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 1817?

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.11

10. Lachmann 1831 (1846).
11. For an attempt to demonstrate the correctness of Lachmann’s rules without having recourse to corrections of this sort, cf. Lutz-Hensel 1973 and 1975: 228–39. 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 nebulousness and capriciousness. Cf. now also Cecchini 1982, 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.4

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. 51–52, 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 e 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 e–g might also provide an indication of a model in uncial, 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 apographa of a lost manuscript α presents errors of its own due to misunderstanding of a given script, then that was the script of manuscript α; (2) if the apographa of α present shared errors due to misunderstanding of a given script, then that was the script not of α but of the model from which α was directly or indi-
rectly copied—for in that case the errors shared by the apographs were already found in α, and hence they were due to misunderstanding the script of a preceding manuscript.

Neither of these criteria is immune to objections. Against the former it can be noted that an error already found in α might have been reproduced by a copyist A and corrected conjecturally by a copyist B in this case we would be attributing to α the script that was instead that of α’s model. Against the latter it can be noted that various copyists might have misunderstood the same graphic sign of the model α independently of one another (for example, in pre-Caroline minuscule they might all have mistaken for a u an “open a,” since this letter was particularly subject to being misread in that script); in this case we would be attributing erroneously to the model of α the script which was instead that of α.

If the examples on which we base our conclusions are few, these objections doubtless have a considerable weight; but in the face of a large number of examples, it becomes quite improbable that various copyists might independently have made a mistake every time at the same point, or that now one copyist, now another, might have made a felicitous conjecture, especially if there are not only two apographs of α but three or more.

Giovanni Battista Alberti has called my attention to another possible objection against the former criterion: mistakes due to misunderstanding a given script which are found in the individual apographs of α might include the script not of α but of lost apographs intermediate between α and each of the extant manuscripts. This objection is valid above all when our data are few or contradictory, as in the case of Lucretius (see below). But if the graphical errors in the individual apographs A, B, etc. are numerous and they all derive from misunderstanding minuscule letters, then it is highly probable that they reflect the script of α and not of hypothetical intermediate links, since it would seem strange if no trace of misunderstanding of the script of α had survived. Furthermore, if the graphical errors of the individual manuscripts are all derived from misunderstanding majuscules, it is even more improbable that the common model α was not in majuscules: otherwise we would have to suppose a passage from an ancestor in minuscules to various descendants in majuscules—something not impossible, to be sure (especially now that we know that as a rule the first ancient copies of our Latin classics were not written in capitals: see below, p. 152 and n. 161), but difficult to postulate when all the manuscripts descend from a medieval archetype, and virtually impossible for Greek texts.

The two criteria enunciated above are not to be found formulated explicitly in the manuals of textual criticism or palaeography I have seen, but they do not represent a real novelty.3 Giorgio Pasquale knew them well,4 as some passages in his Storia della tradizione make clear, even if a few other passages show some uncertainty.1 And yet especially the second criterion has often been neglected. For example, W. V. Clausen quoted a large number of corruptions common to the two manuscripts of Persius A (Montepessulanum 212) and B [Var. tabularii basilicæ 116] in his edition1 of that poet in order to demonstrate that they were both derived from a model in minuscules (Clausen 1956: viii–ix). But no: those corruptions demonstrate that what was in minuscule was the manuscript from which the model of AB was copied. Certainly, this makes it likely that the model of AB too was in minuscule, even without going in search of minuscule corruptions characteristic of A alone and of B alone—but only as a secondary inference.2

For the same reason, even if the examples of corruptions of scriptura Langobardica (in the sense of pre-Caroline minuscule) that Scaliger collected in the whole tradition of Catullus he knew of had been numerous enough and had all been certain enough,5–1 they would have served to dem-

1. Pasquale 1952a [1954]: 149 and esp. 194–95. This question has also been well treated by Fabre 1947 [1955]: lvi–lvi; Andrieu 1954: 49; Hübner; Pfeiffer 1949–51: li–xxxiv; K. Müller 1954: 785–97. See also the beginning of the following note. We shall discuss Lachmann and Duvéa shortly.

2. Later, Clausen himself made this point quite well: Clausen 1965: 255. On the ancestor of the Palantine manuscripts of Plautus, see also O. Seyffert’s correct objection (Seyffert 1954: 151) against Lindsay, quoted and confirmed by Questa 1963: 128. In his edition of Caesar’s Bellum civile, Klotz (1955: 2) erroneously cites examples of errors due to misunderstanding of abbreviations in order to demonstrate that “the men who made the archetype of the manuscripts used many abbreviations, some of them rare,” even though in the meantime the problem had been discussed in the right terms by Fabre (1947 [1955]: lvi–lvi): since those errors are found in all our manuscripts, the abbreviations that gave rise to them must have been found in a pre-archetype, not in the archetype. An analogous mistake is to be found in Klotz’s prefixed to Statius’s Thesbaida, a prefixed that, in other regards, remains fundamental even today: he cites many examples of errors of minuscules (insular and perhaps, more generally, pre-Caroline ones) in individual manuscripts of the u class and concludes on this basis that between them and their ancestor there were intermediate exemplars in insular script (Klotz 1958: lvi–lxi). In theory this is possible (see Alberti’s observations cited above, p. 146); but precisely the large number of examples—which, as Klotz himself says, represent only a selection—provides a sufficient demonstration that already u was written in insular script or in pre-Caroline minuscule: the “intermediaries” are neither a necessary nor a demonstrable hypothesis, one suggested, I suspect, by the preconception that the archetype necessarily had to be in capitals. The minuscule errors shared by all the descendants of u which Klotz cites at 1960: 1x serve to demonstrate that not u but one of its ancestors was written in minuscules as well.

