface to his Lucretius (1850: 15) he will refer to the future exegetical works of Steinhart and Reisacker. The critical apparatus, which in his first edition of Propertius is still "selective," becomes "scant" in his editions of Catullus and Tibullus and in his second edition of Propertius.

With regard to the manuscript tradition, Lachmann insists above all on the distinction between interpolated manuscripts and uninterpolated ones. Manuscripts that contain Humanist interpolations must be set aside: you are in trouble if you yield to the attraction of their specious readings! In this polemic favoring truth over elegance, which assumes a tone of particular vehemence in his preface to Propertius, in this distrust of the docti Italiani [learned Italians], Lachmann feels himself particularly close to Scaliger but has harsh words for most of the Dutch Classical philologists. Lachmann's language, like Scaliger's, reveals not only the pure requirement of documentary truth but also a motivation typical of aesthetic criticism: anyone capable of judging can tell that the original reading is more beautiful than the interpolated one; Classical harshness is preferable to Classicist tinsel.

Lachmann sympathizes more with Bekker than with Gottfried Hermann among the Classical philologists nearest to him in age, and this is only natural. It is strange that he does not name Wolf; yet it was Wolf who had demanded editions based on a solid diplomatic foundation (see above, p. 714), even more than Bekker; and Lachmann, who studied the Homeric question and created an analogous "Nibelungen question," knew Wolf's Prolegomena ad Homeron better than anyone else.

But once the interpolated manuscripts have been excluded, how is the text to be constituted in those passages in which the uninterpolated ones differ from one another? Lachmann answers that the original reading can be

2. Wilmotowitz 1894: 42.

3. Other examples could be cited. For example, Friedrich Ast, one of the leaders of the new historical approach to Classical philology, took the Aldine edition of 1515 as the basis for his own edition of Plato (Ast 1819: 15) and set himself the goal of adhering to it as closely as possible! For Elnke as a partial exception in England, see above, p. 57.


6. Lachmann 1876: 4:2. Cf. Lachmann 1876: 3:145-46. "In my edition of the Roman elegists I had the modest aim of presenting the authentic transmission completely, excluding as far as possible all later vagaries."
found in these cases only "by reason and the effort of a skilled mind," so to determine the original reading he does not yet have recourse to any kind of mechanical criterion (of the sort Bengal had already enunciated). Nor can he have such recourse, since he is not yet tracing a genealogy of the manuscripts in these editions of Propertius, Catullus, and Tibullus. He does indeed refer a very few times to the fact that some manuscript "almost completely agrees" with some other one, or that some manuscripts are "derived from a single source," but these references are so sporadic that they could have furnished no basis for mechanical choice among the variants. From this point of view, he is still very far behind Heyne's and Schwegläusser's genealogical attempts, imperfect as these were. His distinction between sincere manuscripts and interpolated ones refers only to their value, not to their origins: unlike Politian and other Classical philologists who followed him, Lachmann eliminates interpolated manuscripts not because they are copies of extant manuscripts but simply because they are untrustworthy. In part because of the difficulty of procuring collations of manuscripts preserved in distant libraries, in part out of a haughty disdain for anything that looked to him like a useless aggregation of erudition, he always ended up basing his editions on an extremely limited number of manuscripts, selected sometimes rather arbitrarily: for Catullus he used only two manuscripts, and in this case it is quite obvious that the choice among variants could only be based on internal criteria, since mechanical criteria require at least three witnesses. Yet in that very same edition of Catullus he thought he could reconstruct the pages of the lost ancestor, whose numbers he indicated in its margins—a failed attempt, despite Moritz Haupt's attempt to defend it. As we shall see, in the case of Lucretius Lachmann will have far more success with an analogous attempt.

At the same time as he was publishing his editions of Propertius, Catullus, and Tibullus, Lachmann was also extremely active as a textual critic in the field of medieval German poetry. To the flowering of Germanic studies during those years in response to the stimulus of Romanticism (it is not necessary to recall the names of Wackenroder, Tieck, Uhland, Arnim and Brentano, and the brothers Grimm), he contributed a rigorously philological method that he had learned from the Classical philologists; in this way he made a significant contribution to the birth of a scientific approach out of what had originally been a "return to the Middle Ages" espoused by populist and reactionary literary figures. In the period 1816–29, which we discussed earlier with regard to his Classical philology, Lachmann published editions of the Nibelungen (1826), Hartmann von Aue's Iwein (1827), and the poems of Walther von der Vogelweide (1827), alongside a large number of articles and reviews concerning Germanic philology. In the same field his editions of Wolfram von Eschenbach (1833), Hartmann von Aue's Gregory (1836), Ulrich von Lichtenstein (1841), and various articles and re-editions will follow later.
Obviously, it is not up to me to judge this part of Lachmann’s activity, given my ignorance of Germanic studies. Nonetheless, I might perhaps venture to say something regarding the pure and simple technique of recensio as he applied it in this field. It seems to me that the Lachmann recognizable in these works is indeed fundamentally the same as the Classical philologist who wrote the prefaces to the Latin poets,22 but nevertheless it is noticeable that as a Germanist Lachmann devotes greater care to seeking out the manuscript material (as the mere list of the manuscripts he collated already makes clear) and greater effort to investigating the kinship relations among the manuscripts or at least to distinguishing among different redactions of the same work. I think that this slight difference in method can be explained partly by the greater accessibility of the manuscripts (which are almost all found in Germany or Austria), partly by the fact that here Lachmann’s polemical target was different: in the case of Classical texts he felt above all the necessity of eliminating the worthless excess variants that had been accumulated in the editions of the seventeenth and eighteenth centuries, and this ended up leading him to excessive simplification; in the case of medieval texts, on the other hand, he had to refute the opposite prejudice, that the edition had to be based on a single manuscript (as a rule, the oldest one),23 and this induced him to collate other manuscripts and to enlarge the documentary basis for his edition. So we see Lachmann the Germanist asserting that to restore a text to its original form, at least four or five manuscripts are necessary;24 that even if a recent manuscript contains linguis-

22. For example, in his preface to Wolfram von Eschenbach (Lachmann 1879 [1833]; xxviii) he refuses with typical impatience to study the genealogy of the manuscripts in greater depth (“But why should one extend the investigation to the smallest detail?”) and declares that he has constantly followed one class of manuscripts, however much this “might impair the truth as a whole.”

23. Among Medievalists this prejudice lasted for a long time: in Italy it was defended in the second half of the nineteenth century by Ernesto Monaci and attacked by Raimo and Barbi (cf. Pasquale 1942: 224 = Pasquale 1968: 2:157–158). In our century Bélier sought to revive it [Bélier 1928: against his attempt see Pasquale 1932: 130–131 and 1942: 232–33 (= Pasquale 1968: 2.163–164); Dain 1975 (1949): 122; cf. also below, Appendix C, n. 3, and Avalle 1972: 28–29 (who bases his argument to part on the methodological observations of Contini 1942: 329–331). As these scholars have observed and as Barbi already understood, the only case in which Bélier’s criterion is somewhat justified occurs when every manuscript (or group of manuscripts) represents an independent redaction, in a certain sense an autonomous work, as often happens in texts of popular tradition (or in literary texts revised by the author); if practical considerations make it absurd as impossible the idea of publishing all the different redactions separately, it is indeed better to select only one of them rather than to compile a contaminated text that does not correspond to any redaction that has ever existed.

Addendum to Chapter 3

In the last years of his life, did Gottfried Hermann feel the need to refer to manuscripts instead of taking the vulgate as the sole basis for a conjectural criticism? Did he recognize at least in part the justice of Lachmann’s criticisms of him in his review of the Ajax many years before (cf. above, chap. 3, n. 4)? The answer seems certainly yes. Antonio La Penna has brought to my attention this statement by Hermann from the beginning of his essay “De hymnis Dionysii et Mesomedis” (Hermann 1842: 1 = Hermann 1827–77: 8, 143): “There can be no doubt that the reliability of the written documentation must be examined first of all when an erroneous transmission is emended. Usually this is easy when we have only one exemplar of a text; it is more difficult when there are several that differ from one another; it is most difficult of all when we suspect that the true form of the text has not been transmitted but must finally be tracked down by conjecture.”

Sauppe’s testimony, which we quoted above and which dates from 1841, only one year before this essay of Hermann’s, therefore corresponds at least in part to the truth, even if with some distortion. Indeed, as we shall see in chapter 5, by this time not only Lachmann but also Orelli, Madvig, C. G. Zumpt, and Ritschl had been repeatedly declaring for many years that it was necessary to investigate the documentary foundation of the texts, the fides scripturarum. Hermann could not possibly have remained entirely deaf to these voices.

On the other hand, by this time he was too committed to his own version of philological practice (which indeed had produced many splendid results) and perhaps was also too old to “renew” himself completely. He never applied the new methods in a way corresponding to that statement of principle, not even in that very same article, except for a few sporadic references to manuscript readings; what is more, the statement itself is not a model of clarity and coherence.

Above all, Hermann seems to be still committed to the distinction between emendatio ope codicum and ope ingenii, rather than to the one be-
Lachmann as an Editor of the New Testament

From Germanic studies let us return to Greek and Latin texts. We can distinguish two parallel lines in Lachmann’s activity as a textual critic during the fifteen years from 1830 to 1845. On the one hand he edited a series of texts transmitted in only one manuscript or editio princeps, for which it was naturally not recensio that posed problems but only emendatio: Genesis (1834); Terentianus Maurus (1836); Gaius (1843), an edition begun by J. F. L. Goeschen and completed by Lachmann; and Babrius (1845). On the other hand he edited the New Testament: the editio minor appeared in 1831, the first volume of the editio maior in 1842.

The last important edition of the New Testament had been Griesbach’s (see above, p. 70). Lachmann severely criticized Griesbach’s persistent acquiescence in the textus receptus (Lachmann 1876: 2.131), and he finally achieved what Bentley had planned: an edition founded solely upon the ancient manuscripts and Saint Jerome’s Vulgata. Not even Lachmann was spared unpleasant accusations by narrow-minded theologians, but times had changed and the authority of the receptus was no longer able to reassert itself.

1. Shortly after his edition of Babrius, in the same year, 1845, Lachmann published a little edition of Avianus in Berlin. In this case there were many manuscripts, but Lachmann pushed his desire for simplification to an extreme and hence did not even indicate them, instead using in the critical apparatus vague terms like “very many,” “few,” “two very ancient ones,” “a very ancient one.” The only value to this edition consists in some good conjectures. The first scholar to undertake a true recensio of Avianus was Fohliner in 1864; cf. Fohliner 1864: xlii.

2. Lachmann 1831, without a preface or critical apparatus (but Lachmann explained the criteria he had followed in Lachmann 1830 = Lachmann 1876: 2.150–72, which I cite); Lachmann 1842–50: 1.xxx–xxxiii and passim; Hertz 1851: 160, 165–67.

4. Lachmann states in the preface to his editio maior, “these men alone [i.e. Bentley and Bengel] understood what it is that I call editing (reconsensio)” (Lachmann 1842–50: 1.xxx). 5. Already before Lachmann, Semler (in the preface to Semler 1763) had designated the two families as “the Eastern” and “the Western” ones; he called them recensiones, a term that suggests he conceived them as “ancient editions,” “revisions,” rather than as mere genealogical groupings.

6. Lachmann 1876: 2.137; his qualification “even if perhaps it is the only genuine one” precedes the distinction between leçon strasse [true reading] and leçon authentique [authentic reading] established much later by Haver 1911: 125–127. But, as Lachmann himself declares (pp. 238, 240), in general he followed the “Eastern” family in the editio minor, even where the other one offered readings that he himself recognized to be superior. He remedied this incoherence in his editio maior, but this too was based on a manuscript material that was too incomplete to allow him to really reconstitute the original readings of the two families (which as it turns out are more than two and all contaminated) cf. Tischendorf–Gebhardt 1897: 2.758–761; Gregory 1900–1909: 2.966–842; Metzger 1968 (1964): chaps. 2, 6, 8 (chap. 6 must be used with caution).


8. Lachmann 1842–50: 2.viii. Furthermore, the confirmation in the agreement of witnesses brought together from different regions is greater than the danger arising from some
had made an observation of this sort, but he connected it with the reliability of the New Testament tradition as a whole, not with the choice among variants: "Tis a good providence and a great blessing, that so many manuscripts of the New Testament are still amongst us; some procured from Egypt, others from Asia, others found in the Western churches. For the very distances of places as well as numbers of the books demonstrate, that there could be no collusion, no altering nor interpolating one copy by another, nor all by any of them." And we have seen (chap. 2, n. 22) that Bengel included a geographic consideration among his rules as well.

Pasqualli insisted on the importance of this geographical criterion, emphasizing its analogy with the criterion of lateral areas used by "neologinists." Shortly before, while Pasqualli was studying the manuscript tra-

"Above all we shall take account of the most ancient [sc. witnesses], and among these of such ones as derive from the most widely separated places" (p. vi). Cf. also: "Where manuscripts from distant regions agree with one another, this is likely to have been propagated from very ancient sources into the various places: on the other hand, we must suspect that unique readings of individual exemplars were born at home and are not derived from a common source" (p. vii). But in this last passage what is opposed to agreement among "distant" manuscripts is not agreement among "near" manuscripts, as being less decisive, but rather the readings of individual manuscripts unless we understand exemplar in the sense of "subarchetypes;" there is a certain contamination between the "geographical criterion" and eliminatio lectorum singularum.

9. Bentley 1816–186: 3360 (from his Remarks upon a Late Discourse of Free-Thinking; see also above, p. 63f.). Lachmann had this passage in mind and expresses himself in a similar way on p. ix of his preface. Less convincing is Lachmann's attempt (p. vii) to find an anticipation of the geographical criterion in S. Jerome, Ep. Ad Damasum, in Migne 1840: 539.


On the value Pasqualli attributed to this criterion, cf. also Pasqualli 1932a (1914): 345: "indeed, this is one of the ideas that prompted me to write this book." Pasqualli himself cites Bartoli 1927: 6–23. See also the conclusion of Bartoli 1943: 76. Neologinistics (also called area or spatial linguistics) was a school originating from the teaching of Hugo Schuchardt and Jules Gueffier, which aimed above all to reconstruct the relative chronology of linguistic facts on the basis of their geographical distribution. The "neologinists" were opposed to the "neogrammarians" and were influenced by the idealism of Benedetto Croce and Karl Vossler; this influence appears to be stronger in Giulio Bertoni, much less so in Matteo Bartoli, in whom it had to struggle against a prejudicial distrust of any philosophy of language (as, as he used to say ironically, "glotonomy"). This brief indication is intended for any non-Italian readers who may not have heard of neologinistics.


