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Additional materials A

[Final Remarks on Bipartite Stemmata] *

Being refute inflict a wound, to be sure, on that petty person vain which almost all scholars possess and which I too possess. But on the other hand it also produces a sense of joy when the refutation is rigorous, lucid, based on vast scholarship and lively intelligence, and, at the same time, is free from personal rancor, indeed leaves a friendship intact. So, after having read and reread attentively the refutation that M. D. Reeve has written (Reeve 1986) about the third appendix [i.e., Appendix C, "Bipartite Stemmata and Disturbances of the Manuscript Tradition"] of my too-often revised and reprinted Genesis of Lachmann’s Method, I said to myself, ‘Well then! That appendix may well have persuaded even some notable philologists, but Reeve has finally cut it to shreds by all the rules of our craft.’ If I could have read Reeve’s article before the recent [1985] reprint of my little volume, I would have prevented that reprint. If that little volume— which continues to

* This text was found among Timpanaro’s papers at his death and has not previously been published; I am grateful to his widow, Maria Augusta Timpanaro, for making it available to me and permitting its publication. It represents a further stage in Timpanaro’s reflections on bipartite stemmata beyond the point reached in 1985 in the last published form of Appendix C of the present book, although the manuscript is undated, the similarities in argument and language to a letter written by Timpanaro to Paolo Mari on 20 October 1986 (published in Per Sebastiano Timpanaro, 2001: 176–83) and the fact that Timpanaro wrote this essay in part upon page proofs of Pasquale 1986, which he coveted, indicate 1986 as the likeliest date for its composition. Given the interest and importance of this text, it is appended to the present translation of this book, even though it was never published by Timpanaro himself and clearly expresses his firm intention not to permit the book’s republication. The manuscript did not receive Timpanaro’s final revisions for publication and is sometimes little more than a sketch or outline; in a number of passages it can be deciphered only with great difficulty, and in others hardly at all. Material in square brackets has been supplied by myself where the text is unreadable or lacunose but the meaning is fairly clear; when in such cases the meaning is not clear at all I have used the indication [. . .]. For the sake of convenience I have supplied the text with a title that corresponds to its contents.
sell copies, I believe, only because friends of mine who teach at universities often suggest it to their students—should happen ever to be sold out again, then I will do what I should have already done, since a reprint in which the third appendix was suppressed (or with a third appendix revised to the tune of self-criticisms and of mea maxima culpa [if my grievous fault]) would be extremely strange. My little volume has now fulfilled its purpose, it has also revealed its flaws (and not only in the third appendix), and now is the moment in which requiescat in pace [may it rest in peace].

I fear lest those readers who up to this point have appreciated my so-called eloquence (though in fact there is little to appreciate, since what is involved is my clear duty) will feel some consternation when they discover that the present article continues for a number of pages instead of stopping right here. They will suspect that I have made an insincere ostentation of scholarly rectitude and then wish to even the score by resorting to sophisms and to demonstrate that, all in all, that notorious third appendix is not as mistaken as Reeve thinks. But this is not the case. I remain convinced that that appendix, in the form in which I wrote it, is full of contradictions and of genuine, serious errors (even more in the last, more ambitious version than in the earlier ones, as Reeve himself has noticed, cf., e.g., Reeve 1986: 681n32). But I would like to explain what I meant to say subsequently, and to say it as briefly as possible in a more correct form. This will not save that text of mine from damnatio [being convicted]; but it might be useful for others (including, obviously, Reeve himself) who will wish to continue to work on the question of bipartite stemmas. As for me, I swear by the Styx that after these few pages I shall never work on it again, at least not as a general problem of methodology.

A first problem: In the critical editions of Classical and modern texts and in studies of manuscript traditions, does that overwhelming majority or even totality of bipartite stemmas that Bédier denounced really exist? In a first reaction to Bédier’s essay, Pasquale 1932a: 130–31 wrote that if Bédier had extended his investigation to the traditions of Classical texts, he would have found “umpteen” [von Hüle und Füller] tripartite, quadripartite, quintupartite stemmas. Reeve writes that in 1963 Timpanaro had declared that Pasquale’s assertion was exaggerated, but “now, under the influence of Giovann Battista Alberti’s book Problemi di critica testuale, published at Florence in 1979, he has convinced himself that multipartite stemmata are even rarer than he believed in 1963”; he [i.e., Timpanaro] has not noticed that Alberti’s principal purpose was to determine the number of (lost) archetypes that can be reconstructed mechanically, and hence that from his own viewpoint Alberti was justified in neglecting those cases in which the “archetype” (understood here by Reeve simply as the single ancestor of all the surviving manuscripts) is preserved (Reeve 1986: 59). Reeve has also explored traditions in which the ancestor is preserved, making use of a book on which he collaborated himself, Reynolds 1981. He has discovered that out of eighteen traditions in which a preserved ancestor has produced copies that are preserved as well, six have a multipartite stemma (mostly with three branches), while for the time being the other twelve cannot be reduced to a precise stemma. Reeve also writes that “Alberti ignores, and consequently Timpanaro ignores, a number of lost archetypes that gave rise to pluripartite stemmas” (Reeve 1986: 62: Cornelius Nepos [triptite stemma, according to Marshall 1977, an exemplary investigation], Valerius Maximus [a tripartite stemma, which Marshall brought to Reeve’s attention as well; cf. Reeve 1986: 61]; the Notitia dignitatum [six Renaissance copies made independently of one another from a lost medieval ancestor; cf. Reeve in Reynolds 1983: 253–57], and finally Cicero’s De optimo genere oratorum, in the tradition of which Reeve has recognized three independent groups, which might also be six (Reeve 1986: 61; cf. Reeve in Reynolds 1983: 100–102). Reeve expects that further research will uncover other stemmas with more than two branches; all the same, he admits (Reeve 1986: 61), “I still incline to agree with Timpanaro that Pasquale exaggerated” (in the phrase cited above).