3. Grafton 1972: 171838 cites from Scaliger’s Castigationes five examples of the confusion u-u, three of i-t, seven of c-t, one of c-g. But the confusion i-t and c-g can oc-
onstrate that a manuscript earlier than the archetype was in pre-Caroline minuscule, but not the archetype itself.4

Considerable confusion reigns on this subject in many editions of Lucretius. To read the prefices, one would think that Lachmann relied on corruptions shared by the whole tradition in order to maintain that the archetype was written in rustic capitals, and that Louis Duvau later demonstrated on the same basis that the archetype was in insular minuscule, copied in its turn from an archetype in capitals.5 If this were correct, then both Lachmann and Duvau would have fallen victim to the same error that we have just now noted in Scaliger and in some recent Classical scholars. But in fact Lachmann limits himself in his preface to stating, “Many indications prove that the script of that manuscript [i.e. the archetype] was in rather thin capital letters, not in uncials” (1850a: 1.3); in the commentary, unless I am mistaken, he notes explicitly only one of these indications, the reading homofoemian of the first hand of the Oblongus at 1.850, where the Quadratus and the Schedae have the correct reading homoeofoemian.6 He would certainly have done well to cite other examples to confirm his thesis, for that single case is not enough, as we shall soon see more clearly. But it cannot be said that in itself it was chosen poorly: Lachmann deduced the script of the archetype not from a corruption shared by the whole tradition but by a corruption in a single apograph, in conformity with the former of the two criteria we enunciated earlier. Nor can there be any doubt that the confusion between e and f presupposes a model in capital letters.

In an article of exemplary clarity and rigor,7 Louis Duvau collected many
car with other scripts too, and among the other confusions only very few indeed are pro-
itive: most are connected to completely fanciful conjectures on the part of Scaliger and collapse together with those conjectures.

4. There is also the possibility that some minuscule errors in Carullius’s text, as in other authors, go back to a much earlier phase, that of “ancient minuscule”: cf. Brunnholz 1871: 21–22, and below, pp. 151–53 and mm. 16, 18.


6. Lachmann 1850b: 1.57 on 1.850: “The first hand of the Oblongus does not have homofoemian, as Havercourt reports, but homoeofoemian, from which we can tell what kind of script the archetype was written in.”

7. Duvau 1888.

certain examples of corruptions due to misunderstanding of minuscule script;8 but in fact, despite the claims of those who cite him without having read him with a minimum of attention, he derived from them the inference not that the archetype was in minuscule script, but rather that what was in minuscule was the pre-archetype: “If all our manuscripts present shared cor-

rors deriving from a resemblance between certain letters that exists in minuscule script, and exists only in this script, then it follows: (1) that since these errors are shared by all our manuscripts, they were found in their ar-

chetype; (2) that the origin of these errors, that is, the minuscule script, was found in the manuscript to which this archetype goes back directly or indirectly.” (Duvau 1888: 34). His judgment regarding corruptions due to the misunderstanding of abbreviations was just as precise: “Since they are found in all our manuscripts, they were made at the latest by the copyist of their archetype: hence their cause, that is, the use of abbreviations, was found in a manuscript earlier than this archetype itself” (1888: 33).

According to him, the archetype too was in minuscule script.9 He did not give a positive demonstration of this claim; the fact that the pre-archetype was in minuscule script must have seemed sufficient to him to demonstrate that the same thing was true a fortiori for the archetype. Against the Lach-}

mannian hypothesis of an archetype in capitals he observed: “I seek in vain for the reasons Lachmann thought he was obliged to believe that the original of the Oblongus was in capital script. The fact that no doubt led him to conceive this idea, the frequent confusion of letters that resemble one another only in capital script (for example, i, e, l, l), seems to me to prove exactly the opposite. For these errors are not committed separately by each of the copyists of the Oblongus, the Quadratus, and the Schedae; the same words are altered in the same way in all our manuscripts [ . . . ]. The conclusion is obvious: these errors existed in the archetype, either because the copyist of this manuscript introduced them himself when he copied an original in capitals or — and this is the hypothesis that must be accepted [ . . . ] — because they already existed in the manuscript that he was copying” (Du-

vau 1888: 34). Hence this was an archetype in minuscules, copied from a manuscript in minuscules too, that in its turn was derived directly or indirectly from a manuscript in capitals.

Now, there is no doubt that Duvau was perfectly correct in explaining


9. Duvau 1888: 33–34: “I believe that not only this direct original of the Oblongus [i.e., the archetype] was in minuscule script, but so too was the lost original of this manuscript.” Pursmann (1846: 34) had thought of an archetype or pre-archetype in scriptum Langobardica (i.e., Beneventan) or rather here too pre-Caroline minuscule, as in the pas-
sages from Scaliger we cited above, at chap. 1, n. 207, but without adding any proofs.
the capital errors shared by the whole tradition as errors inherited from a very ancient phase. But he was mistaken in supposing that Lachmann relied on these errors for his hypothesis concerning the archetype: we have already seen that Lachmann relied on a corruption in the Obelosus alone. Evidently Duvau did not notice this passage in the commentary to 1.830, and so he attributed to Lachmann the very same error of method that the editors of Lucretius would later attribute just as unfairly to himself.

In fact, although there can be no doubt concerning the existence of one pre-archetype in capitals and another one in minuscules, as we have seen just now, the indications regarding the script of the archetype are not unambiguous. And, as far as I have been able to determine, this is a situation that occurs quite frequently: it is much easier to determine the script of manuscripts earlier than the archetype than the script of the archetype itself.