Pasquali, even facts attested in only one marginal area are probably more ancient. 13

In any case, the fact that Lachmann is rigidly Lachmannian as an editor of the New Testament too is demonstrated by the very beginning of his preface (p. vi), in which he not only distinguishes between recensio and emendatio more clearly than in his earlier writings but also repeats quite drastically the difference between recensio and interpretation: “We both can and must edit [recensere] without interpreting”—the very principle against which Pasquali always protested. Lachmann had already objected against Griesbach in his Rechenschaft of 1830 that internal criteria for the choice among variants “by their nature almost all cancel each other out.” 14 and this disdain for internal criticism is also why he unjustly disparaged Wetstein. 15 Without a doubt, there are cases in which internal criteria cancel each other out; sometimes a reading is difficilior in one respect, facilior in another; sometimes the criterion of the lectio difficilior runs afool of the usus scribendi, especially if it is applied with scholastic rigor. 16 But recensere sine interpretatione was never anything more than empty boasting, even on Lachmann’s part, not only because he had at the very least to understand the readings of the manuscripts in order to be able to classify them, but also because after the eliminatio in textual criticism there still remained a large mass of variants of equal documentary authority from among which he too had to choose on the basis of internal criteria. 17 More recently, Quentin’s method was an attempt to really achieve recensio sine interpretatione, but it very quickly revealed its sterility. 18

18. Pasquali 1932: 131 writes justly: “Already Lachmann did not trust indicem enough. Quentin exaggerates this distrust in a somewhat too Catholic, monkish way.” Dahn 1949: 162-84 demolishes Quentin’s “edictum” theory with affectionate irony in a paragraph titled “La grande Illusion” [The grand illusion] (the title reproduces that of Renou’s famous film and is already ironical); Dahn’s criticism of Quentin in the corresponding paragraph of later editions of his work is more perfunctory and less effective. More recently, Baldinou 1979: 232-34 has raised calm but radical objections to Quentin. The Quentinian method has been revived by theoreticians of the automation of textual criticism (Proctor 1968; Zarr 1969, 1979, and other works of Zarr’s which I do not cite for lack of space). I have had the benefit of exchanging ideas with my friend Zarr, and I am convinced that new methods can yield great advantages, and already have done so, for unraveling a certain extent manuscript traditions that are very complex and richer in variants than in real corruptions. More than ever I find mistaken and reactionary any hostility against the methods of automation which is based on rhetorical claims for the uniqueness of the “human spirit.” But one fact remains: Quentin’s method is powerless in the face of the objection that only coincidence in error can indicate the kinship between two manuscripts; coincidence in the correct reading proves nothing, since it is a fact of conservation that can also occur in manuscripts unrelated to one another. The recourse to Quentin’s method on the part of theoreticians of automation is a “sad necessity,” since a computer is not capable of distinguishing a correct reading from a corruption: for that, we would need an “artificial philologist,” something we do not yet possess.
In the meantime, during the 1830s, other philologists had made contributions of enormous importance to the method of textual criticism, especially regarding the genealogy of the manuscript tradition.

"The families and as it were lines of descent, both of manuscripts and of editions, must be established": this was the plan Johann Caspar Orelli proclaimed in the preface to the first volume of his edition of Cicero, which appeared in 1826. He himself only succeeded in fulfilling that plan to a very small extent: a whole lifetime would not have sufficed for a truly critical edition of all the works of Cicero, especially at that time, when manuscript collations were so difficult to obtain; and the interests of Orelli, an attractive figure as scholar and religious and political reformer and an intelligent follower of Pestalozzi’s ideas, were too various to allow him to dedicate himself entirely to this project. For that reason his edition, which was completed in only six years, from 1826 to 1831, ended up being in large part a hurried revision of the preceding editions. Once again we find the same contrast between editorial theory and practice that we have already noted, for example, in Ernesti. But Orelli had the merit of at least reviving the programmatic demand for the investigation of the genealogical relations between the manuscripts in a period in which it had fallen into neglect after having been asserted with such energy at the end of the eighteenth century (see above, pp. 75f.). And just as his brief reference to the manuscript tradition of Lucretius provided a starting point for Johan Nicolai Madvig and other scholars before the publication of Lachmann’s Lucretius (see the following chapter, pp. 102ff.), so too his edition of Cicero stimulated Madvig to give a first, substantially correct genealogical outline of the tradition of Cicero’s Verrine orations in his Epistola ad Orellum (Epistle to Orelli).

This was the first exchange in a fruitful dialogue between the Classical philologist of Zurich and his younger colleague from Denmark, which continued in the following years and can be followed in the inaugural dissertations and editions of individual works of Cicero’s which were published by the one scholar or the other. This was a genuine dialogue in which, even if from the very beginning Madvig displayed a more vigorous and original philological personality, Orelli too made contributions that were far from negligible and demonstrated that he knew how to study certain problems in depth which he had barely touched on in his Cicero edition. And soon the dialogue was enriched by the addition of a third interlocutor, Carl Gottlob Zumpt from Berlin. A student of Wolf’s, Zumpt had learned from him to disdain eclecticism and to demand a text based constantly on the best manuscripts. He put these precepts into practice, first in an edition of Quintus Curtius Rufus, then in an edition of Cicero’s Verrine orations that appeared in 1831. This latter edition bears the traces of a toilsome revision: Zumpt had to begin his work on the basis of old printed editions because of the difficulty of obtaining reliable collations; only later was he able to go back to the manuscripts, and he did not feel up to eliminating completely the old and by now quite superfluous hodgepodge from the pref ace and critical apparatus. All in all, his genealogical reconstruction makes little progress beyond Madvig’s much more concise one, which Zumpt read only when his own work was almost finished. But the stemma codicum that Zumpt drew up as a conclusion and summary of his investigation (1831: i.xxxviii) was a very important technical innovation:

4. For Orelli see esp. Orelli-Beyrer 1850, preceded by an Epistola critica ad Jo. Nic. Madvigium: Orelli 1832, 1835, 1837. For Madvig see the prefaces and dissertations collected in Madvig 1834 and 1842. On Madvig see also below, p. 97. As is well known, Orelli undertook a new edition of Cicero in collaboration with Bätter in 1845; after his death it was continued by Bätter and Karl Halin.
5. C. G. Zumpt 1836: esp. xiv; later, in 1849, Zumpt published an ed. maj. of the same author, cf. K. Möller 1954: 758. C. G. Zumpt 1851. On his relations with Wolf see A. W. Zumpt 1831: 77–34. Zumpt’s preface to the Verrines reveals Wolf’s influence in its very terminology, for example, in the distinction between recensio and recognizio: “Thus, employing a recent, but useful, distinction, it will be true to say that he published a revision (recognitione) of Gruter’s text than a critical edition (recensio) of Cicero” (C. G. Zumpt 1831: 1.xxx–xxv); cf. above, p. 74f.
6. He himself explains this in his preface, C. G. Zumpt 1831: i.xxxiv.
Is this the first *stemma codicum* that was ever actually drawn up, and not only planned like Bengel’s *tabula genealogicalis*? No. In the first version of my study (Timpanaro 1959: 213), I attributed the innovation to Ritschl (see below, p. 93f.), though I added, “I know well how cautious one must be in such assertions of priority.” My caution was all too justified! When this study was first published as a book (Timpanaro 1963: 46 and n. 1), I was able to backdate the first stemma from Ritschl to Zumpt, thanks to a kind reference by Konrad Müller. But, a little later, a Swedish scholar, Gösta Holm, demonstrated that Carl Johan Schlyter had drawn up a stemma of a type more “modern” than Zumpt’s in the first volume of an edition of ancient Swedish legal texts published in 1827 (and thus four years before Zumpt’s edition of Cicero), in a field very remote not only from Classical philology but also from the study of Germanic literary texts as Lachmann had practiced it, and furthermore that already at an extremely early date he was quite aware of certain causes of disturbance in a purely “vertical” transmission (not so much contamination as rather the conjectural activity of individual copyists). Given the subject matter of Schlyter’s publication, it is not surprising that it escaped the notice of Classical philologists; it is almost certain that Zumpt drew up his stemma without knowing that he had such an acute predecessor in Schlyter.8

8. Collin-Schlyter 1827. Cf. Holm 1972: 74–80, 53 (reproduction of Schlyter’s stemma), 77–79 (comparison with other, slightly later stemmata). Perhaps Holm overestimates somewhat Schlyter’s contribution regarding the reconstruction of the text of the ancestor (Holm 1972: 60–64); as far as I can tell (but it must be borne in mind that I only have access to the passages that Holm translates or summarizes), Schlyter oscillated between the criteria of the “best manuscript” and of the majority of the witnesses; thus in this regard he still lagged behind Madvig (see below, pp. 97f.) and Lachmann.

In the field of Latin texts, in 1809 and 1813, even before Schlyter, Johann August Goerens had summarized in the form of a *tabula [table]* or *tabella [small table]* his subdivision of the manuscripts of Cicero’s *De legibus* and *De finibus* into two classes. But Goerens’s classification still referred to the value of the manuscripts (on the one side the *codices potiores* [superior manuscripts], on the other the *deteriores* [inferior ones]), not to their relations of derivation; and his *tabulae* were simple lists of the manuscripts of both categories, not genealogical trees. Zumpt, on the other hand, already indicates the manuscripts’ derivation, even if somewhat less precisely than Schlyter; and so far as we know, he is also the first to introduce the term *stemma* (that is, precisely, “genealogical tree”), which will end up prevailing over other synonyms.9

The young Ritschl is also connected with Wolf, even if he did not study directly with him. Typically Wolfian is his interest in the history of the text (understood in a very broad sense, as the history of Alexandrian and Byzantine philological culture), which he showed starting with his edition of Thomas Magister’s *Εξελέξην ἤμωντιν καὶ ἐμνήσει άττικήν* [Selection of Attic nouns and verbs].10 If I am not mistaken, the very term *history of the text* comes to Ritschl from Wolf.11 But within this broad perspective he also


10. He presents his stemma with the words, “This is produced more or less this stemma of the manuscripts” (C. G. Zumpt 1831: lxxxiii n. 11). The term *stemma* re-appears later in Schröder’s preface to Martial (1844: xxxi–xxxii) (see below, pp. 101f.), in Bernuy’s dissertation on Lucretius (1847, see below, chap. 6, n. 8), and in Ritschl’s edition of Plautus (Ritschl 1849–54: lxxxvii). Earlier, Ritschl had used other expressions: in his edition of Thomas Magister he writes, “The connection of both relations can be displayed in a single diagram in this form” (1832: xxxi), and in his articles on Dionysius of Halicarnassus of 1838 and 1847 (see below, n. 13) he speaks of *artificem* [device]. But, as Konrad Müller points out to me, the notes Ritschl took in Italy in 1857 already contain the German expression corresponding to stemma: “a formal genealogical tree (from the friend-genealogical Steuernamibuc) for the descent and relation of all the fathers, brothers, cousins, and nephews in the great family of Plautus manuscripts” (cf. Ribbeck 1879: 1.101). Madvig speaks of *tabula in tabula* in his 1853 dissertation (see below, p. 97), as Bengel already had a century earlier (cf. above, p. 65); Schlyter (cf. n. 8) used the expression *schema cognomini Codicum manusciptorum* [scheme of the kinship relations among the manuscripts], cf. Holm 1972: 53.

11. Ritschl 1852. We must also recall Ritschl’s studies, only a little later, on Oros and Orion, and on the Alexandrian libraries, which demonstrate his interest in the history of ancient philology.

12. Ritschl writes, “In order to see clearly the history of the text, if I may use a recent term” (1852: xxxiix) (cf. also below, n. 14: “the history of the text as we now say . . .”). And Wolf had spoken of the *Geschichte des Textes* [history of the text], for example, in his preface to Plato’s Symposium (F. A. Wolf 1864: i.14)).
Reads himself the task of reconstructing the precise genealogical relations between the manuscripts: on page xxx of the prolegomena to his edition of Thomas Magister we find a stamata of the manuscripts and first printed editions, in which lost manuscripts are indicated with Greek letters, according to what will become the customary usage:

Clearly this is not a genealogy a primo fonte deducta [derived from the earliest source]: Ritschl does not explain the relation among the four ancestors ΦΧΩΟ, which, as his words and a brief examination of the critical apparatus suggest, is not a pure and simple direct derivation of all four from an archetype or Byzantine edition. Indeed, as we have seen, not even the stamata of the manuscripts of the Verrine orations which Zumpt traced one year earlier started out from a single ancestor for the whole tradition—from this point of view, once again, Schlyter’s stamata is more similar to the ones that became customary later. Ritschl makes another step forward beyond Zumpt by specifying the position of each and every manuscript within the stamata: Zumpt had been satisfied with indicating some groups of related manuscripts. But the greater specification means greater complexity: Ritschl was facing a very contaminated tradition—as was only natural, given that this was a Byzantine anthology widely used in medieval schools—and was obliged to indicate a double derivation for many witnesses. With all those intersecting lines, his stamata already resembles the ones that are found more and more often in recent critical editions and that aim to give some idea of the manuscript tradition in all its disarray, without convenient but arbitrary simplifications. But when the disarray is excessive, it is better to give up on the stamata.

Some years later Ritschl investigated the manuscript traditions of Dionysius of Halicarnassus and Plautus by the same method. For Dionysius, he based his genealogical reconstruction only on internal data, on shared cor-

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14. Ritschl himself distinguishes two kinds of history of tradition, for example: “While in other cases it is possible first to investigate the history of the manuscripts and then to move on to judging their value (this happens, e.g., in the manuscripts of Plautus), we have gone in the opposite direction ourselves, first weighing the value of the individual manuscripts and then moving on to outlining the history of the text (as we now say) even without external evidence” (1838: 25). Cf. Ritschl 1849–54: 1.3xxvi.
tion of the text. Later, returning to this question, he concluded that they were _descripti_ and hence should be eliminated: thus the choice was really between the Chigiuanus (Vatican, Chig. gr. 58) and the Urbinus (Vatican, Urbin. gr. 105) alone, so that it became impossible to apply any sort of mechanical criterion.

Thus Ritschl and Lachmann present a striking contrast: the former was passionately attached not only to _ars critica_ in the sense of Gottfried Hermann but also to the history of the tradition as such and tended to transform it into cultural history, like Wolf before him; the latter was completely intent upon the goal of freeing the text from late interpolations and restoring it to the oldest attainable form but was indifferent to the kind of genealogical inquiry that was an essential step toward reconstructing the archetype. There was also a difference in their explanatory styles: Ritschl loved didactic clarity, to the point of lapsing sometimes into a certain verocity, while Lachmann preferred an oracular style made up of sentences imparted from above and intelligible only to initiates. For the same reason, as we shall see, Lachmann did not adopt the use of _stemmata codicum_ even in his very last editions, although these had rapidly become popular and were even introduced into his own school by Karl Nipperdey.

To use a _stemma_ to make the history of the text more understandable would have seemed to him a narrow-minded pedantry.

15. Ritschl 1838: 25. Ritschl speaks sometimes of _Venantus_, sometimes of _Veniati_, for he was not entirely sure whether the incomplete and secondhand information available to him referred to one manuscript or to two (1838: 19–20).

16. See his _stemma_, quite different from the preceding one, in Ritschl 1866: 1:599, 496 (for the explanation of his symbols). In a specimen edition he published in 1846 he had already written, "Thus it is clear that for emending Dionysius it is better to weigh carefully the meaning and style of the individual words and sentences than to investigate the value of the manuscripts" (Ritschl 1866: 1:492). Karl Jacoby adhered to this criterion in his Teubner edition of Dionysius' _Antiquitates_. As far as I know, no one since Ritschl has ever reexamined the entire manuscript tradition to see whether there might indeed be manuscripts not copied from the Chigiuanus or the Urbinus. It would be highly advisable to do so, especially now, when historians and scholars interested in the most ancient Roman institutions have become more willing to ignore Dionysius' wordy rhetoric and to attribute a high value to his work.

17. He himself declares in his _prolegomena_ to Plutarch, "I confess that my unborn nature leads me to employ a somewhat fuller style than experienced readers might seem to need, since I hear in mind above all the needs of those who wish to learn this discipline" (Ritschl 1849: 54: 1:3). On Ritschl as a philologist and teacher, besides Ribbeck's classical work (Ribbeck 1879), see the lively evocative in Schmid 1968: 131–37, 141–42.