Now, I was fully aware that because of his principal goal Alberti had limited his investigation to traditions with a lost ancestor. But since Alberti (on the basis of his own studies and other scholars’) has demonstrated that various traditions that I mentioned in 1963 as being multipartite are in fact bipartite, it seems to me that I had every right to make use of Alberti’s results in order to reach the conclusion that there are fewer multipartite stemmas than I had believed. Indeed, Alberti asserts what his theme is and repeats it in many passages too. Moreover, I took account (rightly, I believe) of the fact that Pasquale himself, before Alberti and Reeve and myself, had implicitly considered that that energetically anti-Bédierian statement of his was exagerrated. In fact, in Pasquale 1932a (1934) and in other writings of his later than Pasquale 1932a in which he polemizes against Bédier, the “umpteen” [von Hüle und Füller] multipartite stemmas of up to even five branches have vanished. I myself wrote this (p. 160), and perhaps Reeve should have recalled it. From my friend Alberti I have learned an enormous amount. Pasquale 1932a (1934) examines many traditions that can be reduced to stemmas: only very few indeed have more than two branches (check this. Besides, already in 1963, before Alberti, I had observed that tripartite stemmas have been transformed into bipartite ones. If Reynolds 1983 indicates the existence of multipartite traditions with a preserved stemma, I am very pleased, but I could not know this in 1980, nor had I received from Mar-
shall the information about Cornelius Nepos and Valerius Maximus which
Reeve has received (he careful about Marshall Gamberale 1975 disagrees
with Marshall about Aulus Gellius).

On the other hand, the fact I too am sure that multipartite stemmas can be
recognized in cases in which scholars hitherto believed they saw bipartite
stemmas is demonstrated by my continuing even in the last edition of my
text to issue warnings against the dangers of apparently bipartite stemmas
caused by erroneous classification on the part of philologists (definition of a
class of which really exists and the error of calling β everything which is
merely "non-α," pp. 173–76 genealogical classifications that may be a bad
inheritance from axiological classifications, p. 173). And so, if Reeve, who
cites three examples of bipartite stemmas in which conclusions deriving
solely from the examination of conjunctive errors are reinforced by histori-
cal and geographical data, declares that he is "curious" to know whether I
will refuse to agree that these are bipartite stemmas (Reeve 1986: 64), I can
answer that his curiosity is a bit curious. Not only have I never denied the
existence of bipartite stemmas, but I have always maintained that it is likely
that bipartite stemmas are the majority in what Maas called "poor traditions"
(Maas 1937: 293–94). On the other hand, what I considered unlikely was
that the majority was so strong, that what separated bipartite stemmas and tripartite ones was not a slight discrepancy but rather a very
steep "step," given that the latter two are typical of a poor tradition. And
this step continues to be very large even after the many examples of tri-
partite or multipartite stemmas Reeve mentions (note on the Humanist age),
and the same applies for decisions (Reeve 1986: 65–66), except for the
Castellani 1980 (1957). I find no answer to this observation in Reeve's article.
Weitzman 1982 considers the observation to be unfounded for statistical
reasons ("mathematical" ones, to use Reeve's terminology). But Reeve him-
self admits, together with Whitehead-Pickford 1951 (and with me), even if in
a form that is not entirely clear-cut (cf. also Weitzman 1985), that the purely
mathematical method does not resolve the question of the predominance of
bipartite stemmas, and that Weitzman has inserted into his computer "his-
torical information," some of which is open to question (Reeve 1986: 62).
So it is better, he says, to leave the computers aside and to deal directly with

With regard to historical and geographical observations, Reeve observes,
"Strangely for a pupil of Pasquali, Timpanaro says little about history and
even less about geography, which nowhere appears in his appendix" (Reeve
1986: 65). I recognize that this criticism is fundamentally justified, not only
with regard to the third appendix (and to my little volume on Lachmann as
a whole), but also with regard to all my activity as a Classical philologist.
I have concerned myself very little, too little, with manuscript tradition in the
empirical sense of cultural history that goes from Traube to Pasqualli and on
to codicology, a branch of scholarship that Pasqualli lived just long enough
to see institutionalized but that has made extraordinary progress since the
Second World War. What is more, I have concerned myself too little with
something else that is more basic, namely, with studies on individual manu-
script traditions performed with the "old" method as well. My philological
production consists almost entirely of adversaria, of textual and exegetical
discussions of individual passages. This is also because very often Classical
philology has become for me "a second matrix of study" in comparison with
the cultural and political history of the nineteenth century, with excursions
toward the eighteenth and twentieth centuries—but this obviously provides
no excuse for my ignorance. It is symptomatic that I have never prepared a
full critical edition of a text, and that even without the stimulus of a critical
edition I have concerned myself with genealogies of manuscript traditions
only two or three times. In comparison with me, Reeve has an incontro-
vertible superiority in this field, since it is precisely to this field that he has
dedicated what is perhaps the best part of his activity.

But it is not true that I have failed to recognize the importance of his-
torical and geographical investigations directed toward the genealogy of
manuscripts. I myself have indicated, and I believe that Reeve agrees, that
one essential reason for the inadequacy of the "mathematical criterion" re-
sides precisely in the great variety of historical and geographical condi-
tions (citations).

Let us go on to what Reeve calls a "methodological argument" (Reeve
1986: 64–65). I do not repudiate the phrase which I wrote to Reeve in a pri-
ate letter and I assure my friend Reeve that he has in no way "inflicted
scholarly courtesies" by citing it without asking my permission (cf. Reeve
1986: 64–65)—I believe that courtesy among scholars does not consist in
such formalities. But Reeve distorts my thought considerably when he writes
that "Timpanaro has now come close to saying that the application of stem-
matic method is in itself an editorial misjudgment" (Reeve 1986: 65)—when,
that is, he attributes to me the position of Dawe 1964 or Dawe 1973 (check
whether I have cited it) or a very similar position.

I maintain that [...] stemmas are always disturbed, even Lucretius. So I
maintain, in agreement with what various scholars have done in practice,
that in the choice of readings [we must evaluate every reading on its own
merits and not dismiss a reading too hastily as a mere conjecture] (cf. the ex-
a stemma and then says that two manuscript do not count against one.
Eliminatio legiones singularis is useful in the case in which there is no
substantial difference between variant readings; but like Waszink 1975:
23 = Waszink 1979: 87 I am convinced that such readings are less frequent
than is sometimes thought." I too in my article "Cf. . . ." [made the same argument] as Waszink. Many other times eliminatio lectionum singularium serves to eliminate what is clearly corrupt even without it (Lucretius).