In support of an archetype in capitals, there are other indications besides the homosomerician of the Obelosus at 1.830 noted by Lachmann (to which we shall return shortly): only O has sam for tame at 2.1088 and 5.902, versus for ventorum at 5.1230; Q and the Schedae (but not O) have ant-facta for anteacta at 1.233 and tinguette for tingyette at 3.90; only Q has eacies for facies at 4.733, forum for eorum at 5.337—all corruptions that do not seem to be explicable otherwise than by a model in capitals. Less certain is rectit for rectit in O at 1.34: this could be a semiconscious banalization, since rectit seems to yield good sense, and the erroneous prosody re could have been influenced by apparently analogous cases like religio (aside from the fact that unmetrical readings are anything but rare in the manuscripts of Lucretius).10

Important indications in favor of an archetype in pre-Canonic minuscule are 1.282, where Q and the Schedae have augett for auget (and hence so too did the subarchetype from which they descend), 413 (meos aetanis for meo susanis), 506 (paranque for peranque)—all confusions between u and open a.11 So too, partis for partes in O at 5.554 is quite probably an error of minuscules, certainly not one of capitals—if anything, then of uncials, even if the confusion is not between words of generally similar phonetic and graphic appearance but instead between individual letters.1 I would rely

9. In these last two passages the testimony of the Schedæ is lacking; but nonetheless these cannot possibly be errors committed by the copyist of the Quadratus in the course of transcribing the archetype, since the subarchetype was certainly in pre-Canonic minuscule (see below, n. 11). So these must be errors committed by the copyist of the subarchetype in the course of transcribing the archetype.

10. David A. West has drawn my attention to some of these errors of minuscule. At 1.830 the second hand of the Obelosus has augett, preferred by Wolter and Diels. Most editors prefer augett, rightly, I believe; in any case there is no doubt that this was the reading of the archetype.

with less confidence on 2.683 (noscat for noscas in Q and the Schedae) and 2.839 (remota for remota in O): the first case may be a substitution of the more common third person for the second, while remota may be the result of attraction from the preceding somita.12 In the parts of the poem where the Schedæ are lacking, the Quadratus presents some evident errors of pre-Canonic minuscule: 5.374 aequiunt fundis for aequorius undis, 482 gurgites ossas for gurgite fossas, 11.47 lusa for tara (the Obelosus has lara); but since the subarchetype was certainly in pre-Canonic minuscule,13 these errors may be peculiar to the Quadratus, so that we cannot make any inferences from them concerning the script of the archetype.

Thus the indications we have are few,14 and, what is worse, contradictory. Anyone who wishes to keep believing in the hypothesis of an archetype in minuscules will have to suppose that the capital corrections of the Obelosus alone, or of the Quadratus + Schedæ15 alone, were already found in the archetype and were corrected conjuncturally by the copyist of one of the two apographs. This is certainly possible for banal errors like eacies for facies; and even the sole error that, taken in isolation, might seem difficult to correct conjuncturally, that is, the homosomerician at 1.830 upon which Lachmann insisted, could have been heaved by the copyist of the Quadratus (or of the subarchetype from which the Quadratus and the Schedæ descend) by comparison with 1.834, where the whole tradition has the correct reading homoeomerician (as has been correctly pointed out to me by E. J. Kenney).16

On the other hand, anyone who wanted to exclude the hypothesis of con-
jectural interventions in both branches of the tradition would have to take recourse to a more complicated stemma: a pre-archetype in ancient minuscules (which would have caused the minuscule corruptions shared by the whole tradition), followed by another pre-archetype in capitals (which would have caused the capital corruptions shared by the whole tradition) and by the archetype likewise in capitals ("Lachmann's archetype," which would have caused the capital corruptions peculiar to each of the two branches); an intermediate apograph in pre-Caroline minuscule (which would have caused the minuscule corruptions peculiar to each of the two branches) between the archetype and the Oblongus, and also between the archetype and the model of the Quadratus and the Schedae:

\[ \begin{array}{c}
\omega \\
X \\
\Psi \\
U \\
\Phi \\
\tau \\
\end{array} \]

Oblongus

\[ \begin{array}{c}
\phi \\
\end{array} \]

Quadratus Schedae

The phase in "ancient minuscule" (ω) would not constitute any difficulty at all. One need only think of the studies of Jean Mallon and Robert Marichal and their followers, who have demonstrated the early diffusion of this kind of script, and also of the fact that the Virgil manuscripts in capitals of the fourth and fifth centuries present many corruptions that can be explained only in terms of misunderstanding of minuscule letters.16 This is

16. Out of Ribbeck’s long and not entirely certain list (1866: 235-38) I select some particularly significant examples, which I have checked with the apparatus of M. Geymonat’s edition of Virgil (Geymonat 1972): the first hand of the Medicus has morte (Georg. 5.318), orvis for Orion (Aen. 4.52), amnus for Annun (4.61), ta for tu (1.384); Fulvio Orsini’s Schedae Vaticanae have flactur for fluctus (Aen. 3.605); the Palatinius has secundat for secundat (Georg. 4.291), invenias for invenias (Aen. 5.777), cuprum for cuprum (τ. 2.1: 5, Aen. 1.621, 2.116), orcium for saliscus (1.645), nune for nina (5.451), barnus for Sarmus (7.278), alta for alta (10.197). Ribbeck attributed these errors to cursive minuscule (he was thinking of “Pompeian” script, as was only natural at the time), but there is no doubt that these are errors of minuscule. Cf. also the following example, which I ran

what I wrote already in 1960 (Timpanaro 1960: 62 and n. 1). Since then, there have been great steps forward in this field. On the one hand, scholars have studied in greater depth the paleographical and cultural aspects of the “common script” in antiquity and of its various later (or sometimes contemporary) canonizations.17 On the other hand, especially Franz Brunhölzl’s examination of the minuscule corruptions already present in the most ancient surviving manuscripts in capitals (or in the manuscripts considered to be direct descendants of such manuscripts which have been lost), performed in conjunction with the examination of the most ancient Latin papyri, has led to the conclusion that the most ancient literary manuscripts of the Republican and Imperial ages did not resemble the Palatinius or Medicus manuscripts of Virgil (Vatican, Pal. lat. 1651; Florence, Laur. 39. i + Vatican lat. 3225, fol. 76) but were volumina of semicursive script, minuscule or rich in minuscule aspects.18

Even if these recent studies seem capable of confirming the stemma I have proposed now, all in all the first hypothesis (an archetype in minuscules) continues to seem preferable to me, now that Kenney has overcome what had seemed to me the only serious obstacle, as I indicated above. Of course this does not in the least exclude the possibility that the corruptions due to ancient minuscule might in part go back to copies much older than the archetypal...
Naturally this whole discussion can be neglected by an editor of Lucretius, since the only thing that matters for the practical purposes of constituting the text is to know that there are errors both of capitals and of minuscules in the tradition of Lucretius. It can be neglected; but it should not be explained in a confused and erroneous manner, as almost all the editors have done hitherto.  