18. Nipperdey 1847–56: 1:46 (I owe this indication too to Konrad Müller). Nipperdey studied first with Moritz Haupt, then with Lachmann. His _prolegomena_ to _Cæsar_ contain no genuine methodological novelties, but his arguments are very rigorous and clear.

19. Madvig 1833 (= Madvig 1834: 411–70). In the second edition, Madvig 1887: 333–407, he modified this text and suppressed some parts of the original version, because in the meantime the objections of Karl Hahn had convinced him that the tradition should be divided into only two families, not three.

20. Madvig 1833: 7 (= Madvig 1834: 416). Madvig gives examples of such lacunas and errors at 1833: 802 (= Madvig 1834: 660). In an edition of some of Cicero's speeches he published in 1820, he did not yet trace out any _stemmata_ nor use the term _archetype_ but had already written that all the manuscripts of the _Pro Rutilio Amerino_ are "derived from one not very good manuscript and share the same errors, interpolations, and lacunas" (1834: 1:18). As we have seen, more and more scholars were beginning to recognize that the derivation of several witnesses from a single ancestor can only be demonstrated by co-occurrence in lacunas and serious corruptions, not in correct readings, and not even in innovations that can derive from contamination or polygenesis; this conviction is already clear in Madvig, even if it will only be explicitly codified later.

21. Several years later, the expression reappears in Orelli (1837: 4:2) in the Greek form _διὰ τῆς τούτων_; he had already used it occasionally in a more general sense. Then it was picked up by various other classical philologists and was finally sanctioned by Lachmann (see below, p. 102).

22. Madvig 1833: 22 (= Madvig 1834: 434): "While the individuals [sic: inferior manuscripts] have absolutely no authority, it has already been demonstrated by examples that, especially when they are compared with the best manuscripts from other families, readings can be extracted from them that were found in much older and better manuscripts and in the origin of this lineage, and that when these are found they can be used to restore the authentic text and to adjudicate the disagreement of those ancient manuscripts of the first and second family which now should rightly prevail."
with Lachmann’s mechanical method. Madvig is also fully aware that medieval manuscripts already present not only mechanical corruptions but also intentional alterations to the text, though to a lesser degree than Humanist ones; he considers this fact to be “strange” but nonetheless undeniable—a awareness that Lachmann always lacked.  

A few years later, Madvig will give a fuller application of these principles of textual criticism in his famous critical edition of Cicero’s De finibus with commentary. Here he maintains the distinction of manuscripts into meliores [better ones] and detiores [worse ones], which Bremi and Goerenz had already made, but he understands the two families genealogically and not just axiologically; he demonstrates that the manuscripts of De finibus are all derived from a single archetype and shows how the detiores too have a function in reconstructing that archetype.  

A little later (1843) Hermann Saupe published his Epistola critica ad Godofredum Hermannum [Critical epistle to Gottfried Hermann], in which he explains some of the principles of recension and emendatio with exemplary clarity. In this case too the first impulse will probably have come from Orelli and his circle: Saupe taught at Zurich from 1832 until 1845, in close contact with Orelli and his student and collaborator Johann Georg Baiter.  

23. Madvig 1833: 10 (= Madvig 1843: 419): “But what I have said must still be explained, namely, that the Parisinus, even if it is the oldest manuscript and as a whole the least interpolated one [. . . ], nonetheless in a few passages [. . . ] has clearly been interpolated by the substitution of a rash conjecture for a corrupted reading in the archetype—certainly a very strange thing to have happened in that period.” In the specific case of this manuscript, the existence of “rash conjectures” is anything but proven. But the important thing is the general criterion. For Lachmann’s different attitude, see below, p. 111.  


25. He cites himself at Madvig 1869 (1859): xiii. Johann Heinrich Bremi, a student of Wolf’s, had published an edition of De finibus in 1798 (Bremi 1798). For Goerenz see above, p. 93.  


27. Saupe 1841 = Saupe 1866: 80–87. Besides the section on eliminatio descriptorum, about which I shall speak shortly, see especially the passage on the classification of manuscripts (82; Saupe is quite aware that this must be based on “shared errors,” but he recognizes much more clearly than Ritschel, Madvig, and Lachmann that contamination almost always makes this very difficult or impossible; the excellent criteria for using the indirect tradition (111, even if Saupe does not evaluate some of his particular examples correctly) and the discussion about different kinds of corruptions and the rules for emendatio (121–77).  


29. Nicolas Boileau publicized Boivin’s discovery, and a little later Zacharias Pears supplied some further details: cf. Hemmerdinger 1977: 318; Boivin’s annotation, of which I have only quoted one phrase, is cited fully by some editors of the Heptaprapos, e.g., Rostagni 1947: xxxv. The article on Boivin in Nouvelle biographie générale 1855: 1479–80 is inadequate. On his conjectures on the Gestis of Julius Africanus, of which he started a translation that he did not complete, cf. Vieillefosse 1970: 81; 87; on his discovery of a palimpsest, one of the first such discoveries ever, cf. Timpanaro 1980: 249–50.  

30. Cf. Schweighäuser 1801–77: 1.xxxix–ci. Käfle 1887–90: xxviii–xlix does not seem to me to assign Schweighäuser enough importance: he claims that Dindorf, in a later article (Dindorf 1870), was the first to demonstrate that all the other manuscripts are derived from the Venetus. But in fact Schweighäuser’s arguments are already conclusive.
crowded, but for now I have the impression that the technique of *eliminatio* was "rediscovered" independently on several occasions—after all, in certain cases one arrives at this technique with relative ease. In any case, even if Sauppe cannot claim the priority that some have awarded him, he can claim to have fully formulated this technique, which had been entirely neglected in times close to his by Bekker, with consequent harm to his edition of the Attic orators, and had always been neglected by Lachmann, as has already been indicated. He also gave an excellent application by demonstrating that Palat. Heidelberg 88 is the ancestor of all the other manuscripts of the orations of Lydias (except for the *Funeral Oration*, which has a separate tradition), since a lacuna that in the Palatinus is due to the loss of eleven pages recurs in the other manuscripts without any material damage being detectable in them. To be sure, this very same *Epistola ad Hermannum* and other writings of Sauppe's also contain rash *eliminatones* based not on indisputable evidence but on mere presuppositions. Sauppe got carried away with his first success and extended the procedure of *eliminatio* beyond its legitimate limits. But it should not be forgotten that, in the case of the manuscript tradition of Florus, it was he who reacted against the overestimation of the value of the Bambergensis (Bamberg Class. 31 [E.iii. 22]) taken alone. A little later the Dutch scholar Carel Gabriel Cobet went on to show far less caution in his famous *Oratio*, which laid out the program of his future work: though he was a fine expert on the Greek language, he was hyper-analogist as a critic, and regarding manuscript tradition was convinced that the dependence of the *recensitores* on one or two late ancient or medieval manuscripts (which he called *archetypoi*, with a usage closer to that of certain Humanists than to Mädvig's and then Lachmann's) was not just one of the possible cases but was the most frequent (indeed was almost constant) and, what is worse, the most "desirable" one. This was the beginning...

31. Sauppe 1896: 83–84. Cf. the preface to Baier-Sauppe 1819–43. Earlier, Bekker had considered codex C (Florence, Biblioteca Medicea Laurenziana, Lab. 77.41), one of the apographs of the Palatinus (Heidelberg, Universitätsbibliothek: Codex Palatinus gr. 88), to be superior to all the other manuscripts of Lydias.


33. Sauppe 1870 = Sauppe 1896: 68–78. Cf. the preface to Malcomson 1918: vi. Only with Malcomson's edition has a fair evaluation of the Bambergensis been achieved; it is certainly the best manuscript (Otto Jahn emphasized it), but it does not deserve to be constantly preferred.

34. Cobet 1847: 26–27, 278, and 102–57. See esp. 27: "And we hope with some confidence that someday either one manuscript or else very few will supply a complete basis for the recension and emendation of every single author." Similar ideas recur in various later writings of Cobet's. For criticism of this approach cf. Wilamowitz 1922: 90–91; Kermer 1974: 218–20. Hemmerdinger 1977 contains much interesting material intel-

nigent discussions, also concerning earlier cases of *eliminatio* (Boeijn, Schweighäuser), which I had not noticed; but all in all this essay is too apologetic with regard to Cobet. Kramer 1844–52: 1.ii–iv had warned against too hasty *eliminatones* but was not listened to enough.

35. Cf. esp. Maas 1958 (1957): p. 4, sec. 8 (a) (to which we shall return later, p. 135) and the unsatisfactory discussion of *recensitores*, *non deteriores* in the "Retrospect 1956" (pp. 12–13), ending with a quotation of Cobet's motto "Carmurendi, non conferendi" [They should be incinerated, not investigated]—one piece of evidence among many that Maas understood nothing of Pasquall's work.


C. "** in chap. 2.
Studies on the Text of Lucrètius

Lachmann's last two works of textual criticism were his editions of the Agrimenesores and Lucrètius. The first volume of the Agrimenesores appeared in 1848: Lachmann edited the text but did not provide it with a preface. The second volume did not appear until 1852, when Lachmann was already dead; the complex history of the tradition was explained in it very fully and accurately by his student Friedrich Blume, an excellent investigator of manuscripts (it should be enough to recall his Iter Italicum).

Of all the most important one Greek and Latin authors, Lucrètius was perhaps the most suitable one for applying the canons of the new art critica: only a small number of medieval manuscripts, whose genealogical relations are easy to reconstruct; a mass of Humanist manuscripts that can certainly be neglected without detriment to the recentiores, even if they do not derive from medieval manuscripts known to us (but today such a derivation seems highly probable). For Lucrètius too, as for Cicero, the way was indicated by Orelli and Madvig. In 1827 Orelli referred briefly to the fact that all the manuscripts of Lucrètius are derived from a single ancestor; in 1832 Madvig confirmed this hypothesis and noticed the particularly close relation between the schedae Gottorpii and one of the two Vossiani, the one that Lachmann would later call the Quadratum. But it was above all starring in 1845 that Classical philologists focused their attention and rivalry on Lucrètius. In that year Lachmann began to work on his edition, which saw the light in 1850; and in that very same year, at the initiative of Ritschl, Bonn University established a competition for a study on the text of Lucrètius and the criteria for a new edition. The winner was a student of Ritschl's, Jacob Bernays; his dissertation was published in Rheinisches Museum in 1847. And in the meantime, in 1846, another young scholar, Hugo Purmann, had dealt with the same problems; he had studied with Ambrösch and Schneider in Breslau but was influenced above all by Madvig's writings on textual criticism.

In the preface to his Lucrètius, Lachmann refers to the works of Purmann and Bernays with haughty condescension: "Once Johann Nikolaus Madvig, an extremely erudite man, had indicated the proper method, two very well trained youths, Hugo Purmann of Silesia and Jacob Bernays of Hamburg, [. . .] worked with great energy and some success on evaluating the evidence for improving the text of this poem [. . .]. But although they invested their labor in this subject in a way that was no doubt quite praiseworthy, nonetheless, if I may speak the truth, they did not seem to contribute anything to it as a whole for me, since some of the things they said were ones of which I was already fully cognizant, while other things they either left out or else mixed up with errors and thereby contaminated, since they were young men insufficiently experienced in recognizing Lucrètius's peculiar talent" (Lachmann 1850b: 4). And he absolves himself from any obligation to cite them in the rest of his work, justifying this refusal by his habitual disdain for bibliographical minutiae: "Therefore my agreement with them in certain matters will make it easy to tell when I think they are right: but I prefer not to indicate everything in detail so as not to burden my readers" (1850b: 4). With the same haughty tone he attributed to himself the merit of having been the first to adopt the term archetype in a technical sense, although in fact, as we have seen, this was due to Madvig, and Madvig had already been followed by Purmann and Bernays in this usage.
In fact Purmann’s work and Bernays’s differ in originality and completeness. Purmann proposed several shrewd conjectures but 1 did nothing more for the classification of the manuscripts than add some further confirmations (not all of them sure ones) to what Madvig had already noticed, namely, that all the manuscripts are derived from an archetype and that the Quadras- tus and the Schedae are particularly closely related.9 Purmann had no clear idea of the importance of the other Vossianus (Lachmann’s Oblongus) for various reasons, including the lack of a reliable collation, and he even inclined to classify it among the deteriores, since he was misled by its real affinities with the Italici.10 The only thing that was really new, even if it was still expressed imprecisely, was an observation regarding the script of the archetype or pre-archetype, which Lachmann neglected.11 Bernays too started out from Madvig, but he studied the problem in much greater depth than Purmann did and succeeded in designing a stemma that is basically correct:12

Stemma we reproduce on p. 205). But the fact that Lachmann stated at the beginning of his preface, “that exemplar, the ARCHETYPE of all the others (that is how I am accustomed to call it),” made people believe in general that he had been the first person to use the word in this sense (see, e.g., Pasquali 1932a [1934]: 3). Cf. also below, p. 111.

9. Purmann 1846: 7–23. I use Lachmann’s designation Schedae to indicate the totality made up of the Guttorni, now as Copenhagen (GK. Kgl. S. 232 27), and Vindobonensis MS 1579, which were particularly studied by Purmann: 1846: 11–16 after Siebel’s 1844: 288 had drawn attention to them. But it was only in 1857 that it was noticed that, although fols. 9–14 of the Vindobonensis belong to the same manuscript from which the Guttorni leaves were detached, the following fols. 15–18 derive from another manuscript (Goebel-Goebel 1857; cf. Dils 1939: 28–96, xix, who designates the former as V and the latter as U. In any case, I neglect this distinction here, both because it has no influence on the stemma codicum (C = V and U both belong to the same family as Q) and because it was still unknown to the philologist I am discussing (Madvig, Purmann, Bernays, Lachmann).

10. Purmann 1846: 16. “Given Haverkamp’s lack of consistency in indicating variant readings, it is not yet clear to me to which family of manuscripts Ludy. 1 [i.e., the Oblongus] is to be assigned, to the genuine and more sound ones or to the more recent and inferior ones. Nonetheless, as matters now stand, I would prefer to assign it to the inferior ones.”

11. See Appendix B, n. 9.

12. Bernays 1847: 5700. Ludg. 1 and 2 are the manuscripts that Lachmann will call Oblongus and Quadratus; Memm. is Lachmann’s codex Memminiusius (which seems to be identical to the Quadratus). Bernays 1847: 146, 350 already inclined toward this identification). Poggianus is the ancestor of the Italici. On the Guttorni leaves and the Vindobonenses see above, n. 9.

Studies on the Text of Lucianus

Exemplar generis 1.

Archetypus

Exemplar generis 2.


codd. interpol.

One can disagree (and, as is well known,13 scholars still disagree) about where to assign the Poggianus, the ancestor of the Italici; and one can point out that Bernays did not demonstrate with conclusive arguments that the Quadratus and Schedae are both derived from the exemplar generis 1 (exemplar of the first family), even if in fact it is entirely certain;14 but everything else is unexceptionable. And unlike Purmann, Bernays fully recognized that the Lugdunensis 1 (the Oblongus) was certainly no less valuable than the Quadratus.

Bernays’s article has exerted very little influence. Until the first version of this study of mine, everyone attributed15 to Lachmann the merit of having been the first to reconstruct the genealogy of the manuscripts of Lucianus.14 Even Usener, who appreciated Bernays’s extraordinary brilliance more than
anyone else in the nineteenth century, did not think it appropriate to include his dissertation on Lucrétius in the collection of Bernays's articles he edited "since it has been rendered obsolete by Lachmann."