Nonetheless, all these doubts do not signify a repudiation of stemmatists on my part (on "norm" and "exception," cf. Timpanaro 1985a). It is strange that Reeve denies (1986: 65) that nonetheless there are cases (many ones, as Pasquale already saw) in which contamination and interpolation have acted so extensively and so early as to make it impossible to trace out any stemma at all (those cases in which Pasquale, referring only to contamination, speaks of "total pretraditional contamination"; 1952a [1934] 146-53, 177-30). Would he be able to trace out a stemma of the manuscripts of the New Testament, of the Iliad, of the Odyssey, of the Metamorphoses, of many P Aristotelian texts, of the works of Virgil, to name only a few of the most well known and, I believe, indisputable cases? In any case, I believe that his statement is erroneous or ambiguous. Or else, if the stemma turns out to be so complicated by "dotted lines" (indicating contamination or interpolation) as to preclude any certain eliminatio lectionum singularium (cf. Ovid's Fasti, e.g., Pighi 1973: lxvii-xviii), does he believe that it is worth the trouble to dedicate years of work to tracing it out? Certainly, a stemmaticians with value only in terms of cultural history is legitimate as the schematization of the later reception of an author's work; but it will be necessary to recognize that in this case, even if its utility is hardly diminished, the scholars who believe that contamination and interpolation are so extensive as to preclude tracing stemmas "are biting the hand that fed them, because unless one can actually see scrbes contaminating and interpolating under one's nose it is only by applying stemmatic method that one can detect contamination and interpolation" (Reeve 1986: 65). Let us suppose with Maas 1958 (1957) that . . . . If Reeve calls "applying stemmatic method" this consecration of its failure too, then he is right but he is saying something that is sophistical and useless. If, on the other hand, he believes (as his citation of Irgoin 1977 at Reeve 1986: 65n8 would lead one to suppose) that it is only possible to recognize the "violation of the stemma" after the "vertical" tradition has been represented in a stemma and if he refers indifferently to contamination and interpolation, then he is mistaken, for he does not consider the aforementioned case, in which contamination and interpolation are revealed precisely by the impossibility of tracing any stemma at all. Irgoin's observation is correct in its first part ("[an editor] must try to discover . . . the constants of the 'vertical' tradition with all the means available to his scholarship and to his native talent"; Irgoin 1977: 243) as an admonition not to abandon the field too early, not to refuse to recognize that agreements, for example, in lacunae or in highly significant errors, should be attributed with high probability to the vertical tradition (note: this too with some reserves, see below). But it is no longer correct in its second part ("It is at this price, and at this price alone, that he will be able to determine the reality, and perhaps also the extent, of a horizontal transmission"; Irgoin 1977: 243), for the reasons that we have already indicated. On the other hand, Reeve himself, a little later, writes, "My general impression of contamination, and I believe Alberti's too, is that it has the effect not of falsifying stemmata but of frustrating attempts at drawing them up; and where it does falsify stemmata, as in Timpanaro's example, it falsifies them in a way that may mislead editors but will not alter the total of bipartite stemmata" (Reeve 1986: 67). So according to him too there exist cases in which contamination reveals itself not as a secondary fact after the stemma has been traced out, but as a fact that precludes any attempt to trace out the stemma. Since Reeve is a powerful reasoner and only with difficulty contradicts himself, there is quite probably something in his argument that I have not understood well, and I would be grateful to him if he would explain it to me.

Let us return to bipartite stemmas. Here the level of Reeve's polemic rises again after the observations that we have discussed just now, and that frankly seem to me erroneous; and I must recognize (though with reservations, which I shall explain) the incisiveness and correctness of his distinction between contamination and interpolation with regard to stemmas that are probably bipartite. Here I must recognize that I did not explain with sufficient clarity what I mean by extra-stemmatic contamination—or, to put it better, I had explained my meaning better the first time. Reeve distinguishes two cases: contamination "below the level of the archetype" and contamination in the archetype itself; in this latter case, on which I shall not linger further, for the sake of brevity, he admits that contamination can produce stemmas that are only apparently bipartite, but only in very rare cases (Reeve 1986: 67), and I agree with him (but at Reeve 1986: 67 n90 he would have done better to cite Irgoin 1977: 242-43 rather than Marichal 1961: 1284). But what I mean is outside of the archetypic; see Herman Franke's concept of Fremdlusung [foreign reading] (Franke 1964: 7802). I do not intend here to discuss once again the question of whether there was always an archetype. I am referring to the cases, which I believe are frequent, in which there was indeed an archetype; [some scholars speak of] éclcticisme des papyrius [see Dain 1975 (1949): 111], but in reality the eclecticism occurs lower down. In this case figure 2 at Reeve 1986: 66 does not need to be completed, and my not completing it myself was not intended as a "device" in order to conceal the fact that the stemma is really tripartite and to make it seem bipar- tite instead), but instead precisely because I believed that the dotted line [produces nothing other than] an extra-archetypal line. Here Reeve has been handicapped by only working on Latin texts, for which papyri are rare; but here too see [the case of] Sallust [Cat. 6.2, also transmitted by

Finally Reeve 1986: 68 [criticizes pp. 180–81 of the present work]. In the form in which it appears in the last edition of my little volume, the argument pertained to the pitfalls of the tendency to “verticalize.” My argument: (a) it is valid for successive bipartitions; (b) Appendix Vergiliana, Dictys Creten- sis [cf. Timpanaro 1965 = Timpanaro 1978a: 397–422]. [My reference to] “insignificant errors” merits Reeve’s contemptuous dismissal. It is true that I had expressed myself better in the 1971 edition, and I add that it is also true that even back then I did not express myself very well. Here too it would be useful, for my partial excusation, to go read the first formulation of that argument which is found in Timpanaro 1965. It is clear from there that what I wanted to warn against was not so much initial bipartitions of the stemma as rather “infinite bipartitions,” to use a jocular phrase. In my view, stemmas that have the following configuration are very suspect, and unfortunately very frequent:

Now, the multiplication of subarchetypes [as produced] very often, by the work of excellent philologists too, on the basis of variants of little signifi- cance or of none whatsoever [cf. Winterbottom 1979: 234–35]. (Sometimes one arrives at the archetype too; Reeve 1986: 60n165 cf. Watt, whom I then did not name; Dictys.) As Reeve notes, I never did write that article criticizing the stemmas of the Oxford edition of the Appendix Vergiliana, which I had once promised [in Timpanaro 1971: 148]. I did not write it because of the appearance in the meantime of Courtney 1968, which, even if it does not satisfy me in every detail, says in substance a large part of what I would have wished to say and other things that did not occur to me. But let me cite a single example: [.] [Dictys].