* * *

Finally, there is one field of textual criticism in which the examination of graphic corruptions can render a useful service: eliminatio codicium descriptorum. As is well known, this operation is particularly difficult when a manuscript presents not only a large number of corruptions in common with another older manuscript, but also a number of certainly or probably correct readings in passages in which the older manuscript is corrupt. This raises the following problem: can these readings be the fruit of conjecture, or are they such that they could not have been excogitated conjecturally in any way whatsoever? In the latter case, the more recent manuscript is a brother of the older one, not its son, and therefore must not be eliminated; in the former case (which unfortunately occurs quite frequently), the more recent manuscript can be a copy of the older one, but there is no certainty that this is how matters really stand. Hence it is a great piece of luck to be able to find positive proofs that one manuscript is derived from another, consisting in material damage to the older manuscript (displacement of leaves or fascicles, “windows,” etc.) to which transpositions or lacunas in the more recent manuscript correspond. But it is just as useful to find proofs of non-derivation, which let us assert with certainty that the more recent manuscript is not a copy of another sur-

ened by the fact that the author is too sure that he can explain as being due to graphic confusions errors that could be psychological; (1) when late ancient manuscripts in capitals are not preserved but are reconstructed from Caroline copies, one must never forget the alternative hypothesis of errors of pre-Caroline minuscule: cf. Timpanaro 1970: 288. In the case of Lucretius this hypothesis becomes a certainty when what is involved are the errors of the common model of the Quatras and the Scholae, cf. above, n. 15.  

20. In his edition of Lucretius, K. Müller provides a far better stemma than those of earlier editions, even if it too is not free of “dubious passages” (upon which I shall not insist, since my doubts can easily be inferred from what I have said until now): K. Müller 1975: 497–500. I am disagreeing, of course, the problem of the “italici,” to which I have already referred (above, pp. 108–9, 111–12).  


Determining the Script of Lost Manuscripts

viving manuscript and that in consequence the correct readings that it contains may represent tradition and not conjecture. Now, one good proof of non-derivation can be provided by certain graphic substitutions. If, for example, a manuscript that is suspected of being derived from another surviving manuscript in Caroline script presents a certain number of corruptions typical of pre-Caroline minuscule, then we can conclude with certainty that that suspicion was groundless and that this manuscript must not be eliminated. Pasquali warns quite correctly against hasty eliminations based on “graphic signs that lend themselves to being confused with one another” and observes, “When someone maintains that A is derived from B because A has misread a case ending in β which was expressed by an abbreviation ‘by suspension’ that can be resolved in more ways than one, or by a letter drawn so poorly that it can be confused with another one, then it is legitimate to observe that any manuscript of the same scriptorium, or even of more or less the same period as B, used or could have used the same abbreviation, which could easily be confused with another one every time it was not written impeccably” (1932a [1934]: 35). But although these corruptions due to misunderstanding of graphic signs count little or nothing for the purposes of the elimination of a manuscript, they can be decisive for the purposes of its nonelimination. Whereas five or six certain confusions between s and r do not at all serve to demonstrate that a manuscript A was copied from an extant manuscript in insular script, inasmuch as any other insular model would have lent itself to analogous misunderstandings, they serve perfectly well to demonstrate instead that A was not derived from a manuscript C in Caroline script, even if by chance the two manuscripts present many coincidences in corruption. Hence Maas’s formulation, “If a witness, J, exhibits all the errors of another surviving witness, F, and in addition at least one error of its own (peculiar error), then J must be assumed to derive from F” (1958 [1927]: p. 4, sec. 8 [a]), is not only open to the more general objections that can be raised against such a rule; what is more, one must in any case specify: (and in

22. What is more, this rule is presented by Maas himself as a deduction from a presupposition that need not correspond to reality: cf. Maas 1958 [1927]: pp. 8–9, sec. 11; and Pasquali 1932a [1934]: 303f. (but Pasquali displays a certain reluctance to discuss directly with Maas and minimizes the difference between Maas’s position and his own analysis of the problem of eliminatio descriptorum). In any case, a manuscript that had the characteristics of J, even if it had not been copied from F, would have to be eliminated anyway because it would be completely useless for constituting the text. The interesting (and difficult) case is not this one, which Maas considers “typical,” but another one, in which J presents good readings that are not found in F and that can (but need not) be the fruit of conjecture; and in this case Maas always inclines toward elimination, not in his theorist-
addition at least one error of its own that does not presuppose a script (or even a context) different from that of F.”

In fact, Pasquali already indicated a specification of this sort when he wrote, “When, after collating a more recent manuscript in its entirety with an older one, one has found no probative indications of dependence, but neither has one discovered better readings or individual divergences, and not even corruptions that cannot derive from the older manuscript but raise the suspicion of a tradition that may be extremely disfigured but is still different—in that case, and in that case alone, one can be satisfied with the 'presumption' that the more recent MS is a copy of the older one” (1934: 35, our emphasis). All the same, as is clear from the words “but raise the suspicion of a tradition that may be extremely disfigured but is still different,” Pasquali was not thinking so much of graphic errors that can be explained by one determinate script and not by another, but rather of larger corruptions such as presuppose at their origin a different reading from that of the presumed model. And this too is a possible case; but I believe that the examination of purely graphic corruptions as well will be able to rescue from elimination manuscripts that have been unjustly suspected hitherto, if care is taken to collect a considerable number of certain examples.

cal pronouncements, but in his scanty exemplification: cf. 1948 (1927) pp. 26–27, sec. 27;
cf. Maas 1960: 32. But I intend to return elsewhere to the problem of the descripts.