15 Lachmann and Bernays themselves contributed to this unjust oblivion: the former by the condescending reference we have cited, which gave the impression that Bernays had only glimpsed in a confused manner what Lachmann had recognized as clear as daylight; the latter by his excessive modesty, which led him to admit fully Lachmann's superiority.16

It would certainly be unjustified to accuse Lachmann of plagiarism. A letter of his to Moritz Haupt makes clear that he had decided that he would read Bernays's article in its entirety only after he had finished the first draft of his preface to Lucrétius to his own satisfaction,17 and there is no reason to doubt that he did just this. But what matters here is not a dispute about chronological priority but rather a comparison between the one scholar's results and the other's; and the comparison is not entirely favorable to Lachmann. Lachmann's mistake does not consist in his having made use of Bernays's work, but rather in his not having made sufficient use of it.18

As a matter of fact, Lachmann went beyond Bernays in two regards: eliminatio lectio nium singularium and reconstructing the archetype's external form. Bernays did not think of eliminatio lectio nium singularium, just as his teacher Ritschl had not thought of it in the case of Dionysius of Halicarnassus. Although Bernays understood clearly that the Schedae were not copied from any of the surviving manuscripts, in practice he reduced recentio to the two Vossiani alone: "Therefore the two Lugdunenses are the basis upon which alone the textual criticism of Lucrétius rests today" (Bernays 1847: 570). Lachmann, on the contrary, saw that the mechanical criterion could be applied to Lucrétius, just as to the New Testament, so that the agreement of the Oblongus with the Schedae against the Quadratus and

15. Ueber in Bernays 1883: 1. In fact, Ueber added, "and it is easily accessible for the specialist." But as we have seen, among specialists too no one has acknowledged Bernays's just merits.

16. This excessive modesty inspires Bernays's preface to his Teubner edition of Lucrétius, Bernays 1854.

17. Cf. Lachmann 1892: 180. Wolfgang Schmid has drawn my attention to this passage. All the same it is not free from ambiguity: Lachmann himself says that he has given a first glance at Bernays's article. So the observations of Kenney 1974: 107 and nn. 3–4 are justified; he also recalls that Lachmann's attitude to Bernays must have been influenced by the very strong hostility between Lachmann and Ritschl, Bernays's teacher. Nonetheless, I believe that the essential point, in scholarly terms, not moral ones, is the one I make immediately below in the text.

with the Quadratus against the Schedae gives us with certainty the reading of the archetype. It is true that the lack of accurate collations of the Schedae meant that he was only able to make a very limited use of this criterion; even today, indeed, eliminatio lectio nium singularium is not very useful in practical terms for Lucrétius, both because the Schedae are lacking for a large part of the text and hence recentio is limited to the Oblongus and the Quadratus, and because almost all the lectio nium singularum are obvious errors in that any case would not have misled any editors.18 All the same, there is no doubt that from the point of view of methodology Lachmann was ahead of Bernays in this regard.

Lachmann's ability to calculate the number of lines of every page of the archetype—and consequently the number of pages too—was based on the length of certain passages that were transposed or damaged.19 It was above all this reconstruction that impressed his contemporaries: "And where is

18. For a long time I have been hoping that some scholar would determine the real contribution of eliminatio lectio nium singularium to constituting the text of Lucrétius. Now this has been done by Alberi 1979: 60–61, and the result is just what might have been expected: "the lectio nium singularum are made up for the most part of quite bland errors which can be eliminated without recourse to mechanical criteria." On the other hand, in about twenty cases a lectio singularis supplies what is certainly the right reading, and polygenesis of errors must have taken place (according to Alberi 1979: 61; but in some passages I think one must hypothesize contamination or, even more, a correct conjecture on the part of a copyist). Of course, this does not imply a devaluation of the method of eliminatio lectio nium singularium, which has proven its practical utility for many other texts. And in the text of Lucrétius itself there is one passage, not mentioned by Alberi, in which the method functions usefully: 3.1, where the agreement of the Oblongus with the Schedae serves to confirm the correctness of the initial interjection C, missing in the Quadratus (the concept of lectio singularis also includes what might be called "zero readings," i.e., omissions); cf. Timpotam 1978: 135–93. In this case Lachmann accepted the bad conjecture E and thereby failed to make fruitful use of his own "method."

19. What is involved are not only the passages transposed in the Schedae and the Quadratus (these alone would have no probative value, since the displacement could have occurred in the archetype from which the Quadratus and Schedae derive: see above, n. 13) but also the transposition of 4.383–47 before 299–322 and the mutatiun of 7.1058–75 to 7.1054 which the Lucem after 1.1094 corresponds. This damage is found in all the manuscripts, hence it goes back to the archetype, and since it is explained on the hypothesis that each of the pages of the archetype had twenty-six lines, it follows with great probability that the transpositions of the Quadratus and Schedae too, which presuppose the same number of lines per page, go back to the archetype (from which the Oblongus had already been copied), not to the archetype, as Bernays had supposed; see the note cited just above. On this point too some of the recent editors (Ernout, Martin) are anything but clear.
this manuscript described with such precision? It was destroyed or lost; and yet there is not a single point in the description that is not demonstrated with almost mathematical certainty.”

20. Nowadays this kind of certainty has been quite shaken: doubts have arisen regarding the exact number of the pages and the script of the archetype above all, scholars have come to realize that they cannot use the reconstruction of the archetype for practical purposes (that is, to justify transpositions of whole passages) as hastily as Lachmann supposed. All the same, the reconstruction remains valid in its essentials and is a fine proof of Lachmann’s acumen.

But Lachmann’s explanation regarding the actual genealogy of the manuscripts is much more confused and contradictory than Bernay’s. The most important contradiction has not yet been noticed, as far as I know: it regards the place in the stemma to assign the ancestor of the Italici. As we have already suggested, this question was controversial for a long time and probably will never be finally resolved: scholars have disagreed — and they will continue to disagree — about whether the ancestor of the Italici constitutes a third branch of the tradition alongside the Oblongus and the shared ancestor of the Quadratus and the Schedae, or whether it was derived from a subarchetype from which the Oblongus also descends, or whether, as now seems almost certain, it was even derived from the Oblongus. But

22. See Merrill 1913: 227–29, 234–35. Doubts about the number of pages and lines were already expressed by Chorælus 1808: 71; cf. Ernest 1848 (1850): xi–xviii, but this is barred by nationalistic animosity. A more balanced and precise judgment can be found in Good 1958. For the script of the archetype, see below, Appendix B.
23. This is the view of Martin 1960 (1954); Smith in Leonard-Smith 1942: 114; Bailey 1947: 114–45. According to Böckner 1956: 201 (= Böckner 1964: 1.121–33), the ancestor of the Italici was in fact independent of the archetype to which both the Oblongus and the shared source of the Quadratus and the Schedae go back. But his hypothesis has been contested with good arguments by Pizzani 1951: 82–87 (cf. 54–78) and, even more effectively, by Schmid 1967: 475. A similar hypothesis had already been suggested by Chiari 1924, republished with an addendum in Chiari 1961: 3.2–27. Chiari 1961: 23–24 is right to defend his article against one unfounded objection raised by Pasqualli 1952a (1953): 113; but I think there is some validity to Pasqualli’s other objection, that Chiari too easily considered to be tradition what in the Humanistic manuscripts “can derive either from contamination or from conjecture,” especially from conjecture (ibid.).
24. As has been seen, this was Bernay’s opinion; it was taken up again by Birt 1915: 22. It is strange that it has enjoyed so little success. Whoever denies that the Italici are derived from the Oblongus must consider their descent from a shared subarchetype to be the most obvious solution, given the undeniable affinity between them. We shall see that Lachmann inclined toward this kind of solution, but incoherently.
25. This hypothesis, already maintained by Diels in his edition (Diels 1925–24: xxi–xxiii) and thereafter, more succinctly, by R. Heinze, H. Mewaldt, and U. Pizzani, has been


Lachmann does not opt for one of these solutions (as Bernay had done) or admit he was uncertain, but instead slips from the first to the second in the course of his explanation without noticing it. He begins by saying that three copies were derived from the archetype, the Oblongus, the ancestor of the Italici, and the ancestor of the Quadratus and the Schedae. In this case we would have one of those tripartite stenmas that notoriously constitute a very rare piece of good luck in the criticism of ancient and medieval texts:

\[
\begin{array}{c}
\text{Oblongus} \\
\text{Italici} \\
\text{Quadratus} \\
\text{Schedae}
\end{array}
\]

In this case the mechanical method would be quite fruitful: the agreement of two branches would give us the reading of the archetype with certainty; we would only remain in doubt if each of the three branches presented a different reading, but in practice this happens only very rarely.

But just one page later, although Lachmann still speaks of three independent apographs, he characterizes the Italici in the following way: “they are extremely similar to our Oblongus in every regard and yet they are not derived from the Oblongus; for sometimes they disagree with it and go together with the Quadratus, and indeed this happens in readings that no one could have arrived at by conjecture” (1850b: 5). While the derivation of the Italici from the Oblongus is decisively excluded, no reason is given why both the Oblongus and the ancestor of the Italici could not depend on a shared subarchetype (as we have seen, Bernay’s thesis was just this). The good (and, according to Lachmann, not conjectural) readings that the Italici share with the Quadratus might indeed go back directly to the archetype, but they

revived with new arguments by K. Müller 1973 and, even more analytically, by Cini 1976. And indeed it seems to be the most probable hypothesis. All the same, it does not seem to be possible to perform an eliminativo descriptum in “a T” like those of Politian, Boëthius, Schwenghäuser, and Sauppe, already mentioned by us. And so it is not surprising that the hypothesis of the independence of the Italici keeps coming up again: cf. now Flores 1978: 21–37 (attractively polemical and intelligent but, I think, rather lacking in arguments).

25. Lachmann 1850b: 3: “From that [sc. archetype . . . ] many have been derived from three copies, as far as we can tell.” 4: “But from those three apographs,” and he goes on to speak about each one.

26. Except, of course, for the cases of a disturbed tradition noted by Alberti (cf. above, n. 18).
might also have been found in a subarchetype, which the copyist of the Poggiana copied faithfully while the copyist of the Oblongus committed errors singularis. For the moment, Lachmann does not consider this possibility; but his reference to the extraordinary similarity (in correct readings or only in errors?) between the Italici and the Oblongus seems to indicate that, like Bernays, he was already inclining unconsciously toward the hypothesis of a bipartite tradition.27

For the moment this is a vague and rather uncertain suggestion; but it becomes an explicit declaration on page 9, where Lachmann writes concerning the norms for reconstructing the readings of the archetype:28 "It annoys me to have to speak too much about matters of no usefulness whatsoever; the whole tradition of the original reading must be derived from the Vossian manuscripts, except that sometimes the Italici cancel out the Oblongus's testimony, and once in a while, as I said,29 the Schedule diminish the Quadratus's authority" (1850:9). Here the only function assigned to the Italici is to eliminate the Oblongus's errores singularis, just as the Schedae serve in parallel to eliminate the Quadratus's errores singularis. Without saying so and, apparently, without noticing it, Lachmann has slipped from the hypothesis of a tripartite tradition to that of a bipartite one, a family represented by the Oblongus and the Italici, the other by the Quadratus and the Schedule—just as Bernays had maintained. If Lachmann had adhered firmly to the tripartite scheme he would have had to say that the Italici "sometimes annul the Oblongus's authority, sometimes the Quadratus's and the Schedule," and not the Oblongus's alone.

Once again we read a little later (p. 10), "I say this. Wherever it is clear from trustworthy testimonies compared with one another that there were two readings in the archetype (and this is the case whenever the Oblongus and the Quadratus disagree with one another and it is clear either from the Schedae or from the Italici, either manuscripts or printed editions, that nei-

27. Casfara 1964:612 has drawn attention to this passage. But perhaps it is an exaggeration to say that "Lachmann lacked in part the very concept of subarchetype and the ability to identify it and make use of it in constituting the text" (even if the words "in part" do well to lessen the force of the claim). When he wrote the final version of his preface, Lachmann had certainly read Bernays's article with its schema, in which the "concept of subarchetype" was quite clear. What is instead the case (and the observations of ours that follow will demonstrate it) is that Lachmann slipped from one stemma without subarchetypes to another one with subarchetypes without noticing it.

28. The words "as I said" refer to Lachmann 1850:8: "None of these Schedae, neither the Hammarskios nor the Vindobonenses, have any authority of their own except when they sometimes agree with the Oblongus."


30. One might perhaps suppose that, before reading Bernays's article carefully, Lachmann had become convinced that the tradition of Lucianus was tripartite, and that reading Bernays then made him incline toward the other hypothesis. But it remains strange that he did not revise his whole exposition to make it coherent. Probably the preface to Lucianus was written hastily.

31. Pasquale 1929:457. In the corresponding passage of Pasquale 1952:114, he seems to change his mind. In someone who knew the German language as perfectly as Pasquale did, the stylistic harshness of the phrase "der Denk- und halbbewusst umgestaltenden Tätigkeit der Schreiber" [the copyists' activity of thought and half-conscious transformation] is strange, even if the meaning is clear [conscious and semi-conscious changes
were double readings in many archetypes, and perhaps in Lucretius’s too, but on the basis of completely different arguments. And another peculiarity needs to be pointed out. We saw (chap. 3) that Lachmann’s distrust for the docti Itali, those insidious interpolators, was deeply rooted and that he tended to neglect a priori the manuscripts they transcribed. In the case of Lucretius, unlike that of other authors, such a distrust would have been legitimate, and indeed we have seen that it ended up prevailing. But in Lachmann it is far less strong than we might have expected. To be sure, in the course of his explanation he does “declass” the Poggianus from being a direct copy of the archetype to being a copy of a subarchetype; but he never supposes that it might be a descriptus (indeed, as has been seen, he excludes this possibility from the very beginning, and on this point he never belies himself), and he assigns it a function in reconstructing the archetype, even if only a subsidiary one. And even after that initial passage we have already quoted, he asserts that the Humanist manuscripts do indeed have “many […] conjectural emendations” but also other passages “which are less emended, that is, which better preserve the reading of the ancient archetype” (1850b: 6). Must we suppose—but the question will have to be studied further—that Lachmann might perhaps have been influenced by Madvig’s less negative attitude toward the recentiores of Cicero or Jahn’s toward those of Persius (see below, pp. 135f.)? In constituting the text, Lachmann went on to take account almost exclusively of the Oblongus and Quadratus, as was only right; but one can say that he presents himself in his preface, paradoxically, as an almost too ardent believer in recentiores non detestabunt.44

In any case, Lachmann’s preface to Lucretius contains the results he arrived at in the course of studying that specific manuscript tradition and not a general methodological exposition, such as is found in contrast in Madvig’s and Sauppe’s writings, which have already been recalled, and to a certain extent also in Lachmann’s own preface to the New Testament. On the first page of his book Pasqualli has characterized perfectly the tone of detached superiority that one senses in Lachmann’s preface to Lucretius; but it is not with the same degree of justice, I believe, that he then went on to say that Lachmann explained the method in this preface “in the most com-

33. Bliss 1892: 281 still considered the derivation of all the manuscripts from an archetype as only one of the possible cases. 34. Excellent conjectures are, for example, alia at 1.665, diri at 2.411, parcit at 2.660 (680), consequit . . . redarat at 5.679. Lachmann also collected the indirect tradition and used it very well, except in a few cases. 35. G. Müller 1958 and 1959 have tried to revive this hypothesis. K. Müller 1975 ad- here to it as well: hence his many deletions, which are always ingenuous but, in my judgment, too numerous. The problem would deserve a more detailed discussion; but my own view is that, even in many cases the hypothesis of an original text left incomplete by its author, with passages not yet definitively put into their proper location, “doubters” not yet eliminated, etc., has demonstrated itself to be little more than a convenient expedient for shirking the responsibility of constituting a reliable text, in the case of the De rerum natura what we know about its incompleteness and its posthumous publication makes that hypothesis still quite legitimate—without considering that it is characteristic of Lucretius’s style to repeat his verses, to retract his steps, in a certain way. This does not jus- tify a hyperconservative mode of criticism, but it ought to counsel caution. 36. The most celebrated of these observations is “Lachmann’s law” (1850b: 24, on Lucretius 1.835), according to which verbs of the type facio—factus are opposed to those of the type ago—actus, as is made clear by their compounds (effectus but exactus): cf. Leumann 1977: 114. Staring from Mommsen’s drastic negative judgment, the question

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introduced by copyists into the text). But a typographical error or the omission of some words is quite unlikely.