Toward the end of his article, before summarizing his objections, Reeve writes courteously, “The new version of Timpanaro’s Appendix C, even more than the old, should be read and digested by anyone who proposes to edit a text, because there is no better warning against the pitfalls that may occur in classifying manuscripts” (Reeve 1986: 69). I too believe that, for the present, a more detailed treatment of the pitfalls etc. is lacking; but that appendix has revealed itself to be too full of ambiguities and errors, and hence—after having responded to certain objections of Reeve’s that seemed
Differences among the Various Editions

This appendix lists textual differences among the various editions of Timpanaro's *The Genesis of Lachmann's Method.*

The basis for the present translation was provided by Timpanaro's own copy of the last edition printed during his lifetime:

*La genesi del metodo del Lachmann,* first corrected reprint with some additions (Padua: Liviana Editrice, 1985); indicated in the apparatus as 1985. The addenda to this edition have been added to the main body of this translation as footnotes signaled by one or more asterisks and are indicated in the apparatus as 1985 Addenda; Timpanaro's marginal annotations on this copy are integrated into the text and notes of this translation and are indicated in the apparatus as MS 1985.

Differences between this edition and the following ones are signaled in the text of the translation by superscript letters and are recorded in this apparatus:


*Die Entstehung der Lachmannschen Methode,* 2nd enlarged and revised edition, authorized translation from the Italian by Dieter Irner (Hamburg: Helmut Buske Verlag 1971); indicated in the apparatus as 1971. The postscriptum of this edition is indicated in the apparatus as 1971 Postscriptum.

*La genesi del metodo del Lachmann,* new edition, revised and enlarged (Padua: Liviana Editrice, 1981); indicated in the apparatus as 1981. Timpanaro's marginal annotations on this copy are indicated as MS 1981.

Only divergences in the body of the text, not in the footnotes, are recorded here: given Timpanaro's habit of scrupulously bringing his biblio-
In Appendix A ("Lachmann's First Attempt at a Mechanical Reconstr in 1817, SIFC 32, 1962: 52-56), Timpanaro at first adopted a suggestion of A. D. Lachmann and Eugenio Grassi and so corrected Lachmann's third stenographic rule of 1817 in the following way: "3. Where there are three readings, then $EM > B - G$ (the reading shared by $E$ and $M$ is preferable to) the two others in $B$ and $G$), $GE > B - M, GM > B - E$; on the other hand, $BG = E - M$ (the agreement of $B$ and $G$ leads to no secure decision against the two readings of $E$ and $M$), $BM = G - E, BE = G - M^*$ (p. 52). In later versions he preferred Fritz Bornmann's emendation and adopted the following text of Lachmann's rule: "3. Where there are three readings, then $BG < E - M$ (the reading shared by $B$ and $G$), the two others in $E$ and $M$ are preferable), $GE > BM, G - M > BE$; on the other hand, $EM = B - G$ (the agreement of $E$ and $M$ leads to no secure decision against the two readings of $B$ and $G$), $B - M = GE, B - E = GM^*$ (1962: 107-8; 1981: 107-8).

In Appendix B ("Determining the Script of Lost Manuscripts," SIFC 32, 1962: 57-63), Timpanaro at first wrote, "It is not so easy to believe that the homoeomerism of [Lucretius] 1.830 was healed conjecturally by the copyist of the Quadranus or of the subarchetype, given the rarity of the word and the very low cultural level of the medieval copyists of Lucretius" (p. 62). After E. J. Kenney brought to Timpanaro's attention the fact that the scribe could well have made the correction by comparison with 1.834, where the whole tradition has the correct reading homoeomerism, Timpanaro abandoned his skepticism (1962: 106-7; 1981: 116-17). Timpanaro notes his change of opinion at 1962: 97, 1981: 107, 981: 107-8. In 1962 this appendix concluded with the following sentence: "It is clear that the review of von der Hagen [i.e., Lachmann] had set the goal of reconstructing not $\phi^*$, but $\phi$ and $\omega$ (as would have been more natural), he would have ended up formulating a rule analogous to the one he will later devise for the New Testament (see part I, p. 208); the agreement of $B$ with one of the apographs of $\phi$ gives us with certainty the reading of $\phi$ and $\omega$" (p. 63). In its place a discussion of the practice of eliminatio codicum descriptorum is found in later versions (1962: 108-11; 1981: 119-21).

Preface to the Second Edition

a. N. N. On Pontederan see now Nardò 1981, others will follow. added 1983 Addenda
b. This edition ... new ideas to it. added 1983

Introduction

a. (this was ... authors) added 1981
b. divinatory talent is 1981, divinatory talent and sense of style are 1965, 1971