In the last fifty years (though often with long intervals, as is only natural), the scholarly discussion regarding the extraordinary frequency of bipartite stemmas has given rise to the most varied positions and to results of considerable methodological interest. Until about twenty years ago, Classical philologists often displayed no knowledge of the contributions of their colleagues in Romance languages (sometimes the opposite occurred too, but less often); more recently the field of Greek and Latin studies too has witnessed a reawakening of interest in this problem, which is less marginal and less strictly technical than might be supposed. The first version of this appendix, in the 1963 edition of the present volume, may perhaps have made some contribution to this state of affairs. The second version, which I present here, does not differ substantially from the first one, but without aspiring to what would necessarily be a confusing bibliographic completeness it does take account of those works that have appeared since then, some of which are very important; and it aims to draw attention more decisively to what, in my opinion, has always been the central point of the whole controversy.

In his brilliant and paradoxical article on the manuscript tradition of the Lai de l'Ombre, which we have already had occasion to cite more than once, Joseph Bédier observed that in the overwhelming majority of cases the manuscript stemmas traced out by the editors of medieval texts had only two branches; even if the extant manuscripts were very numerous, they were almost always made to derive not directly from the archetype, but through two and only two subarchetypes; and even when preparatory works on the

1. Besides the studies I shall cite in the course of the following pages, see the copious bibliographical indications in Frank 1957: 463n1 (brought to my attention by Alfredo Stanci). Cf. also Baldusino 1979: 237–43.
2. See above, pp. 46, 80, n12. Bédier had already presented the same ideas in his preface to Renart 1953: xxv–xxvi, but less fully and with less polemical force.
manuscripts of a given text had hypothesized the existence of three or more families, the scholar who finally set about to do the critical edition had reduced the number of families to two.

So, Bédier observed, it would appear that by a very singular chance almost all archetypes had possessed a direct progeny consisting of only two apographs, or, at least, that it was from only two apographs that all the extant copies had been derived. "The flora of philology knows only trees of a single kind: the trunk always divides into two dominant branches, and only into two. . . . A bifid tree is not at all strange, but a clump of bifid trees, a grove, a forest? Sikea portentosa [a wondrous forest]." Bédier observed an analogous frequency of bipartite stemmas in the manuscript traditions of Classical texts as well, though he admitted that his research had been less extensive and profound in this field than for French medieval texts (1928: 171-72).

Bédier identified the cause of this strange phenomenon not in the objective conditions by which medieval manuscripts were produced but in the philologists' unconscious desire to maintain their freedom of choice when choosing variants. In fact, if the manuscript tradition has three or more branches, the reading of the archetype can almost always be established by a mechanical procedure, as long as the eventuality of contamination is excluded (see above, p. 109); but if it has only two branches, the mechanical method can only serve to eliminate those innovations that have been produced in descendants of individual subarchetypes, whereas the decision must be entrusted to internal criteria whenever the one subarchetype's reading is opposed to the other's.

According to Bédier, Lachmann's method, which had been elaborated precisely so as to expel subjective judgment from textual criticism, had been applied by philologists in such a way as to preserve the widest possible field of application for subjective judgment. If Bédier had noticed that Lachmann himself had unconsciously slipped from a tripartite classification of the manuscripts to a bipartite one in his preface to Lucretius (see above, pp. 108-11) and that Madvig had consciously changed his mind in an analogous way with regard to a group of Cicero's orations (see chap. 5, n. 19), he would have seen in these facts the best possible confirmation of his thesis!

But following a suggestion of Mario Roques, Bédier also indicated another cause, this one subjective too, for the philologists' tendency to bipartition, besides their unconfessed desire to preserve freedom of choice: their habit of always seeking new connections between groups of manuscripts, and thus of assisting to more and more encompassing groupings, until they have reduced the fundamental regroupings to only two. According to Bédier, the Lachmannian textual critic feels "the persistent anxiety that, however far he has extended the criticism of variants, he has still not ex- tended it far enough." And so, if a first phase of his research has led him to establish the existence of three families of manuscripts, that "anxiety" induces him to look for readings that unite two families against the third one, to convince himself that such readings are erroneous and hence to make these conjunctive errors go back to a shared subarchetype, from which the ancestors of the two families would have descended (instead of directly from the archetype). "It is not with impunity," Bédier concludes with ironic emphasis, "that he has accustomed himself to oppose the good reading to the bad one or ones, the rays of light to the darkness, Ormuzd to AhIran: once the dichotomic force has been aroused, it continues to act to the very end" (Bédier 1928: 176).

From this denunciation of Lachmannism, Bédier derived an exhortation to abandon any attempt at recensio and to adhere instead to a single manuscript. The illogicity of this exhortation has already been demonstrated too clearly for it still to be necessary for us to linger on this subject. What interests us here, and what constitutes the only really interesting and acute part of Bédier's article, is the question of the frequency of bipartite stemmas.

On this point, two very different answers were given to Bédier, by Giorgio Pasquali on the one hand, and by Paul Maas and various Romance philologists on the other. Pasquali, at least at first, denied for Latin and Greek texts the datum from which Bédier had started out, namely, the extraordinary rarity of stemmas with more than two branches: "I would like to ask Bédier to extend his inquiry to Classical texts; there he would find untrumpet three-, four-, five-branched stemmas." 4

Was this answer correct? To a certain extent it was, but to an entirely inadequate extent. There is no doubt that Bédier exaggerated the extreme rarity of stemmas with more than two branches when he performed his examination of the editions of medieval French texts: Arrigo Castellani reëdits that examination with great accuracy and precision and reached less radical con-

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1. See the works cited above, chap. 3, n. 31. And setting aside, as always, the case in which each manuscript represents an independent "redaction," it should be noted that it is not at all true that the "lesser evil" is to follow a single manuscript when no stemma can be reconstructed. In these cases the lesser evil is to choose the variants according to internal criteria, without abandoning the attempt to provide a complete evaluation of the greater or lesser tendency of each manuscript's copyist to reproduce the model faithfully even where it is corrupt or on the other hand to "patch it up," to "pretify," to falsify. It is senseless to reject such a procedure as "eclectic." Every time more than one copyist transcribes a model, "eclecticism" is objectively created, inasmuch as they make different mistakes in different parts of the text, with rare exceptions. To this random and irrational eclecticism we must oppose our choice, which is based on rational argument and therefore is not eclectic in the pejorative sense.

clusions. And yet he had to admit that Bédier's accusation remained substantially valid. As he writes, "Bifid trees are about 75-76 percent; they are 81-83 percent if the uncertain trees are not included in the total. Even if their predominance is not as overwhelming as in Bédier's statistics, it still remains quite remarkable. Four bifid stamens for every multifid one: that is a ratio which does indeed seem surprising."