32. Pasqualli 1929: 498. For Lucretius cf. Diels 1925–24: xxix–xxvii (but almost all the cases Diels cites are anything but certain), and more recently, but quite unsuccessfully, Büchner 1962: 1218–1219 (cited by Schmid 1957: 476–78). But “archetypes with variants” is another question that ought to be reexamined from top to bottom. Cf. below, Appendix C, n. 51.
tion to our knowledge of the linguistic and prosodic peculiarities of the
archaic poets; especially for Ennius and Lucilius his achievement is in a
certain sense analogous, even if of smaller dimensions, than Ritschl’s for
Plautus in the same period. This in regard Lachmann’s commentary on Lu-
cretius is still alive and valid.

whether Lachmann had an adequate knowledge and understanding of the thought of Epi-
curus and Lucrètius has been much discussed: cf. Kieney 1974: 128 and n. 6. Probably he
did not, and certainly his commentary does not indicate that he did. But we must consider
that, with extremely rare exceptions regarding commentaries to individual books of the
De rerum natura, until now it has been almost impossible to find a commentator or ed-
tor or Lucrètius endowed in equal measure with abilities in the fields of philology and lin-
guistics on the one hand and in philosophical interpretation on the other.

37. He also undertook an edition of Lucilius; interrupted by his death, it was com-
pleted and published by Vahlen 1876.

Let us briefly recapitulate the results we have attained.

Ordinarily, when reference is made to “Lachmann’s method,” what is
meant is a complex of criteria for recensio. As we have seen, a number of
different Classical philologists contributed to its formulation.

1. The rejection of the vulgate and the requirement that the manuscripts
not merely be consulted from time to time but be used as the foundation of
the edition. We have seen that this point was already clear to Bentley, and
even more so to Ernesti and Wolf. Lachmann’s merits consisted in his return
to insisting on it after Gottfried Hermann had caused it to be almost for-
gotten and also in his application of it to the criticism of the New Testament,
where nonscientific reasons had delayed its application, even if in that same
field it had been theorized lucidly in the eighteenth century.

2. The distrust for manuscripts of the Humanist period. The predecessor
for Lachmann’s distrust (much reduced, as we have seen, in the preface to
Lucrètius) was Scaliger in particular (and earlier Politian and Vettori).

3. The reconstruction of the history of the text and particularly of the ge-
nealogical relations that link the extant manuscripts. This is usually consid-
ered to be an essential characteristic of “Lachmann’s method,” yet I believe
that I have demonstrated that Lachmann’s original contribution in precisely
this regard was very limited and uncertain. The true founders of the ge-
nenalogical classification of manuscripts are Schlyter (in a field different from
Classical philology), Zumpt, Madvig, and above all Ritschl and Bernays,
who concretely fulfilled the requirement to which Bengel had given expres-
sion. In particular, for the derivation of all the manuscripts of a work from
a single archetype we must go back even further, to Erasmus and Scaliger;
and, for the technical term archetype in the specific sense of a lost medieval
or late ancient ancestor, to Madvig. In the procedure of eliminatio codicum
descriptorum, the scholars who distinguished themselves were, after Poli-
tian, above all Boivin, Schweighäuser, and Sauppe. The first history of a
text in antiquity was attempted by Wolf in his Prolegomena ad Homerus.
producing this result: Lachmann's oracular tone, which ended up creating around him an atmosphere of veneration and "untouchability," as later around Wilamowitz; the tendency—sincere and affectionate but too exclusive—to glorify the master on the part of certain students and friends, above all Moritz Haupt. We have also recalled the impression produced upon his contemporaries by a fact whose importance is all in all only secondary—the calculation of the number of pages and lines of the archetype of Lucretius. But there were also more serious and fundamental reasons for this development. Whereas editorial textual criticism was only one of many activities for a Ritschl or a Madvig—Madvig was justly celebrated above all for his conjectural emendations in the field of Latin prose, to say nothing of his innovative historical and linguistic writings, Ritschl studied all the aspects of Plautine philology, and in particular prosody and metrics—this was the activity to which Lachmann primarily dedicated himself starting from his earliest years and to which therefore his name was linked. And above all, as we have shown, Lachmann was the one who aimed in the clearest and most immediate way at the practical goal of reconstructing the archetype without wasting time on problems of the history of the tradition—a limit, as we have said, but also a strength. He was a great simplifier, with all the virtues and vices this brings with it. The very one-sidedness with which he separated recensio from interpretation, though mistaken in itself, had a pedagogical function: it made a powerful contribution toward recalling scholars' attention to the requirement to give critical editions a solid documentary basis (a requirement that other Classical philologists had expressed in a way that was more complex and balanced, but for that very reason less harshly ef-

1. The first to make use of the stemma codicum in order to reconstruct the archetype was not Lachmann but Madvig; and if in the case of Bengel we must speak of a mere project that was not yet fulfilled, one cannot say the same of Madvig (c.f. above, pp. 97–98).

2. Perhaps more out of impatience than because of a true lack of aptitude, Lachmann himself remained strangely unable to reconstruct those genealogies of manuscripts which were the indispensable prerequisite for eliminatio lectionum singulorum: he needed to make use of genealogies traced out by others, and his preface to Lucretius shows us how difficult this was for him, so much so that he does not notice that he contradicts himself within a few pages.5

But at the same time as we reduce the dimensions of Lachmann's figure and acknowledge the claims of other Classical philologists who cooperated with him or preceded him in certain fundamental points, we must explain the reasons for which his fame as a textual critic has ended up obscuring that of all the others. No doubt various inferior reasons contributed toward

1. But this process happened gradually, and there was no lack of scholars who continued to recall the merits of some of Lachmann's contemporaries, even if only incidentally: e.g., Boeckh 1886: 205 recalls Sauppe together with Lachmann; Blass 1892 (1886):
fective:] *primum reconsere* [Do the reconsio first]! Although Lachmann's natural talent as a Classical philologist was less acute and profound than that of some of his contemporaries (Gottfried Hermann, Ritschl, Boeckh, Karl Otfried Müller) and although he tended more toward a certain dogmatism than they did[,] he still deserves a place of considerable prominence in the history of nineteenth-century Classical scholarship because of his salutary insistence on the problem of reconsio. And we will be able to continue to speak of "Lachmann's method," even if we will have to use this expression as an abbreviation and, as it were, a symbol, rather than as a historically accurate expression.

Already on one occasion (pp. 86–88) we have pointed to a parallel between the methods of textual criticism and those of historical-comparative linguistics. Now we must linger a bit more on this subject, not for a mere ostentation of interdisciplinarity but because the comparison really can help explain the difficulties that the application of Lachmann's method soon encountered and certain hostilities in principle to the method itself.

There is an undeniable affinity between the method with which the Classical philologist classifies manuscripts genealogically and reconstructs the reading of the archetype, and the method with which the linguist classifies languages and as far as possible reconstructs a lost mother language, for example, Indo-European. In both cases inherited elements must be distinguished from innovations, and the unitary anterior phase from which these have branched out must be hypothesized on the basis of the various innovations. The fact that innovations are shared by certain manuscripts of the same text, or by certain languages of the same family, demonstrates that these are connected by a particularly close kinship, that they belong to a subgroup: a textual corruption too is an innovation compared to the previously transmitted text, just like a linguistic innovation. On the other hand, shared "conservations" have no classificatory value: what was already found in the original text or language can be preserved even in descendants that are quite different from one another.

Naturally, like all analogies, this one too is valid only within certain limits: in linguistics there is nothing corresponding to the distinction between archetype and original; and even when a corruption has spread through the whole manuscript tradition, it is still always felt to be something that disturbs the context (and hence it is felt as a real error, or as a banalization that constitutes a deterioration too, not as a simple neutral "innovation"), while a linguistic innovation, once it has achieved success, ceases by that very fact to be felt as an error. But the parallel between the research methods of the two disciplines remains valid: indeed, it becomes even more evident if we
think of the linguistics of someone like Schleicher, who really thought he
could reconstruct the Indo-European language with the same certainty with
which Lachmann had reconstructed the archetype of Lucretius (to the point
that he presumed to rewrite the celebrated fable of the sheep and horses in
Indo-European), and who regarded phonetic changes as a "decline" from an
original state of perfection and therefore as being completely analogous to
textual corruptions.\footnote{1} And just as the philologists, beginning with Schlyter,
Zumpe, and Ritschl,\footnote{2} represented the genealogies of manuscript traditions
graphically by means of *stenomata codicium*, so too somewhat later Schleicher
introduced the use of genealogical trees into comparative linguistics.\footnote{2}
If one wished, one could also compare the trust of Lachmann and his con-
temporaries in the oldest manuscripts, and their exaggerated depreciation of
the *recentiores*, with the analogous prejudice on the part of the linguists of
that period (or better, of a somewhat earlier one: Jones, F. Schlegel)\footnote{3} that
Sanskrit, the language of most ancient attestation, always preserved the
most ancient phase as well.

These analogies authorize us to ask whether there might have been a di-
rect relation of influence of the one discipline upon the other. At first sight,
it might seem likelier that comparative linguistics, which arose between the
end of the eighteenth century and the beginning of the nineteenth, supplied
Lachmann, or better still Madvig and Ritschl, with a model for the method
of textual criticism.\footnote{3} Such a hypothesis might seem to be supported by
the fact that the founders of that method, unlike other Classical philologists
such as Gottfried Hermann, were interested in and sympathetic with the
new linguistic science.\footnote{3}

However, this hypothesis does not withstand closer examination.\footnote{8} In
fact, as we have seen, editorial textual criticism came about by developing
*ius suum propria principia* [close to its own beginnings], and Lachmann and his
contemporaries did nothing but systematize and apply coherently methods
that had already been formulated by the textual critics of the eighteenth cen-
tury (or, in some cases, already by the Humanists and Scaliger). Moreover,
there is no reference in the writings on textual criticism of Lachmann, Ritschl,
or Madvig to comparative linguistics. It must also be noted that in the pe-
riod 1830–42—when, as we have seen, the fundamental principles of the
new *ars critica* were being established—Indo-European linguistics had not
yet taken on the predominantly reconstructive character that it possessed
for Schleicher starting in 1850. For Friedrich Schlegel, for Rask, for Bopp,
the essential purpose was still to *demonstrate* the kinship among the Indo-
European languages and to go back from there to the problem of the origin
of the grammatical forms; they certainly were not yet thinking of precise gene-
alogical trees, of reconstructions of "asterisked" forms or even of texts in
Indo-European. So in the 1830s and 1840s the analogy between textual crit-
icism and linguistics was not yet as clear as it later became.

At first the inverse hypothesis, that the model of textual criticism had in-
fluenced Schleicher, seemed improbable to me as well. I thought I saw a di-
fficulty in Schleicher's rigidly naturalistic mentality: he had already estab-
lished a clear distinction between philology, a "historical discipline," and
linguistics, a "natural science," toward the end of the 1850s,\footnote{4} and had stud-
ied botany starting in his youth. But Henry H. Hoenigswald\footnote{5} has had the
merit of recalling a fact that was not quite unknown but had been too eas-
ily forgotten: the young Schleicher was seriously interested not only in bot-
any but also in Classical philology, studied with Ritschl at Bonn, and even
started his career as a textual philologist. So Schleicher probably derived the
idea of a genealogical tree of the Indo-European languages\footnote{6} and of recon-
structing their extinct mother language from Ritschl himself (one of the first

\footnote{1} Schleicher 1874: 139–201. "Philology is a historical discipline [...] Linguistics on
the other hand is not a historical discipline but rather a natural historical one [...]. The
object of glottic science is a natural organism." Cf. Schleicher 1865: 701. Mahler 1966 has
well demonstrated that Schleicher's naturalism is pre-Darwinian, and that even the work
of his that I have just cited does not represent a real change in his thought, notwithstanding
its title and the vague profession of Darwinism. We should not be misled by the adjecti-
"natural historical" in the passage just quoted: Schleicher is referring to *historia na-
turalis* in its old sense, for which *lógos* means "description" without any diachronic
implications. In any case, as is well known, Schleicher saw pure *Wesen* [becoming] and
even decadence in the diachronic evolution of languages (cf. above, p. 120 f. 1), while he
reserved the dignity of *Gechichte* [history] for conscious human history alone, in con-
formity with his early formation in Hegelianism, which lasted into his later materialism,
as into Jacob Molechoit's.

\footnote{2} Hoenigswald 1963: esp. 8.

\footnote{3} Morpurgo Davies 1975: 65–6515 has observed that there is already a genealogical
table of languages in Klaproth 1823. But as far as I know, Klaproth's work no longer en-
joyed great prestige or diffusion at the time of Schleicher. Hence the derivation of Schleicher
from Ritschl remains probable, even if Mahler 1966 and Morpurgo Davies 1975 are right to
note that an image as obvious as that of a genealogical tree could have arisen inde-
pendently in various diachronic disciplines.
to study the genealogy of a manuscript tradition in depth and to trace out *stemmata codicum*, as we saw in chap. 5) — even if reconstructing the archetype, the counterpart of this second linguistic operation, was practiced by Madvig and Lachmann, not by Ritschl, as we have seen and as Hoeningswald does not sufficiently emphasize.  

But beyond this direct connection (which, I repeat, is probable but still only hypothetical), one might think that the comparativist atmosphere widespread in all of European culture at that time could have favored the rise both of comparative linguistics and of that form of *vergleichende Textkritik* [comparative textual criticism] that is "Lachmann's method." Even before linguistics, comparative anatomy had stimulated a taste for the comparative method. But the relations between comparative anatomy and linguistics too cannot be reduced to a pure and simple influence of the one upon the other, as I hope to demonstrate in studies to be published shortly.