c. or a Bentley added 1981
d. at all added 1981

Chapter 1. Emendatio ope codicum from the Humanists to Bentley

a. evaluating ... exaggerated added 1981
b. first and second added 1981
c. corrupt 1981: interpolated 1962, 1971
\[\text{d. compositional structure 1981: compositional structure and style 1962, 1971}\]
e. sometimes ... believable added 1981
f. Later ... majority 1981: in certain cases, to be sure, one has the impression that this eliminatio codicum descriptorum is merely affirmed rather than being actually demonstrated. A manuscript of remarkable antiquity on one side, a mass of recensions on the other naturally suggested the idea that the latter were derived from the former: the hypothesis that both the one and the others were derived from a lost common model was less obvious. Thus too in the genealogy of the Indo-European languages, the hypothesis that first suggested itself as the most natural one was derivation from one of the historically attested languages, be it Greek or Celtic or German, or later Sanskrit; only at a later time, and not without dissent, did scholars come to postulate a lost mother-language. And yet Polianus was also capable of performing an eliminatio codicum descriptorum based upon solid evidence. 1963, 1971
g. He eliminated ... prove added 1981
h. venerates 1981: respects 1963, 1971
i. What is more ... occasionally. added 1981 (he had a feel ... apply it added 1981; Plate 1981; Varro 1971)
l. with clarity added 1981
m. the use added 1981
n. We must linger ... traditions. added 1981: Here the word archetypes does not yet denote the medieval ancestor of the manuscript tradition, as it does later in Madvig. Instead, as already in Cicero (ad Att. 16.3.1) and often in the Humanist period, it indicates the first official text, from which all the other copies are derived. This is the only way we can explain that limiting qualification, "so long as it has some specious appearance of the truth," which is illegitimate if one thinks of a medieval archetype (in which quite mechanical and crude errors could have been), but is justified in the case of an ancient "archetype," written under the supervision of the author and revised and corrected by himself or by someone he trusted in such a manuscript only insidious errors, ones with an appearance of truth, could remain undiscovered. In any case, Eranus enunciated in this way the concept of a lost ancestor of all the manuscripts that have reached us; this permitted him to explain the existence of errors common to the entire manuscript tradition. 1963, 1971
o. we shall ... Lachmann. 1981: this contrast can be noted already in Scaliger; we shall find it again in Bentley and also in Lachmann. 1963, 1971
p. N. N. On Scaliger, A. T. Graflex ... Jocelyn 1984; 60 added 1985 Addenda
q. From his ... collations added 1981
Chapter 2. The Need for a Systematic Receptio in the Eighteenth Century

a. N. "On the textus receptus ... no need. added 1985 Addenda"

b. just like ... isolated added 1981

c. so that in Italy that 1981: And in philologically marginal areas, for example in Italy, this 1981, 1972

d. but the very spirit of the Reformation encouraged 1981: but the very spirit of the Reformation, especially in the rationalistic developments of certain currents, such as Socinianism and Arminianism, encouraged 1963, 1972

e. He understood that those principles ... Methodism. added 1981

f. In comparison, added 1981

g. except ... Simon added 1981

h. We have named ... criticism. added 1981

i. But history follows ... task. 1982: Bentley was the first to plan an edition of the Greek New Testament based exclusively on the comparison of the oldest Greek manuscripts with the Latin Vulgata. He recognized that receptio had to take precedence over conjectural criticism in the case of a textual tradition that was so rich and ancient. But his project, though inspired by a thoroughly revolutionary concern to defend the authority of the biblical text against the destructive criticism of Anthony Collins, encountered the opposition of the theologians who, as we said above, identified the tradition with the receptus. And Bentley, also because he was committed to other projects, ended up giving up this plan, which, in this form, was to be fulfilled only by Lachmann, 1963, 1972

t. There were certainly more practicable ways ... questions. added 1982


l. — an infelicitous expression ... a better one added 1981

m. (which in fact ... immediately) added 1981

n. and other Englishmen added 1981

o. (accusations that he was aiming ... Amsterdam) added 1981

p. N. "On Wetstein ... particularly committed. added 1985 Addenda"

q. ended up adhering ... criteria. added 1987: ended up abandoning internal criteria and adhering to the erroneous criterion of the majority of manuscripts above. 1963, 1971

r. with such clarity 1981: with such great methodological clarity 1963, 1971

s. did not free himself from it courageously enough 1981: continued to take it as the basis for his own text 1963, 1971

t. N. "... In the 1982 edition ... unpublished documents. added 1985 Addenda"

u. of the Antioch orators and of Athenaeus like Libanius 1981: Of Demosthenes and of other Greek prose authors 1963, 1971

v. But Wolf's recognition of the need ... directly), added 1981

Chapter 3. The First Phase of Lachmann's Activity as a Textual Critic

a. not only one aspect ... century. 1981: one aspect of that antihistoricism which prevented him from understanding the new Classical philology of Wolf and Böckh. 1963, 1972

b. , sometimes ... caution, added 1981

c. and all ... circle," added 1981

d. and other Classical philologists who followed him added 1981

e. popular 1982: popular 1963, 1971

Addendum to Chapter 3

a. Addendum added 1981

Chapter 4. Lachmann as an Editor of the New Testament


c. times had changed and added 1981

d. included 1982: implied 1963, 1971


f. or no added 1981

g. (that is, to polygenesis of innovations) added 1981
Chapter 5. Contributions of Lachmann's Contemporaries

a. an intelligent follower of Pestalozzi's ideas 1981; full of the spirit of Pestalozzi 1965, 1972
c. It is this the stemma codicum in Schlyter, 1981: As far as we know hitherto, this is the first stemma codicum that was ever actually drawn up, and not only planned like Bengel's tabula genealogica, 1965, 1971
d. In the field... Goerres 1982: Certainly, already Johann August Goerres 1963, 1971
e. than Schlyter added 1981
f. and so far as we know added 1981

— from this point of view... later added 1981
h. But when the disarray... stemmat added 1981
j. the fourth one in time after those of Schlyter (Collins-Schlyter 1827), Zumpt (1833), and Ritschl (Ritschl 1833, 1981; the third one in time after those of Zumpt 1833) and Ritschl (Ritschl 1833, 1981, 1971
k. he considers this fact to be "strange" but nonetheless undeniable added 1981
l. some of the principles of recensio and emendatio 1982: the principles of the genealogy of manuscripts 1963, 1971
m. descriptorum... century 1983: descriptorum, a procedure that had already been practiced, as we saw, by Politian, but had been entirely neglected by Bekker and by Lachmann himself 1961, 1972
n. Later, Jean Boivin... stopgap added 1981

o. These eliminations follow one another... indicated added 1981
p. He also gave an excellent application, 1981: Saupe gave a splendid example of this procedure 1965, 1971
q. A little later... Criticism added 1985
r. Pasquale's work, 1985: Pasquale's work and perhaps did not even have the desire and the patience to read it 1982
s. and we shall see that with regard to Lucretius it is also true of Jacob Bernays added 1982

t. N. 5: Cf. n. 24 in chap. 2 added 1985

Addenda

Chapter 6. Studies on the Text of Lucretius

a. perhaps added 1981
b. (but today... probably) added 1981
c. usage. 1971, 1981; But when it was a matter of singing an easy poem in the poor Classical philologist, for example, against the wretched Forbiger, then any fear of burdening his readers with too frequent quotations vanished all at once from Lachmann's mind. 1965
d. proposed several shrewd conjectures but, added 1981
e. much added 1981
f. as is well known 1981; as we shall indicate 1965; as we shall see 1971