As for Greek and Latin texts, in 1932 Pasqualli was still able to collect a respectable number of stamens with more than two branches on the basis of the most authoritative editions; he himself cited some of them two years later in his *Storia della tradizione* (e.g., 1934 [1934]: 149, 195, 270, 303). And yet it is not by chance that his brash phrase about the "untame" multiparite stamens is no longer to be found in that book; it is not by chance that Pasqualli later returned to attacking Bédier rightly for his demand that editions should be based on a single manuscript but never again breathed a word regarding the question of stamens with more than two branches. Those very same passages of his *Storia della tradizione* that we have cited just now, and others which could also be cited, demonstrate that even when Pasqualli accepts a multiparite stamen as a starting point for his discussion about the tradition of an author, he almost always ends up observing later that the distinction into "branches" or families is far less clear than is customarily believed, or even making such branches (it matters little whether they are two or more in number) go back to stages earlier than the archetypie, to ancient editions compared with which the medieval archetypie, even if it is still hypothesized, is conceived as a "collecting basin" for different traditions, or else as a true Lachmannian archetypie, but one whose descendants have received by collation the contribution of other streams of ancient tradition, now lost (on this possibility see below, p. 183 and n. 53).

Thus Pasqualli already admitted implicitly that there were only a few true tripartite stamens, and later studies have reduced their number rather than increasing it. In Classical philology the same phenomenon has occurred that Bédier observed in his own field of studies (and that anyway had already begun with Lachm and Madvig, as has been seen): manuscript traditions that had first been considered tripartite were later reduced to only two initial subarchetypes. This has happened, for example, for Plato's fourth tetralogy and for Macrobius's *Saturnalia*? And just now Giorgio Battrista Alberti has performed a strict examination that leaves very few stamens with more than two branches intact (Alberti 1979). In certain cases this exami-

5. For Plato see the introduction to Carlini 1965; other Platonic tetralogies or individual dialogues cannot be traced back to medieval archetypes, so that the problem that interests us here does not even arise. For Macrobius, Willin 1957: 156-57 against a tripartite stamen proposed earlier by A. La Penna.

6. Cf. Alberti 1979: 67-68, who discusses the tripartite stammas proposed by Castagna 1976: 131-144. Apart from three other passages, which Alberti too admits are not decisive, the only conjectural error between two of the three branches traced out by Castagna is *airos for aridos* at Calpurnius Scilicus 2.48. The error belongs to the category of confusion between words of similar graphical or phonetic "total appearance," on which cf. Timpanaro 1976: 64-71, 97, with further bibliographical references; it may already have been present in the archetypie, and the correct reading may be the fruit either of conjecture or of collation with another manuscript unknown to us (extra-stematic contamination; cf. below, p. 170). Alberti declares that he is skeptical with regard to the former possibility but he does not consider the latter one. But I repeat that the price that must be paid in order to obtain a bipartite stamen is a subarchetypie whose copyist committed only one serious error. On the tradition of Calpurnius Scilicus see also below, p. 173. One problem I would like to reexamine sometime is that of Cicero's *Catilinaires* (Alberti 1979: 61-67). On the tradition of the lesser Latin bucolic poets, see now, after Castagna 1976, Recchi 1978. I have not yet had the time to reexamine this problem with the attention it deserves."

7. As we shall see, such arguments only rarely induce Alberti to construct bipartite stammas (cf. below, p. 183); in substance his conclusions coincide with my own regarding the role played by contamination. Alberti is always or almost always right in his opposition to hypothesized stammas with more than two branches which permit a mechanical recension.
harm, to avoid adducing more than two of these copies in order to reconstruct a hyparchetype of no stemmatic importance. 8

Objections against this statistical argument have been raised correctly, if a bit too briefly, by Jean Irigoin, Arrigo Castellani, and István Frank; 9 and more recently, after the first edition of the present study, Alexander Kleinlogel has expressed even more radical objections, as we shall see shortly. 10 But since Maas (an exceptional philologist, but entirely impervious to other people's objections, entirely unable to understand from others anything that he did not understand from himself) always continued to believe in the validity of that absurd argument, 11 and later as well, and even recently has received the approval of scholars who are excellent but too inclined to insebereinverba magistri (to swear upon their teacher's words), 12 it will not be useless to return to this question in a rather detailed way, even at the cost of causing the reader some fatigue. 13

According to Maas (1937: 287–89 = 1958 [1927] 44–47), with three witnesses there are twenty-two possible stemmatic types. More precisely, there are six combinations in which from a first manuscript a second one derives, and from this latter a third one; three combinations in which from one of the three manuscripts derive the other two; three combinations in which two manuscripts derive from the third by means of a lost intermediary; three combinations in which a lost archetype has produced on the one hand one of the surviving manuscripts and on the other a lost manuscript from which in turn the other two surviving ones derive; six combinations in which a lost archetype has given rise to two surviving manuscripts, from one of which the third surviving one is then derived; and finally one combination in which each of the three surviving manuscripts is derived independently from a lost archetype. This last combination is the only tripartite one, while none of the other twenty-one is tripartite. Here is an outline of the various species of stemmas (the Greek letters indicate lost manuscripts):

\[
\begin{align*}
\text{I} & \quad \text{II} & \quad \text{III} \\
\alpha & \quad \alpha & \quad \alpha \\
A & \quad B & \quad A \\
\beta & \quad \beta & \quad \beta \\
C & \quad C & \quad C \\
(6 \text{ combinat.}) & (3 \text{ combinat.}) & (3 \text{ combinat.})
\end{align*}
\]

\[
\begin{align*}
\text{IV} & \quad \text{V} & \quad \text{VI} \\
\alpha & \quad \alpha & \quad \beta \\
A & \quad B & \quad A \\
\beta & \quad \gamma & \quad B \\
C & \quad C & \quad C \\
(3 \text{ combinat.}) & (6 \text{ combinat.}) & (1 \text{ combinat.})
\end{align*}
\]