What is certain is that the similarity of their research methods ended up seeming clearer and clearer to linguists on the one hand and textual critics on the other. 7 Georg Curtius, a linguist who always claimed to be a philologist too and who insisted more than once on the necessity of a rapprochement between philology and linguistics, 8 developed the comparison fully in the introduction to his *Grundzüge der griechischen Etymologie* [Fundamentals of Greek etymology]:

The individual languages of the Indo-European trunk resemble just as many old copies of a lost original manuscript. None reproduces the original text exactly, but all are important for us inasmuch as they are old witnesses to a state of affairs of which we have no direct knowledge [. . .]. If we indicate with A the stage earlier than the differentiation of the Indo-European languages, the Greek language (C) and the Latin one (D) cannot be derived from it directly, but both go back to an apograph (B) which is lost for us, Greek—Italian, 9 which itself descended directly from A. In the same way there is a particularly close affinity between Sanskrit, which deserves the first place among all the copies of A for its legibility and correctness, and Persian, as well as between the readings of the Germanic languages on the one hand and those of the Balto-Slavic languages on the other [. . .]. To try to deal with etymological questions by limiting oneself to a single language is just as im-

8. E.g., Curtius 1862.
9. Curtius still believed in a particularly close kinship between Greek and Italic, as did most of his contemporaries, for that matter: cf. Meillet-Vendryes 1963: Introduction; De Voto 1938: 1.129.

Textual Criticism and Linguistics

As these last words indicate, Curtius intended that his comparison between the two disciplines would help convert Classical philologists to linguistics — many of them were still persisting in the footsteps of Lobbeck in constructing etymologies of Greek words based only on Greek. 10

Since then, the evolution of linguistics and that of textual criticism have continued to follow parallel lines. If the passage from Curtius that I have just quoted still displays an unshaken faith in the genealogical method, in the last decades of the nineteenth century such a faith began to falter among both textual critics and linguists. In textual criticism cases of the perfectly successful application of the genealogical method were not lacking (and they have not been lacking even in times nearer our own); it should suffice to recall Leo's preface to Venantius Fortunatus, of which Eduard Fraenkel has rightly emphasized the paradigmatic value. 11 In Romance philology too, the method has had distinguished applications; without tarrying in a field with which I am insufficiently familiar, I shall only recall two eminent names, Gaston Paris and Pio Rajna. 12

All the same, little by little scholars came to realize that the method achieved full success only in a relatively limited number of cases. All the manuscript traditions that were "too simple" (those represented by only one or two witnesses) remained outside its range, as did all those that were "too complicated" (those in which the copyists not only transcribed but also collated or conjectured so much that the kinship relations among the manuscripts were obscured). As we have seen, even the founders of the method had some trouble before they found in Lucrétius an author to whom the method was fully adapted; and even the tradition of Lucrétius is absolutely clear only if we set aside the problem of the Italic and leave out of consideration certain cases of contamination among the Oblongus, Quadratus, and Schedae as well. 13

In the face of these difficulties, some scholars followed a tendency that,
as we have seen, had its first representative in Lachmann himself: they preferred to cut the knot rather than untie it. They tried to eliminate as many manuscripts as they could, as suspected in general of being interpolated or descripti. 13 Once the manuscript tradition had been reduced to one or two manuscripts, every genealogical difficulty conveniently vanished; and so, for example, Wilhelm Dindorf could prepare editions of very many (too many) 16 Greek authors with little effort, and Eysenhardt could publish a Macrobius based arbitrarily on only two manuscripts; even a critic as prudent as Vahlen mistakenly eliminated one of the best manuscripts (the Heiniansium: Leiden B.P.L. 118) in his edition of Cicero's De legibus and refused to change his mind explicitly even when faced by Jordan's and C. F. W. Müller's stringent objections; 14 even Leo, whom we have already mentioned as an intelligent Lachmannian for his edition of Venantius Fortunatus and whose edition of Plotinus and Plautinische Forschungen would much later contribute to overcoming Lachmannism, published an edition of the tragedies of Seneca in 1878 that remains important for the metrical studies contained in its first volume but is fundamentally mistaken in its prejudice that the only independent witness is the codex Etruscanus (Florence, Laur. 37. 13). 15

Other critics realized that this was not the right way: instead, it had to be recognized that many traditions were extremely complex, since contamination and the innovations introduced by copyists and ancient and medieval "editors" 7 had played a large role in them, starting from the most ancient stages we can reach; therefore the mechanical method of choice among variants adopted by Lachmann was not applicable to these (or if it was, then only with many precautions and reservations). 8 This meant a positive reassessment of the internal criteria (lectio difficilior, usus scribendi) which Lachmann had despised, a return to principles already maintained by Classical philologists before Lachmann or those contemporary with him (recall how Sauppe had insisted on contamination), and, at the same time, an


14. See the preface to Vahlen 1883, and now the preface to Ziogler 1950: 16.

15. As is well known, the positive revaluation of the manuscripts of the so-called family A (certainly purer but less reliable than the Etruscan, but superior in very many passages) is due above all to Carlson 1926 and his later contributions; cf. Pasquale 1932a (1934): 126-49 and now many recent studies (by Philip, Gardina, Tarrant, Zwiebeln, and various others). Frankel in Leo 1960: 1. 7. 22 does indeed mention the defects of Leo's edition, but out of love for his teacher tends rather to minimize them. Besides the question of the manuscripts, I do not think one can say that Leo contributed "a large number of excellent emendations". Leo was a great interpreter and historian of Latin literature and culture, a distinguished metrician, but an infatuated conjecturer—of his conjectures on Seneca, in particular, almost none are still remembered.

16. I have learned much about this brilliant and nonconformist scholar, whose activities extended to many fields (philology, archaeology, history of ancient and modern music), from conversations with Edward Frankel. On John's attachment to Lachmann's memory, see the testimony of Gompertz 1905: 29.

17. John 1843: ccxxii—ccxvii—"Although I do not know whether I will succeed in convincing you that I have done well to indicate the readings of so many recent manuscripts too." In his editio maior (John 1851), John provided a much more concise apparatus.
immediately goes on to indicate that in those “all of them” he does not include all the most recent ones. In the specific case of Persius, Jahn was exaggerating somewhat; a genealogy of the manuscripts can be traced out, even if it is not entirely rigorous and goes back not to a medieval archetype but to at least two ancient editions, and it was Jahn himself who traced out its first outline; nor should the importance of the recentiores be exaggerated.18 But all the same the passage we have quoted has a considerable methodological value: it is a first rebellion (even an excessive one, let us repeat) against orthodox Lachmannian by a disciple and admirer of Lachmann.19 As we shall see, these ideas on contamination and on the impossibility of following mechanical criteria were taken up again and developed at the end of the nineteenth and in the twentieth century.

The crisis of comparative linguistics occurred at the same time as the crisis of “Lachmann’s method.” The concept of an absolutely unitary mother language, from which two daughter languages branched out and then went on in turn to produce by successive differentiations the various historically attested Indo-European languages, began to seem unsatisfactory. Already in 1872 Johannes Schmidt, a student of Schleicher’s, had opposed the “wave theory” to the theory of the genealogical tree.19 His ideas were developed further by his student Paul Kretschmer, who emphasized more and more the importance of “horizontal transmission” of linguistic facts as compared with “vertical transmission,” the only one that the theory of the genealogical tree had considered.20 Hugo Schuchardt had arrived at analogous results starting out from the study of the Romance languages. More and more—and with undeniable exaggerations—linguistic kinship started to become something that was not inherited but acquired by means of contacts. And more and more the “intermediate unities” between Indo-European and the historically attested languages were dissolved; after Italo-Greek was dissolved, it was Italo-Celtic’s turn (later, with Devoto, came Italo-Latin’s too). The original Indo-European language itself was conceived more and more as having already been rich in dialectal differentiations that could not be located geographically.

Here too the analogy with textual criticism is clear. In both disciplines the claim for the importance of “horizontal transmission” was made in the same period. Whoever wanted to have some fun writing a showpiece like Carttius’s on the analogies of the Schleicherian-Lachmannian period could note that for Kretschmer the Indo-European languages, or for Schuchardt

21. Schuchardt wrote, with a bit of exaggeration, “The number of genuine graphic mistakes is very limited; most of what are called lapsus calami [slips of the pen] are lapsus linguae [slips of the tongue]” (Schuchardt 1886–88: 1:17).

22. Le Clerc had attributed considerable importance to psychological corruptions and those due to the vulgar pronunciation; see above, p. 62f. Cf. also Hermann 1842–77: 6:43: “Substitutions of words which occur to the copyist at the wrong moment because they are in constant use cannot have a diplomatic explanation because their motivation is psychological.”

23. This tendency is noticeable, for example, in Ribbeck 1866; it reached grotesque extremes in Hagen 1879. On the other hand, Brunn 1889 demonstrated the frequency in the text of the Greek tragedies and Horner of psychological corruptions, including more complex ones than those investigated by Schuchardt: glossalizations, substitutions of words with a basically similar sound and length, etc. Brunn, repeating observations of Le Clerc’s (cf. above, p. 62f) in all probability unconsciously, called attention to the fact that the copyist reads relatively long passages of the original and hence when he copies them down is exposed to errors of memory and of “self-deception,” especially at the end of the sentence (or at the end of the verse in poetic texts).

24. On Haver’s studies as a young man, cf. Chatelain 1925: 32; and Frey 1926: 31, who observes correctly, “He enriched the domain of philology by making his concerns as a linguist penetrate into it.” Among Haver’s methodological works preceding his great Manuel de critique verbale [Manual of verbal criticism] (Haver 1911), one of the most interesting is Haver 1884; see on p. 804 his distinction between “servile mistakes” and “critical mistakes” and his assertion that this second kind of error is more frequent. Cf. also below, pp. 129–30.
best students were linguistic philologists, like Jules Marouzeau and Alfred Ernout.

All the same, in the second half of the nineteenth century, just as in the preceding period, the analogies between linguistics and textual criticism are the result not only of undeniable direct influences but also of a shared cultural atmosphere. Just as around the 1850s and 1860s linguists and philologists (and philosophers, and scientists) had breathed a common comparativist and evolutionary air, so too at the end of the nineteenth century people began to breathe an air of reaction against positivism. As is well known, this was a reaction that combined some justified elements (an impatience with hasty schemes and generalizations, the need for greater faithfulness to the complexity and variety of historical facts) with other more dubious ones (a return to a spiritualist metaphysics far older than the old positivism, a sophistic rejection of empirical classifications in the name of the uniqueness of the individual phenomenon, irrationalist and anti-historical tendencies now named “historicism”).

It is not up to me to recount how these latter aspects became stronger and stronger in linguistics, especially in Italy, during the course of the nineteenth century, and how scholars fell headlong from a conception of language that was still as historical and as rich in problems as Schuchardt’s into “aesthetics as general linguistics.” 25 But textual criticism too, a much more specialized and less ideologized discipline, witnessed not only justified criticisms of the excessive schematism of Lachmannism but also exaggerations in the opposite direction, caused by a desire to deny to every manuscript tradition any “mechanical” character whatsoever and to demonstrate that the history of textual transmission is essentially a “spiritual” history. For example, when the great Ludwig Traube wrote, “A conjecture does not become better because it can be explained paleographically, and certainly it does not become correct because in the best of cases it is paleographically possible,” 26 without any doubt he was right regarding the second proposition, but not at all regarding the first one; or, at least, that expression contained a certain ambiguity. 27 Traube’s lofty concept of paleography as cultural his-

tory in the fullest sense, which constitutes his glory, led him in this case to underestimate the strictly graphic aspect of textual transmission, which rarely is the sole cause of errors, but very frequently acts as one cause among others. 28 Among Traube’s conjectures to the Antologia Salmisiana there are some splendid ones, but others are faulty precisely by reason of their palaeographical improbability; 29 and the Antologia Salmisiana is one of those texts in which mechanical corruptions (graphic ones, or else ones due to the vulgar pronunciation, but not to the copyist’s whims) are prevalent. 29 We must also remind ourselves that “mechanical corruption” and corruption due to confusion of graphic signs are not identical: many psychological corruptions are just as unconscious and involuntary (and hence “mechanical”) as graphic corruptions are, or sometimes even more so. This is an ambiguity into whose trap many have fallen, sometimes even Pasquale (e.g., 1932; 1934; xviii, 114–115, and 481–86 [Appendix 2, “Conjectures and Diplomatic Probability”]). 30

So too, there can be no doubt about the extraordinary methodological value of Eduard Schwartz’s “Prolegomena” to his edition of Eusebius’s Historia ecclesiastica [Church history]; yet all the same Schwartz went too far in his distrust for genealogical classifications based on shared corruptions and in his view that horizontal transmission (not only of correct readings or of conscious innovations but also of real errors) was just as frequent as vertical transmission, or even more so. 31 It is true that prose texts have in general a much less mechanical transmission than poetic ones; it is also true, and Madvig had already noted it very exactly, 32 that mechanical corruptions are less frequent in Greek texts than in Latin ones, because the Byzantine Middle Ages had no Dark Ages comparable with those in the medieval West. But even so, a text like Eusebius’s Historia ecclesiastica is quite an exceptional case, even among Greek prose texts, since it was linked with theological disputes and changes in political affiliation and hence was exposed all the more to conscious rewritings; 33 so it is risky to assign it a paradigmatic value.

The French school was better at avoiding dangers of this sort. As we have already suggested, Haven’s Manuel de critique verbale (Havet 1911) does indeed represent a reaction against the Lachmannians’ oversimplification regarding the genesis of corruptions and the affiliation of manuscripts; 34 and

25. Many observations can be found in Nencioni 1946. On Crocean and Vossianic linguistics and a certain kind of Structuralism as two different forms of antimatériaлистic reaction (the one subjectivist, intuitionist, and aestheticizing, the other matériaлистic and “Platonic”), see my observations in Timpanaro 1975: 177–30. But it cannot be denied that nonetheless Structuralism has been much more fruitful and richer in scientific discoveries than intuitionist linguistics has been.


27. Naturally, the possibility of justifying a conjecture paleographically does not make it better from the point of view of meaning and style. But ceteris paribus, paleographical probability is a strong argument in favor of a conjecture.

28. Traube 1900–20: 3:57–59. Among his best conjectures may be cited those to Amb. Lat. 118, 58 and 304, 44 among the least successful ones, the one to 81, 88 and his complete rewriting of poem 177.

29. Schwartz 1908: cxlv–cxlvi. Even so, it cannot be denied that real corruptions can be transmitted horizontally, even if this happens only rarely: cf. below, Appendix C, n. 44.


31. “My whole book, in any case, is a continuous protest against mendacious oversimplification,” declares Haven in his preface, 1911: xii. And see at 1911: 418–24 his ex-
yet Havet has no wish at all to dissolve textual criticism into a multitude of individual problems that cannot be compared with one another. On the contrary, he even aspires to turn it into a rigorous science, a “pathology and therapy of errors”: the study of the genealogy of manuscripts is replaced by the study of the genesis of corruptions. As Ernout writes, “Louis Havet was an enemy of empirical conjecture, of what he called amateurs’ criticism. Between two corrections of different value for the same error, I am sure that he would have chosen the inferior one—or at least the one that most scholars would consider such—if this allowed him to determine more exactly the process by which the restored word, had it been the original reading, could have given rise to the faulty reading.”

Here, certainly, he went too far: although explaining easily the genesis of a corruption has great importance (more, I think, than Maas and Pasquali attribute to it), no genetic consideration can ever induce one to prefer the “inferior” correction. And in fact most of the too many conjectures Havet published in the *Revue de philologie*, many of which he collected in his *Manuel de critique verbale*, are unacceptable precisely because they try only to explain how the corruption was produced, not above all to find the right word for that particular context. Havet had many abilities, but he was not a great interpreter, and this harmed his activity as a textual critic too. But Marouzeau and especially Dain, his students, knew how to profit from his extraordinary experience and, together, partly to overcome his limits.