Chapter 7. What Really Belongs to Lachmann

a. even if in that same field... century added 1981
b. (much reduced... Lucretius) added 1981
c. an 1971, 1981: the 1965
d. very limited and uncertain 1981; almost nothing 1965: very small 1971
e. Schlyter (in a field different from Classical philology), added 1981
f. or late ancient added MS 1985

C. Boivin, Schweighäuser, added 1981

Homerum. 1981: Homerum. The only time Lachmann ever set forth a genealogical classification of manuscripts, in his preface to Lucretius, he did nothing more than follow in Bernays's footsteps, and did so, as we have seen, in a rather confused way. 1963, 1971

i. most genuine 1982: real original 1963; real individual 1971
j. and, nearer in time, to Madvig added 1981

K. It might be objected that we have committed a sophism... pages. 1981: The third and fourth points (genealogy of the manuscripts and mechanical reconstruction of the archetype) seem to us, and are in reality, united by a particularly strict connection; one cannot perform eliminato lectio, summation, unless one has first made oneself a stemma. And yet we have found that the two requirements were fulfilled for the first time by different scholars. Ritschl, who showed better than anyone else how
to disentangle a manuscript tradition genealogically, did not make use of this work for the constitution of the text (the same may be said of Zumpt and Bernays). Lachmann knew how to use genealogies traced out by others for the purposes of textual criticism; he possessed neither the patience nor, fundamentally, the aptitude for tracing them out himself. Among the founders of the new method, the most "complete" and balanced one was Madsen (even if he was not, perhaps, the most original one).

1963, 1972

I. as a textual critic added 1981

m. But there were also more serious and fundamental reasons for this development.

Whereas 1982: But it must also be borne in mind that, whereas 1963: But there are

also quite important and essential reasons for this development. Whereas 1972

n. —a limit, as we have said, but also a strength added 1981

o. (a requirement that other . . . effective) added 1981

p. and although he tended more toward a certain dogmatism than they did added 1981

Chapter 8. Textual Criticism and Linguistics, and Their Crisis at the End of the Nineteenth and in the Twentieth Century

a. not for a mere orientation of interdisciplinarity . . . itself added 1981

b. On the other hand . . . another added 1981

c. or as a banalization that constitutes a deterioration too, added MS 1985


e. (or better, of a somewhat earlier one: Jones, F. Schlegel) added MS 1985

f. These analogies authorize us to ask . . . criticism. 1982: These analogies might in

themselves make one think of a direct relation: one might hypothesize that compar

ative linguistics, which arose between the end of the eighteenth Century and the

beginning of the nineteenth, supplied Lachmann, or better still Ritschl, with a model for

textual criticism. 1963, 1971

g. However, this hypothesis does not withstand closer examination. 1982: However, I
do not believe in a direct connection of this sort. 1962, 1972

h. At first the inverse hypothesis . . . emphasis. 1987: The inverse hypothesis, that

Schleicher had been influenced by the model of textual criticism, would be more

probable from a chronological point of view, and yet this does not seem probable to me:
ether his gaze was directed at the natural sciences, not at Classical philology: as

is well known, he saw an abyss between philology and linguistics. 1963, 1972

i. But beyond this direct connection (which, I repeat, is probable, but still only hypo-

thetical) one might think 1982: At most one can think with a certain probability

1963, 1971

j. What is certain is that the similarity . . . on the other. 1982: But even if Indo-European

linguistics and "Lachmann's method" originally had no direct relation, at a certain

moment the adherents of the two disciplines became aware of the similarity in their

research methods. 1963, 1971


l. In Romance philology too . . . stagna. added 1982

m. and leave out of consideration certain cases of contamination among the Oblongus,

Quadratus, and Schedae as well added MS 1985

Appendix A. Lachmann's First Attempt at a Mechanical Recensio in 1817

a. The eminent Germanist Karl added 1981

b. an article dedicated to this problem 1982: a recent article 1963, 1972

c. (except . . . return) added 1981

d. Cf. now also Cocchi 1982: . . . this note. added 1985 Addenda (Bormann's article never

never appeared)
Appendix B. Determining the Script of Lost Manuscripts

a. [that is . . . reasons] added 1981
b. at least in many cases, added 1981
c. [the conclusions . . . one] added 1981
d. just as well (or better) added 1981
e. way too. 1981: way too, just as Scaliger did for Carullus. 1965, 1971
f. Giwan Bartista Alberi . . . texts. added 1971
g. novelty. 1981: novelty; many editors have already applied them correctly. 1965, 1971
h. well 1982: very well indeed 1965, 1971
i. even if a few other passages show some uncertainty added 1981
k. For the same reason . . . enough 1981: For the same reason, even if the examples of corruptions in the text of Carullus cited by Scaliger had been probative—and we have already seen that they were not—1965, 1971
l. quite 1981: rather 1965, 1971
m. much added 1981
n. Q and the Schedule . . . 190 added 1971
o. all 1982: absolutely mechanical 1965, 1971
p. Less certain . . . Lucerius, added 1982
q. 413 (mea simia for mea simia), 506 (parumque for parumque) added 1971
r. even if the confusion . . . letters. added 1981
s. few 1981: very few 1965, 1971
t. + Schedule added 1981
u. a pre-archetype in ancient minuscules . . . branches) 1981: an archetype in capitals (as Lachmann had supposed; a pre-archetype in ancient minuscules (which would have caused the minuscule corruptions shared by the whole tradition), preceded or else followed by another pre-archetype in capitals (which would have caused the capital corruptions peculiar to each of the two branches) 1965, 1971
v. 1981:

Appendix C. Bipartite Stemmas and Disturbances of the Manuscript Tradition

b. (though often with long intervals, as is only natural) added 1982
c. the extraordinary frequency of bipartite stemmas 1981: bipartite stemmas and their frequency 1965, 1971
e. more recently the field of Greek . . . controversy 1981: Hence it will not be useless if I provide a brief panoramas of the discussion, adding some observations on those points that in my view have hitherto been treated too hastily. 1965, 1971
f. the mechanical method 1981: the mechanical method encounetgated by Bengal and formulated with greater precision by Lachmann 1965, 1971
g. of individual subarchetypes 1981: of one subarchetype or another 1964, 1971
h. and that Madvig . . . (see chap. 3, n. 19) added 1981
i. encompassing 1981: var 1965, 1971
j. and various Romance philologists added 1981
k. at least at first added 1981
l. two 1971, 1981: three 1965
n. On the tradition of the lesser... the attention it deserves. added 1987 Addenda
o. Was this answer correct?... abandoned. added 1987
p. Mass tried... its explanation 1987: Maas, on the other hand, fully admitted the
tuth of the fact Bédier had pointed out, and not only for the Latin and Romance tra-
ditions but also for the Greek one; but he sought its explanation 1965, 1971
q. and more recently... shortly. added 1981
r. (an exceptional philologist... himself) added 1981
s. always continued to believe in the validity of that absurd argument 1987: has not
abandoned that argument even in the fourth edition of his Textkritik 1965, 1971
t. later as well, and added 1983
u. scholars who are excellent... word! 1981: some scholars 1965, 1971
v. in a rather detailed way, even at the cost of causing the reader some fatigue 1981: in
a slightly less hasty way than that of the critics just cited 1965, 1971
w. The destructiveness of Maas's reasoning... empirical data. added 1971 Postscriptum
x. seems to lead us onto a genuinely historical and empirical terrain, and hence a much
more concrete one 1981: leads us onto this terrain, a much more concrete one 1965, 1977
y. And yet this argument too... For example, Castellani has acutely observed that
1981: But enunciated in this way the argument still gives rise to serious doubts. To
be sure, the limited diffusion of culture in the Middle Ages may well make it seem
probable, in very general terms, that a considerable number of archetypes could have
produced a line of descent of only two copies, a smaller number of archetypes could
have produced three lines of descent, an even smaller number four lines of descent,
and so forth. But this would not explain so great a preponderance of bipartite tradi-
tions over tripertite ones as the one which was pointed out by Bédier and accepted
by Maas and most scholars. In this way it could certainly be understood why stema-
mas with very many branches were so rare, but stenomas with three branches ought
to not be so much rarer than those with two branches. What is certainly correct (and
we shall see this more clearly in what follows) is the reference to contamination; but
when Maas contrasts "little-read" texts (those with a tradition that is almost always
bipartite) to "much-read" ones (in which contamination makes a stemma impos-
sible), one wonders why there ought not to have existed numerous intermediate
cases, those of a tradition not so rich and entangled as to be irreducible to a stemma
but not so poor as to be limited to only two branches. A tripartite tradition is a little
bit richer than a bipartite one, and hence ought to have been produced fairly easily
even in the case of little-read texts. It is true that, as Castellani has acutely observed,
1965: Maas's reference to "little-read texts"... the number of tripertite ones added 1971 Postscriptum: Such a "jump"... in very different periods? added 1971 Post-
scriptum. 

z. Another hypothesis... with caution. 1981: At this point we ought to wonder
whether tripartite or multipartite stemmas really are as rare as Bédier, Maas, and
many other scholars maintain. With laudable precision and self-denial, Castellani
has reexamined those editions of medieval French texts from which Bédier had de-

d. 562 in the Carolingian period or even later added 1981
cc.但也，出人意料，保留恰反的读音，将被保存在最旧手
dd. — so long as we understand this expression... of which a single witness remains
ee. but they cannot be applied to all Latin texts... extended it too far in the other added 1981
With this hypothesis ... more frequent. 1981; But contamination and the copyists' conjectural activity constitute far more serious and frequent causes of "apparent bipartition." If these two phenomena occur to a very great extent, affecting the whole range of the manuscript tradition, the philologist must even abandon any form of genealogical reconstruction, and in that case one no longer speaks of stemmas, either bipartite or tripartite ones: it is in this extreme case that Maas is referring when he says that stematic rigor is lacking in the case of "much-read texts." But often contamination or the scribes' conjectural activity has the effect not of totally obscuring the manuscripts' genealogy but of falsifying it. Let there be given a tripartite tradition: A, B, C, derived independently from the archetype. If the copyist of A successfully corrects a certain number of errors of the archetype, or if a certain number of errors of B are transmitted by collation to C or vice versa, the tradition will take on a deceptively bipartite appearance: the shared errors of BC will be attributed by the textual critic to a subarchetype. Castellani has observed these two possibilities more clearly than his predecessors and observes that there are methods for detecting this kind of error of reconstruction, or at least for suspecting it and adopting in consequence a position of prudent skepticism. It will be enough to attribute the value of conjunctive errors only to those corruptions which cannot be healed by conjecture and which, on the other hand, constitute no evidence that they cannot be the object of a horizontal transmission; in fact, only errors with a semblance of truth (that is, for the most part, bimizations) are transmitted by collation, not obvious errors. These warnings, which are well known and are universally recommended in theoretical terms, are not always easy to apply in practice, because it is difficult to set precise limits to the conjectural skill of copyists and medieval philologists. Furthermore, the danger always remains that the copyist of A might have healed errors not by conjecture but by checking a manuscript of an entirely different tradition which later was lost. But those criteria have often been neglected even when applying them did not present particular difficulties. If we read through the prefaces of critical editions and studies of textual criticism written even by first-rate philologists, it is easy to run into genealogical groupings based only on a few coincidences in bimizations and even in correct readings! Hence we must suppose that a careful reexamination would lead to the conclusion that some presumed bipartite stemmas are more probably multipartite stemmas in which contamination has produced phenomena of "convergence" between certain branches or in which a certain number of corruptions that go back to the archetype have been corrected in one part of the tradition and have only remained in another part, thereby creating the impression that this whole part is derived from a shared subarchetype. In short, in many cases we shall have to go backward along the path followed until now by editors who, as Bédier observed (and his observation of the phenomenon was correct, even if the explanations he provided were unfounded), have gradually multiplied the subarchetypes until they reduced the principal branches of the tradition to only two. Certainly, in the majority of cases this laborious reexamination will not lead to establishing with certainty multipartite stemmas in place of bipartite ones, but rather to rendering our conclusions more cautious. We shall often remain uncertain whether particular agreements in innovation must be attributed to a shared model or to horizontal transmission. This is the element of truth contained in Fourquet's paradoxical statement that every multi-