But to combine all these possibilities under the common label of “types of stemma possible where three witnesses exist” means to lump together things that ought to remain quite distinct for the purposes of the calculation of probabilities. For in the stemmas of the first and second species there was an original total of three manuscripts, all three preserved; in the stemmas of the third, fifth, and sixth species there was an original total of four manuscripts, of which one has been lost and three are preserved; in the fourth species finally the lost manuscripts are two, that is, the original total was five manuscripts. The only element common to the twenty-two types listed by Maas is the fact that the surviving manuscripts are always three. But the number of genealogical combinations of three surviving manuscripts out of an undefined number of originally existing manuscripts is not twenty-two but infinite. Maas’s list does not include, for example, stemmas like these:

\[
\begin{align*}
\alpha & \quad \beta \\
A & \quad B \\
\gamma & \quad C
\end{align*}
\]

9. Irigoin 1954; Castellani 1958 (1957) (an excellent treatise: Castellani gives a particularly detailed refutation of J. Fourquet’s arguments, cf. below, p. 164 and n. 13, but his refutation is also valid against Maas, to whom he replies briefly at p. 170) [Frank 1951: 465 (correct, but a bit too general).
10. Roncaglia 1951: 281–82; Erbe 1959: 97; Hering 1965. Hering’s article is an attempt to develop Maas’s argument further by correcting it in some secondary points by leaving its substance intact; it is worthwhile reading for some acute observations on individual points and for its author’s full knowledge of manuscript traditions, but Hering does not invalidate the basic objections that had already been raised against Maas by myself and others; indeed I would go so far as to say that he has not even understood them, even though they are not difficult. Even more recently, in a valuable anthology of writings on philological method, Bruno Basile selected from Maas precisely that wretched passage on stemmatic types and on the rarity of stemmas with more than two branches [Basile 1973: 55–64]! Cf. Belloni 1976: 307.
and like all the others that can be traced out by multiplying the number of lost manuscripts at will.

It will be objected that from the point of view of recensio these infinite stemmas can be reduced in every case to one of the twenty-two listed by Maas. The stemma that the textual critic ends up tracing out on the basis of the indications furnished by the shared corruptions is in fact a greatly simplified one, as has long since been made clear: the method of shared corruptions allows one to establish that two manuscripts AB both descend from a lost manuscript a, but except in rare cases it does not allow one to identify possible intermediate copies between a and A and between a and B. If it were possible to trace out the genealogical tree of all the manuscripts of a given text that really existed (what Fourquet and Castellani call "the real tree"), then this would almost always turn out to be much richer than the stemma that we end up reconstructing on the basis of shared corruptions. For the purposes of recensio, this causes no problems: our simplified stemmas function just as well for reconstructing the reading of the archetype as they would if we were able to trace out the "real stemmas." But when the point is to calculate the probability that three manuscripts belong either to a bipartite stemma or to a tripartite one, then it is not legitimate to neglect the real stemmas as Maas does and simply to assign an equal probability to each simplified stemma.

Let us consider, for example, these four stemmas:

```
\[ \begin{array}{ccc}
\phi & \phi & \phi \\
A & B & C \\
\end{array} \]
```

For the purposes of recensio, the first two have the right to an autonomous existence, but the third and fourth do not, since even if the existence of an intermediate member \( \phi \) between \( a \) and \( A \) or between \( a \) and \( B \) could be demonstrated, this would in any case be irrelevant for reconstructing the archetype's readings. But when we calculate the probabilities, it falsifies everything if we take account of the first and second type, attributing an equal probability to each one, and then eliminate the third and fourth ones by reasoning.


13. Castellani 1980 (1957): 164–70. Castellani 1980 (1957): 170ff cites another refutation of Fourquet by Whitehead-Pickford 1952: 2; it was to see these thanks to Castellani himself, who kindly lent me an offprint. See now also Whitehead-Pickford 1973: but this article has too many gaps to provide a good summary of the whole discussion, nor does it seem to me to contain new contributions of any great importance.

14. Castellani 1980 (1957): 171ff: "Answer to Mr. Maas: the number of bibl stemmatic types that three manuscripts can form has no importance. What matters is the place occupied by these three manuscripts in the real tree."

15. This objection was already raised by Fryxell 1954: 272, but he still gave too much credit to Maas, inasmuch as he maintained that only the twelve cases in which a surviving manuscript is the source of the other two were to be eliminated from his list.
But when Bédier noticed to his astonishment the great prevalence of bipartite stemmas over multipartite ones, he was referring to stemmas traced out after eliminatio codicium descriptionem had already been performed. So that while on the one hand Maas excludes from his list the infinite "real stemmas" to which three surviving manuscripts can belong, on the other hand he inflates the list illegitimately by including within it all the cases in which one or two of the three manuscripts ought to be eliminated.