Among these new approaches to textual criticism, Giorgio Pasquali’s book occupies a special position. As is well known, Paul Maas provided the occasion for inspiring this work with his *Textual Criticism*; but the contrast between the mathematical arbitrariness of Maas (who was interested above all in the rigorousness of his formulations, without their always being rigorous in fact), and the lively sense of the uniqueness of each manuscript tradition that animates Pasquali’s exposition, leaps to the eyes of every reader. The true inspirers of Pasquali’s book were Wilamowitz, Traube, and above all

Schwarz, “the greatest textual critic of the century, the first one to overcome Lachmann in his method” (Pasquali 1952a [1914]: 471).

Other approaches that were akin to his own despite the difference in formation and cultural background remained little known to him, especially because of a prejudiced aversion, which went back to his youth and which lasted for a long time, against Classical studies detached from a general conception of *Altertumswissenschaft* [the science of antiquity], such as was practiced in France and England from the beginning of the nineteenth century on. In Pasquali’s last years he did indeed manage to approach the French school, some characteristics of which we traced out a little earlier in summary fashion, with interest and almost with an astonished sympathy: in his lengthy review of Alphonse Dain’s *Les manuscrits* [Manuscripts] he observed many points of agreement (especially regarding the genesis of corruptions and the history of tradition conceived as cultural history rather than as an abstract stemmatics) alongside some differences in intellectual character (Dain’s interest in text constitution, in *reconscrip* and *enmendatio*, was weaker than his predominant interest in codicology and the vicissitudes of the manuscript tradition: the title of Dain’s book had already indicated this, and it was confirmed later by his mediocre edition of Sophocles). Pasquali’s death nipped in the bud what might have become a very fertile exchange of ideas and experiences between Dain and himself.

Pasquali’s relations with English textual philology followed a rather different course. There really had been a period of depression in English Classical studies, more or less from 1855 (Dobree’s death and, a little earlier, Elmsley’s) until the last years of the nineteenth century (when the star of Housman rose on the horizon), a depression partly, but only partly, compensated for by the many good commentaries that appeared in England during that long interval. It cannot be said that Pasquali did not recognize Housman’s brilliance, but his image of Housman as a “Humanist,” not a

34. On French philology see the conclusion of Pasquali 1964 (1920): 89-90, with its bizarre final juxtaposition of Kant and Treitschke, a genius on the one hand and a mediocr and narrow-minded Realpolitiker (pragmatic politician) on the other, as the two greatest representatives of modern German culture. An important reason for Pasquali’s low opinion of French Classical philology were the all-too-numerous had, and sometimes terrible, editions published in the first years of the collection *Bella Lettre*: cf. Pasquali 1952b (1933): 242-43, 249 and esp. Pasquali 1951: 206-7 = Pasquali 1968: t.191, t.96, 379. On English Classical philology, aside from Housman and Lindsay, to whom we shall refer shortly, cf. Pasquali 1964 (1920): 81.


"scientist," remained too reductive, a bit because of insufficient knowledge, a bit because Housman's own aggressive character, his contempt for all routine and mediocrity, inevitably provoked in those who did not know him very well either irritation or a fanatical enthusiasm, but only rarely a reasoned admiration. Certainly, one notes in Pasquati a growing admiration for Housman, from the reference in Filologia e storia ("a scholar of acute but uncontrolled natural talent"); 1964 [1930]: 81) to the exclamation witnessed by Otto Skutsch, "There is only one man who knows how to make emendations, and that is Housman!" 38 to his judgment on Housman's edition of Lucan ("for knowledge of the poet's highly individual language and style, for judgment, for sureness in emendation, it is a masterpiece, notwithstanding the author's well-known eccentricity"); 1951 [1934]: 432n1; and in its spontaneity the assessment Skutsch reports is even quite exaggerated. But although Pasquati always defended the legitimacy and value of *omendatio* against short-sighted Italian critics, he was more interested in *recensio* and his lack of familiarity with Housman's edition of Manilius (in which the genealogy of the manuscripts is traced out with a sure hand), an evidently infelicitous hypothesis in Housman's edition of Juvenal, 39 Housman's fundamentally correct (but still somewhat too summary) liquidation of the genealogy of the manuscripts of Lucan as being completely contaminated 40—all this convinced Pasquati that Housman, brilliant as he was, was in essence unmethodical, indeed antimethodical. Certainly, Housman had words of contempt for Textgeschichte; but his *omendatio* was always guided by rigorous methodical criteria, and the material on various types of corruptions and their genesis which he collected in the prefaces and notes to his editions and in many articles confirms what he himself always repeated, that the "intuitive" element which one cannot do without in conjectural activity must receive the confirmation of experience and reasoning. His syntactical, stylistic, prosodical, and metrical observations, which he always considered to be a necessary support for his conjectures (or for his defenses of transmitted readings: these too exist, and for the most part they are excellent), go in the very same direction. 41

Wallace M. Lindsay, an English contemporary of Housman's, is cited in Pasquati's *Storia della tradizione* more often than Housman is—very often with agreement but with the conviction that Lindsay's "histories of the text" and his editions remain below the best results obtained by German Classical philology. 42 And it is certain that Lindsay was much better as an expert on Latin linguistics (his old Latin Language, in certain points, is still superior to Leumann's *Late- und Formenlehre* [Phonetics and morphology], on grammatical works and Latin glossaries, above all on paleography, than as an editor of texts (although his edition of Martial remains exemplary, 43 and his editions of Festus, Nonius, and Isidore of Seville, for all their defects, are unlikely to be replaced in the foreseeable future). But the greater accusation of neglect and forgetfulness of Lindsay must be lodged not against Pasquati and not even against the Germans, but against the English themselves. 44 It is not a question of opposing Lindsay to Housman: there is no doubt that

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37. It seems certain that Pasquati never had direct acquaintance with Housman's edition of *Manilius*. Pasquati 1964 [1930]: 81; 1951 [1934]: 432n1. This was observed simultaneously by Kenney 1974: 238n3 and Morigliano 1974: 370.

38. It must be acknowledged that, even if Housman has exerted an extraordinary positive influence on the rigor and sense of style of the English textual critics of the following generations (it is in large part his merit, besides that of German refugees in England, if all in all the Classical philology of this nation today enjoys an indisputable superiority over all others), he has nonetheless also stimulated an arrogance and an exaggerated meitism in some English Classical philologists to which their value as scholars does not always correspond (and even when the value is there, it would be better if the arrogance were not!). Cf. my observations in Timpanaro 1964: 790–91; Morigliano 1974: 568 on the *imitatio* Housmanni and also Salome 1981: 290.

39. Skutsch 1960: 6–7. I myself can add another oral testimony: the admiration with which during a seminar Pasquati spoke of Housman's celebrated punctuation and interpretation of Catullus 64.332, as simple as it is brilliant.

40. Housman 1951: xxxix. Pasquati's objection (1951 [1934]: 430n1) is entirely correct, as far as I understand it, but it is expressed with a polemic animosity all the more excessive as he agreed with Housman on the authenticity of that passage of Juvenal and disagreed only about the explanation for its absence in almost all the manuscripts.

41. Housman 1926: viii. Pasquati acknowledges that Housman "judges the tradition in its totality more correctly than any of his predecessors"; but he insists that even a contaminated tradition should be disentangled as far as possible and thereby affirms the in fact truly exemplary review of Frenkel 1926 = Frenkel 1946: 2.467–508. We cannot linger here on the later relations between Frenkel and Housman, which are fascinating not only from a human perspective but also because of the difficult symbiosis between English philology and the German philology transplanted into England as a result of the Nazi persecutions; there is a brief testimony in Housman 1972: 1.127–7.

42. Kenney 1974: 127–29 provides the best account of Housman's profoundly methodical character, despite his attacks against "method" as a form of routine; there are other excellent observations in Brink 1978: 1206–13.

43. It is especially in the case of Festus that Pasquati is constantly concerned to place Lindsay below Leo and other German Classical philologists, e.g., Pasquati 1951 [1934]: 331n1, 357n1, 538n4, etc.

44. I am referring both to his edition [Lindsay 1903] and to his *Ancient Editions of Martial* [Lindsay 1902]; Pasquati 1951 [1934]: 416–26 too give a clearly positive judgment of both works, and he even tended to agree too much with Lindsay on the problem of authorial variants, even though he was already then more cautious than Lindsay himself. See now Citroni 1972: xii–xiii.

45. Unless I am mistaken, neither Kenney nor Brink names Lindsay ever once.
Lindsay did not possess Housman's genius. It is a question of reconsidering a scholar who was certainly one of the greatest Latinists of the period between the end of the nineteenth century and the first decades of the twentieth, and who did not follow in others' footsteps but almost always worked in fields that had not been sufficiently explored hitherto.

Let us return to Pasqualli. His defense of the recentiores against prejudiced and hasty condemnations, and his insistence on the importance of contamination in rich traditions and on the nonmechanical character of very many corruptions are in keeping with the Wilamowitzian and Schwarzian inspiration of his work. Pasqualli was oriented in this direction not only by the teachers we have mentioned but also by his own direct experience as a textual critic (the letters of Gregory of Nyssa, which he edited, have a very rich and very contaminated and interpolated tradition) and his growing interest in medieval and modern philology and particularly in Italian texts, especially following the fruitful exchanges of ideas and experiences between himself and Vittorio Rossi, Giuseppe Vandelli, and above all Michele Barbi. This last aspect is especially evident in the last chapter of his Storia della tradizione, dedicated to authorial variants: here the rich documentation offered by the texts of Petrarch, Boccaccio, and Manzoni provides a starting point for going back to analogous phenomena in antiquity whose attestation is far more infrequent and uncertain.

Pasqualli was one of the Classical philologists most interested in linguistics, not old-style Indo-European linguistics—and, what is more, not Structuralism either—but the history of the Greek and Latin languages, and, in his last years, that of Italian. That is why the analogy of method between linguistics and textual criticism, to which we pointed earlier, is far more explicit in his book than in the writings of his predecessors. The very tendency we just spoke of, to apply methodical procedures elaborated in the study of modern texts to the criticism of ancient texts, has its counterpart in the linguistics of the late nineteenth and twentieth centuries, which started out from the study of Neo-Latin origins or even living languages (and not, like Bopp and Schleicher, from Sanskrit), and modified the earlier concep-

tion of the kinship relations among the Indo-European languages on the basis of these data.

All this might make one conclude that Pasqualli's book merely systematizes Schwartz's ideas, supplies them with many examples, and lends them further emphasis in certain points. Indeed, many felt that it did just this. Above all in Italy, in an atmosphere saturated with Idealism, and hence with the indiscriminate polemics against any classification and "mechanicism" to which I pointed earlier, the work of Pasqualli seemed to be an invitation to neglect altogether the greater or lesser authority of the manuscripts, to abandon any effort at genealogical classification, to put recentiores and recentiores on an equal footing, and to constitute the text solely on the basis of internal criteria. The hypothesis of authorial variants, which is legitimate only as a last resort, was invoked to explain obvious banalizations or even graphic corruptions. Pasqualli himself, in his preface to the reprint of 1954, wrote, "I fear that in this regard my work has done even more harm than good, and I feel the duty to warn beginners, even older beginners, of the perils of [late learners] in that kind of philology, to be cautious." 49

But in reality, even in the first edition, Pasqualli had been very far from proclaiming a pure and simple return to subjective "indicum. In this regard, indeed, a difference in mentality and orientation that had existed from the very beginning between Pasqualli and one of the teachers he loved most, Girolamo Vitelli, became clearer. It was to Vitelli, next to Schwartz, that Pasqualli had dedicated his Storia della tradizione. But for Vitelli, just as for

46. See, e.g., Pasqualli 1932 (1934), 261, 160 (on which cf. above, pp. 86ff.); and 1932 (1935): 133, 133, 136–139 = Pasqualli: 1960: 1.204, 206, 207–209 (acute observations on the parallels between linguistics and paleography, though here and there they are a bit flawed by Idealistic influences in linguistics); Pasqualli 1932b. The fact that the analogy between the two disciplines is still capable of being further developed and continues to allow fruitful exchanges of experiences is demonstrated by many of the communications at the Congress of Italian Philology collected in Studi e problemi di critica testuale 1962; see esp. Foleta 1965. Out of this volume later arose a periodical on the subject of textual criticism.

47. In fact, this happened not only in the years immediately following the appearance of Pasqualli's book, and not only in Italy. See D'Ale 1964, acute and learned, but too "angry" and destructive; the author learned of Pasqualli's book after he had already begun his research and reacted to it with enthusiasm (15707), but he ended up agreeing more with Schwartz than with Pasqualli (116).

48. Already in the first edition (1934: 199–200), Pasqualli showed himself more prudent than other scholars, although he sometimes exaggerated in hypostatizing authorial variants, and he warned, "Authorial variants are the last resort of textual criticism, and it is not legitimate to have recourse to them so long as the divergences can be explained in any other way."

49. Pasqualli 1924 (1934): 229; and already Pasqualli 1942: 257 = Pasqualli 1968: 216, 216, and 1947: 261. This caution ended up becoming excessive, also because of the polemics, often acute, but not devoid of quibbles, that Günther Jakobson directed against the hypothesis of authorial variants. Scelvi Manzotti, a scholar who at first had effectively and justly attacked some unmethodological hypotheses of authorial variants (S. Manzotti 1946: 316–17, 1965), later maintained that in certain cases the probability of authorial variants must be indicated, even if absolute certainty is unattainable, as is almost always the case in ancient texts (S. Manzotti 1954: 1954: 318). Cf. the methodologically rigorous analysis in Nardo 1967: 321–82.
Gotfried Hermann, *ars critica* was identical with the perfect knowledge of the style; although Vitelli was an extremely expert paleographer and investigator of manuscripts, nevertheless he did not admit that external considerations based on the authority of the witnesses had a significant weight in the choice of readings, and he felt distrust and even dislike for research into the history of tradition and the genealogy of manuscripts. So one can understand that he did not fully appreciate the value of a book on methodology, even if the methodology was as devoid of precepts as Pasqualli’s was. Commemorating his deceased teacher a few years later, Pasqualli referred with restrained bitterness to that lack of understanding: “In these last years, in which I knew him better, I sometimes even suspected that he condemned systematic disquisitions on the relations among the various manuscripts of an author as useless and found them distasteful [. . .]. Certainly, even a few months before his death, he claimed the right to constitute the text of a verse of Aeschylus according to his own taste without submitting to canons that he found mechanical; I have not succeeded in convincing myself that he was right, either in that particular case or in general.”

Even Pasqualli’s great admiration for Schwartz did not prevent him from noticing the exaggerations in certain methodological pronouncements by the editor of Eusebius to which we have already referred. Reread pages 136-41 of Pasqualli’s book, and you will see that his agreement with Schwartz is accompanied by reservations: “I consider exaggerated only the first of these words, which attack the concept of archetypus” (1952a [1953]: 136); “It would be mistaken to derive a presumption against the existence of an archetype from the number and quality of the variants” (137); “Not even here is everything right [. . .]. If it is true that errors can be transmitted by collation just as much as genuine readings can [. . .], nonetheless it is certain that the transmission of the text, the ‘tradition,’ occurs on principle in a vertical ‘direction,’ as is only natural” (140). Pasqualli also rejected Lec’s opinion that the whole manuscript tradition of Plautus goes back directly to an edition by Valerius Probus, and considered it indispensable to return to the hypothesis of an archetypus—even if not a medieval archetypus, but one of the third century AD (1952a [1953]: 339); and point 11 of the “Decalogue of 12 Articles” in his preface (1952a [1953]: ix–xx) proves that he considered this hypothesis valid for other authors too. In any case, the polemic against Lachmannism in Pasqualli’s book is never divorced from a recognition not only of its historical function but also of the value it still maintains in the present when we have to deal with mechanical recensions.