partite stemma might be an unrecognized bipartite stemma, so that "a scrupulous editor will establish a bipartite stemma, or no stemma at all." Certainly, in purely theoretical terms, one can even do as Fourquet does and go to the extreme of suspecting that two branches of the tradition which are not connected by any conjunctive error go back no further to a shared subarchetype free of any peculiar corruptions of its own; but in practice this hidden subarchetype could be entirely neglected, precisely because it would have been a perfect reproduction of the archetype. In fact, quite serious corruptions are not lacking where the tradition is mostly mechanical (that is, where it is the work of ignorant copyists), and so the possible derivation of certain manuscripts from a subarchetype is not likely to pass unobserved; whereas the tradition is not mechanical, the most wondrous pinfall myths are posed not by the "hidden subarchetype" (and hence by traditions that appear to be bipartite but in reality are multipartite) but instead by contamination (and hence by traditions that appear to be bipartite but in reality are multipartite). In any case it should not be forgotten that Fourquet was starting out from the premise that multipartite stemmas had to be extremely rare for the calculation of the probabilities: the few stemmas with more than two branches that are found in critical editions seemed to him to still be too frequent, and so he suspected that they had arisen in error. We are convinced, on the contrary, that what are too frequent are the bipartite stemmas. But between the cases in which contamination has produced "unifications by convergence" of certain families, and the case in which it has made the genealogical relations entirely unrecognizable, there is another, intermediate case, a very frequent one, in which it has spread irregularly, creating numerous "anomalous constellations" without entirely canceling out every distinction among the families. So it is often possible to establish with sufficient certainty some genealogical groupings on the basis of lacunas or shared serious corruptions, but then there are also coincidences in innovations, in one place between α and β, in another between β and γ, somewhere else between α and γ, etc. In these cases, the correct method would be for the philologist to attribute all the irregular constellations to contamination (or, perhaps, to the copyists' conjectural activity). But the philologist, who legitimately desires that his stemmas serve to explain at least the maximum number of constellations of variants, if not all of them, is led as a last resort to attribute as many variants as he can to horizontal transmission alone. For this reason, if among the numerous irregular constellations it is above all the ones shared, for example, by α and β that strike him (either because they really are more numerous and important than the others, or because for whatever reason his attention has been drawn more to them), he will "regularize" these coincidences by making them go back to a subarchetype δ from which α and β would have derived, and only in the case of the other constellations (β-γ, α-γ) will he resign himself to admitting horizontal transmission. In this way a contaminated tripartite stemma will be transformed into a bipartite one. There is no doubt that if certain coincidences between two groups of manuscripts really are exceptionally numerous and, what matters more, exceptionally significant (coincidences in lacunas, notorious ditographs, etc.), one has every right to attribute them to the vertical transmission and not to contamination. But often the coincidences that are "verticalized" in preference to others are simply the ones that by pure chance first presented themselves to the philologist's eyes; let us not forget that scholars almost always begin to trace
stemmas on the basis of samples and not of complete collations; and once a stemma has been outlined—even if only as a simple working hypothesis—it is not easy to resist its appeal. Just now, while reexamining Werner Eisenbuth's valuable critical edition of Dictys Cretensis, I noticed that the editor had hypothesized the existence of a lost manuscript r, the shared model of the manuscripts P and V, on the basis of only a few insignificant innovations, ones much less numerous and significant than the ones that unite both of the manuscripts P and V to other manuscripts and that Eisenbuth rightly attributes to contamination. In this specific case what is involved is only a secondary branch, not the initial one: the stemma of Dictys Cretensis remains fundamentally bipartite, just as Eisenbuth has traced it. But in other cases this kind of classificatory error, which can happen quite easily psychologically, can have more serious consequences. And if in cases like this one many editors distinguish the vertical transmission from the horizontal one by indicating the former with continuous lines in their stemmas and the latter with dotted ones, it will be necessary here too to see whether this distinction has always been based in fact upon a clear difference in the number and significance of the coincidences in innovation. Whenever this difference does not exist, it is preferable to indicate various stemmas as equally possible, or to give up the schematization of the stemma and to consents oneself with indicating various affinities. 1962, 1971

(i) (we shall return to this point shortly) added 1982
(ii) Another 1981: Laidly, a final 1963, 1971

(ji) The mistake Mario Castella committed... 1971 added 1971

(jj) Schenkl and Castella as two victims 1971: Schenkl as a victim 1963

(l) not without remedy. To summarize: in all probability, manuscript traditions that are really bipartite are only slightly more numerous than those with three or four branches. If in our editions bipartite stemmas are present in only slight prevalence (in any case not in an overwhelming prevalence as other scholars have believed), this is due in part to objective causes that led to the fusion of branches of the manuscript tradition that originally were distinct (and among these causes contamination and the scribes' conjectural activity have the first place), and in part to errors of classification or to the refusal to investigate very intricate manuscript traditions more deeply. A careful reexamination of many bipartite stemmas will in some cases lead to replacing them with multiparticle stemmas, more often to recognizing that the genealogical relations among the manuscripts are far less linear than what has hitherto been supposed. Often the reexamination will have to go beyond the ramifications of the archetype and will end up putting into doubt the existence of the archetype itself—but not so that one dogmatism will have to be replaced by another dogmatism, the faith in the archetype by its a priori denial. This is the direction in which studies have already been moving for some time now, but much still remains to be done; and this is where the problem of bipartite and multiparticle stemmas will find its proper place. 1963 (Appendix C ends at this point in 1963) all that follows is added for the first time in 1981, with the following exceptions.

mm. Consecutive errors peculiar to a single branch of the tradition added MS 1981

nn. I have already expressed... in innovations 1971 Postscriptum
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