Certainly, the criteria of *lectio difficilior* and *ius scribendi* acquire primary importance in nonmechanical recensions. But Pasqualli’s discussion of these two criteria (1952a [1953]: 122–24) aims precisely to free them from the dominion of pure subjective taste (or from that of an abstract rationalism, no less subjective even if it deludes itself that it is “universal”) and to demonstrate that knowledge of the history of the tradition is necessary if they are to be applied well. “Easy and difficult are not absolute terms, and what is difficult, that is accustomed, for us could have been easy for people of other periods. Judgment regarding the ease or difficulty of a reading will be all the more secure, the better the judge knows the customs of language and thought of the periods that transmitted it and that might have coined it. The best critic of a Greek text with a Byzantine tradition will be the one who is not only a perfect Hellenist but also a perfect Byzantinist.”

The best editor of a Latin author transmitted in medieval or postmedieval manuscripts will be the one who knows the Middle Ages and Humanism just as well as he knows his author and his author’s language and times and the language of his times. A critic of this sort is an ideal that no one can incarnate perfectly in himself, but toward which everyone has the duty to try to come as near as possible.”

But Pasqualli did not even abandon external criteria altogether for nonmechanical recensions. He never completely resigned himself to Paul

51. I avoid Pasqualli’s terms *closed recession* and *open recession*, despite their popularity in Italy and abroad, because Alberti 1979: 1–18 has demonstrated that Pasqualli already uses both terms, but especially the second one, in too many different senses. This takes nothing away from the fact that in Pasqualli’s time these two expressions were significant and effective in the polemic against orthodox Lachmannism.

52. See also the discussion with K. Ziegler about clausula in the *Somnium Scipionis* (Pasqualli 1952a [1953]: 127–39); Pasqualli maintains rightly that a text cannot be constituted on the sole basis of *numerus* without taking the documentary authority of the various readings into account. Later, Ronconi and Castiglioni took up the problem of the divergences in the colloquation of words in the *Somnium* once again and gave two different explanations for it (cf. now Ronconi 1981: 40, 64). My own view is that Ronconi was right, at least in most cases, and I think he should have defended his hypothesis more decisively against Castiglioni’s. In any case Pasqualli’s requirement for the constitution of the text remains valid.
Maas's aphorism, "No specific has yet been discovered against contamination" (Maas 1958 [1927]: 49). A good part of the fifth chapter of his book is dedicated to the search for new "objective criteria," ones more sophisticated than Lachmann's and capable of "resolving disagreements in the case of an open recension" (1952a [1934]: 160). Pasqualli indicates one such criterion in the norm of lateral areas, which we have already had occasion to mention, and another in Ulrich Knoche's attempt to classify contaminated manuscripts of poetic texts genealogically on the basis of lacunae that impair the meter (1952a [1934]: 180–83). If there is a defect in this part of Pasqualli's book, rich in erudition as it is, it consists in his excessive faith that these criteria could be fruitful "not only for the history of the text, but for the text itself" (1952a [1934]: 177–78). The impression remains that when the history of a text is very complicated, it is not very useful for textual criticism, but otherwise has a value in itself, that it belongs to the history of culture, to the Fortleben [survival] of the Classics. And the practical exigency remains that certain critical editions not be postponed forever for the sake of studying the history of the tradition in all its smallest details, that scholars not bury themselves so deeply in the study of medieval and Humanist culture that they forget to return to textual criticism. Nonetheless, although in more recent works the separation between history of the tradition and textual criticism has now taken place and been codified, in Pasqualli's book the two disciplines are still conjoined. Pasqualli's interest in the vicissitudes of Classical texts in the medieval and Humanist periods, lively as it was, never makes him forget his job as textual critic and interpreter. It is in this combination of a broad perspective on cultural history with an acute philological intelligence directed to the individual passage of an ancient author that the unmistakable character of Pasqualli's work resides.

54. See above, pp. 36–87.
55. See, e.g., Hunger et al. 1961–64, which even introduces a further distinction between Zweiachsige (history of the text) and Textüberlieferung (transmission of the text), and supports it with a certain doctrinaire one-dimensionalism. Against this distinction cf. also S. Marinti 1966. In the last few years there has been a salutary reaction: more and more philologists first treat the manuscript tradition of a text in a monograph, making ample use of the assistance of codicology and cultural history, and then go on to do a critical edition of it with the same success. Other scholars, on the other hand, in textual criticism as in many other disciplines, have preferred the easy path of terminological exhibitionism to which no genuine conceptual progress corresponds; one of the worst examples of this tendency—it is useless to fool ourselves: it will certainly find admirers and followers—is the interdisciplinary seminar published as Del testo 1952.

APPENDIX A

Lachmann's First Attempt at a Mechanical Recensio in 1817

In July 1817 Lachmann published a long review of Friedrich Heinrich von der Hagen's edition of Der Nibelungen Lied (Hagen 1816) and Georg Friedrich Benecke's edition of Bonerius's Der Edel Stein (Benecke 1816). Lachmann distinguished two redactions in the manuscript tradition of the Nibelungen Lied: a shorter and more genuine one contained in the manuscript he called B; and another, longer and heavily interpolated one, represented by the manuscripts GEM. According to Lachmann, both redactions have reached us disfigured by corruptions and secondary interpolations; but while the first one cannot be reconstructed in its original form until another manuscript, a brother of B, is discovered, the second one can be reconstructed by comparing GEM.

According to Lachmann, such a comparison reveals that the ancestor of GEM was still fairly free of interpolations in the text written by the first hand but that a second hand added many changes and arbitrary additions upon the original text. Each copyist of GEM reproduced now the reading of the first hand, now that of the second hand, and also interpolated on its own. So in order to make Lachmann's thought easier to understand we could trace out the following stenmas:

1. Lachmann 1817, I warmly thank the director of the university library at Jena for sending me a photograph of this review. The review is reprinted in Lachmann 1876: 181–114.

2. B (now designated conventionally as A: Munich, Bayerische Staatsbibliothek, cod. germ. 54) and M [= D: Munich, Bayerische Staatsbibliothek, cod. germ. 54] are in Munich, E [= G: Karlruhe, cod. Donaueschingen 64] in Donaueschingen [now in Karlsruhe], G [= B: St. Gallen, Stiftsbibliothek MS 857] at St. Gallen. Lachmann 1876: 184n indicates the correspondences between Lachmann's symbols and those that von der Hagen had used and which Lachmann himself went on to adopt in his own edition of the Nibelungen Lied. Nowadays, in contrast with Lachmann's view, scholars generally believe that the shorter redaction is the more recent one.
there was a reading \( x \), that it was reproduced faithfully by \( B \) and \( \phi \), and that \( \phi^2 \) added a variant \( y \) in the margin. Let us also suppose that the copyists of \( G \) and \( E \) reproduced the reading of the first hand \( (x) \) and the copyist of \( M \) that of the second hand \( (y) \). In that case the agreement of three manuscripts \( (BGE) \) against one \( (M) \) would give us not \( \phi^2 \) but \( \phi \).

The same thing may be said in case the reading \( x \) was reproduced by \( GM \) and \( y \) by \( E \), or \( x \) by \( EM \) and \( y \) by \( G \). Obviously the agreement of \( BGM \), or the agreement of \( BEM \), would give us \( \phi \) and not \( \phi^2 \).

In reality, in contrast with Lachmann's rule, the agreement of three manuscripts against one gives us with certainty the reading of \( \phi \) (and of \( w \)), if one of the three that agree is \( B \). Only if none of the three that agree is \( B \)—that is, only if \( GEM \) agree against \( B \)—is it possible that the three preserve the reading of \( \phi^2 \). For one can suppose that \( \phi \) has the same reading as \( B \) (and as \( w \)), and that \( GEM \) have all three reproduced the variant of \( \phi^2 \). Such a hypothesis is not improbable, especially if we admit the possibility that the scribe of \( \phi^2 \) did not limit himself to adding the variant in the margin or above the line, but also deleted the reading of the first hand. In any case this is only one of the possible hypotheses: the agreement of \( GEM \) against \( B \) could also be explained on the supposition that \( \phi \) (not \( \phi^2 \)) had innovated with respect to \( w \) and that its innovation had been reproduced by its three apographs, or else that \( B \) had innovated and that the reading of \( w \) had been reproduced by \( \phi \) and hence by \( GEM \). So the probability that the agreement of \( GEM \) against \( B \) represents \( \phi^2 \) is not very high.

The second rule, on the other hand, is correct. When there are two readings, each one attested by two manuscripts, it is certain that the reading attested by \( B \) and by one apograph of \( \phi \) was already found in \( \phi \) and \( w \), while the other two apographs of \( \phi \) reproduce the variant of the second hand \((\phi^2)\).

So too the fourth rule is correct (and obvious).

But the third one presents real absurdities in the form in which it is printed in Lachmann 1817. The agreement of \( BG \) against isolated readings of \( E \) and \( M \) certainly gives us \( \phi \) (and \( w \)), not \( \phi^2 \), and hence it cannot be "preferable" in trying to reconstruct \( \phi^2 \). The agreement of \( GE \) against isolated readings of \( B \) and \( M \), or of \( GM \) against isolated readings of \( B \) and \( E \), can represent \( \phi^2 \), but can also represent \( \phi \). No less erroneous are the formulas \( BM = G-E \) and \( BE = G-M \) in the second part of the same rule: this time too the agreement of \( B \) with an apograph of \( \phi \) gives us with certainty \( \phi \) and not

3. Lachmann 1817: 138 = Lachmann 1876: 1.87: "The editor's task is to discover these changes, which now this copyist, now that one overlooked, and every one increased with new changes." Antonio La Penna has helped me understand the task Lachmann set himself.

4. Although Lachmann considered \( B \) to be the best manuscript, he still maintained that it was not free from corruptions and interpolations: "since it must be expected that neither the older recension in \( B \) nor the more recent one in \( G \) will have been transmitted to us without errors and arbitrary changes, partly negligent and partly intentional, on the part of the copyists." [Lachmann 1817: 117 = Lachmann 1876: 1.86].
B this is impossible. But in reality, as the parenthetic explanation ("the agreement . . .") also makes clear, the rule must be understood to mean that the shared reading of two apographs of $\phi$ and the different reading of the third apograph of B have an equal probability of reproducing $\phi$.7

Thus there is perfect agreement between the second and third rules: both rules mean in substance that a reading attested by one or two apographs of $\phi$ can represent $\phi^2$ as long as such a reading is not found in B too. As Bornmann notes, the two rules are parallel formally: in both cases Lachmann begins with a negative formulation ("such a reading is to be refused in favor of such another one") and then goes on to expound the second and third cases in a positive form ("such a reading is to be preferred to such another one").

All the same a striking contrast remains between these two rules and the first one, which we discussed above (pp. 149ff.). In this case we cannot hypothesize a typographical error; the incoherence was in Lachmann's mind. Bornmann acutely detects the reason for this incoherence in the "superposition of two criteria, one purely recensional, aiming to identify materially the interpolated readings of $\phi$, and the other, which I would call editorial, of reconstructing a text as close as possible to the original (which, however, is supposed to be capable of being reconstructed only in the class of manuscripts derived from $\phi$)." An examination of the concrete examples that follow the formulation of the four rules in Lachmann's article seems to confirm Bornmann's hypothesis.8 In fact there is something paradoxical about this whole complicated attempt to reconstruct $\phi^2$ (a series of interpolations!) when Lachmann could have even reconstructed the first source of the whole tradition, $\omega$, with much less effort and much greater profit. For the agreement of B with one of the apographs of $\phi$ gives us with certainty the reading of $\phi$ and of $\gamma$. It is perfectly natural that although Lachmann had set himself the difficult and all in all not very useful goal of reconstructing $\phi$, he then tended more or less unconsciously to reconstruct $\omega$. But it must be added that the first rule is erroneous not only from the point of view of reconstructing $\phi$ but also, and just as much, from the point of view of reconstructing $\phi$ and $\omega$: the agreement of GEM ("three manuscripts out of our four") against B can represent $\omega$, but it can just as well reproduce an innovation of $\phi$ or even of $\phi^2$. At this point Lachmann seems still to have been a victim of the false criterion of the majority, from which he will free himself only in his edition of the New Testament (see above, p. 83).


6. For a detailed reconstruction of the origin of the errors, see the observations of Bornmann 1962: 50–53.

7. Bornmann 1962: 49n2. "The meaning [of $EM = B - G$] is the coincidence of the two readings in E and M does not lead to a secure decision in comparison with G when G differs from B; that is, it does not permit us to reject G."

Three years later, in a review of a new edition of the Nibelungen Lied by the same von der Hagen, Lachmann reconfirmed the value of his rules, though he admitted the possibility that they could be rendered more precise. Had he therefore not noticed that the rules were almost useless, at least in the form in which he had published them in Lachmann 1872? Finally Lachmann published a critical edition of the Nibelungen Lied himself in 1826. But this time he did not try to reconstruct the longer and (according to him) interpolated redaction and adhered fundamentally to the manuscript that he had called B and for which he now adopted von der Hagen's designation, A.  

10. Lachmann 1851 (1846).  
11. For an attempt to demonstrate the correctness of Lachmann's rules without having recourse to corrections of this sort, cf. Lutz-Hensel 1971 and 1975: 228–29. Lutz-Hensel's interpretation, which Bornmann and I do not consider to be at all persuasive, will soon be discussed by Bornmann in Zeitschrift für Deutsche Philologie. In any case this eminent scholar's book, praiseworthy as it is for its rich and detailed information, is ruined by its nebulosity and captiousness. Cf. now also Cocchiola 1981, an intelligent article, which however does not resolve the question once and for all in my view. But Fritz Bornmann will deal with all this in the article whose appearance I announce in this note.

We have seen how Scaliger already tried to determine the script of the archetype of Catullus on the basis of some characteristic errors that appear in his manuscripts, and how Lachmann did the same thing for Lucretius (above, pp. 51–54, 107–8).

Attempts of this sort have a good chance of hitting the mark if they are performed with a rigorous method, and they are useful both for the history of tradition in itself and for textual criticism: for example, if one succeeds in demonstrating that the archetype was in capital script, conjectures that presuppose a confusion between minuscule letters become less probable, and so forth.

But if we wish to avoid getting lost in unfounded hypotheses, we must rely above all on a large number of readings that are certainly erroneous and cannot be explained otherwise than by the similarity between certain letters in a certain script. Errors that can be attributed to other causes too (that is, to confusions between graphic signs in other scripts, or to nongraphic reasons) have no probative value. For example, confusions between c and g, or between e and i, might indeed be due to erroneous reading of a model written in capitals; but, at least in many cases, they might also be phonetic vulgarisms (the confusions c=g might also provide an indication of a model in uncials, and an even better one;) and hence it will certainly be better to neglect them. Matters become even worse if we rely on readings that might be correct, or that could be corrected just as well (or better) in a different way too.

Once a large number of really probative errors has been collected, we must bear in mind the following points: (1) if each of the apographs of a lost manuscript a presents errors of its own due to misunderstanding a given script, then that was the script of manuscript a; (2) if the apographs of a present shared errors due to misunderstanding of a given script, then that was the script not of a but of the model from which a was directly or indi-