In reality, the fundamental defect of all these probabilistic arguments is that they start "from the tail" instead of "from the head." People tend to forget that the real historical process is that a certain number of copies are derived from a model, and then a certain number of subcopies are derived from them, and so on. The inverse process, grouping together a certain number of copies so as to form different stemmatic figures, is purely abstract. Hence we should be trying not to see into how many bipartite or tripartite combinations a given number of manuscripts can be grouped together but to establish whether it is more probable that only two copies were initially derived from an archetype, or three, or more. Once we put the problem in this way, we immediately see that it cannot be resolved by means of a mere mathematical calculation. Whether a manuscript is copied only once, twice, or ten times depends on a complex set of cultural and economic conditions: the number of persons who wish to read that text, the number of copyists who are available for copying it, the cost of the writing materials, and so forth. And in the same way, the greater or lesser probability that all the copies of that text have been preserved or that they have been destroyed to a greater or lesser extent is determined by quite variable historical conditions. 16

The defectiveness of Maas's reasoning is already clear from what has already been said, but Kleinlogel's arguments make it even more evident. He demonstrates that Maas presupposes tacitly and unconsciously, and without the least experimental basis, that the different stemmatic types are to be considered as "equiprobable events" (Kleinlogel 1968: 66–68). 17 Quite correctly he objects: "In Maas's typology we neither have statistics which establish that each type is found with the same frequency nor have we any reason to assign them the same probability a priori. The structural differences that provide the only criterion for classifying the types imply nothing about their probability or frequency" (1968: 67). I myself am not capable of following Kleinlogel's argument in all its steps and details (he also avoided himself of the advice of a mathematician, H. G. Kellerer; cf. Kleinlogel 1968: 678), but the conclusion is clear (1968: 74–75) and agrees with what others and I had already observed: it is not possible to solve the problem of the great prevalence of bipartite stemmas by merely deductive means, by an abstract calculation of probabilities unsupported by empirical data.

The second of Maas's arguments that we reported above seems to lead us onto a genuinely historical and empirical terrain, and hence a much more concrete one: "it is in the very nature of the medieval tradition that in the case of little-read texts three copies were only rarely taken from the same archetype; more rarely still have all these copies, or descendants from each of them, survived; on the other hand where texts were much read there is a tendency for contamination to creep in, and where contamination exists the science of stemmatics in the strict sense breaks down." And yet this argument too turns out to be fallacious if it is examined with a minimum of attention. Maas's reference to "little-read texts" can certainly explain well the lack of stemmas with ten or twenty branches, for example, but it does not at all suffice to explain the enormous divergence in frequency between stemmas with two branches and stemmas with three. We would obviously expect to find a gradually decreasing frequency of stemmas corresponding inversely with an equally gradual increase in the number of their branches: for example, given fifteen different manuscript traditions, five stemmas with two branches, four with three, three with four, two with five, one with six—obviously I have artificially regularized this example for the sake of expository convenience: in reality the decrease would turn out to be more "capricious." But what we cannot understand is such an abrupt "jump" between the number of bipartite stemmas and the number of tripartite ones. It seems that Maas and the many scholars who have found this argument of his convincing have forgotten that the difference between two and three is only one! To listen to them, one would think that the category of "poor traditions" is

where only two manuscripts remain, the problem of a tripartite stemma does not even arise; hence six other combinations must also be eliminated.

16. According to Greg 1930–31: 405–1, Andrieu 1943: 462, and Ullman 1956: 586, the preponderance of bipartite stemmas is precisely a consequence of the "decimation" undergone by most manuscript traditions. But Castellani 1980 (1957): 174 demonstrates that it is only in special cases that the decimation could have increased the number of bipartite stemmas (somewhat contrariwise, he later assigns greater importance to the decimation; 1980 [1957]: 181). Indeed, why must the decimation every time have been precisely so destructive as to let the descendents of exactly two subarchetypes survive and not of three? This is already unlikely from the point of view of an abstract calculation of probability; it becomes even more unlikely (and here Castellani perhaps did not go into the question deeply enough) if we consider that decimation too is a series of historical events that depend on accidental causes (fires, etc.) and degree of "cultural depression" that are highly variable and cannot be calculated in the absence of detailed documentation.

17. Even before Kleinlogel's article, this was pointed out to me privately by my friends Giuseppe Torresi and Gianpiero Zarrì.
constituted only by traditions with a single branch (for which stemmatic problems do not exist) and by those with two branches; the domain of "rich traditions" would suddenly begin starting with three branches. This is not at all "in the very nature of the medieval tradition," and it is not even in the nature of common sense: just because a tripartite tradition is only richer by a very little bit than a bipartite one, it ought to have occurred fairly often even in the case of little-read texts. Such a "jump" between bipartite stemmas and tripartite ones could only be explained if some regulation or intricate habit in the Middle Ages were known that ensured that every ancient manuscript was copied not more than two times and was then destroyed. But obviously we do not have the slightest indication of such a regulation, and it is entirely improbable. Among other considerations (but I realize that I am discussing absurdities at too great a length), how are we to imagine such a regulation remaining in effect just as much in the Latin and Romance and Germanic West as in the Byzantine East, in different cultural environments, in very different periods?

As we shall see later, the only thing that is really correct in that passage from Maas is his appeal to contamination. But Maas uses this too in a distorted way, exonercating the bipartite traditions from contamination (so that "stemmatic rigor" would rule in them) and postulating a contamination, beginning with tripartite traditions, so intense as to cancel out any genealogical relation. Since all traditions (including those with two branches: cf. above, chap. 6, n. 18; and below, pp. 182ff.) are more or less contaminated, and since richer traditions as a rule are more contaminated, one wonders why there should not have been numerous intermediate cases of traditions not so contaminated as to be irreducible to any stemma but on the other hand not so meager as to be limited to only two branches.

Setting aside Maas's pseudo-explanations, we can still wonder whether the conditions of the transmission of medieval texts were such as to explain this stubborn preponderance of bipartite stemmas. Various hypotheses have been put forward in this regard. For example, Castellani has acutely observed that the widespread diffusion of a text and a stemma with many branches need not be connected with one another (1980 [1957]: 175-82). Anyone who wants to obtain a certain number of copies from a manuscript in the shortest time possible should not have the model copied successively as many times as he wants there to be copies of it but rather should first have

the model copied, then have the model and the first copy copied simultaneously by two different copyists, and so on. By means of this procedure, which Castellani calls "production maximum," three copies can be obtained in two units of time, seven copies in three units of time, fifteen copies in four units of time. And whereas with the method of successive copies the production of seven copies results in a stemma with seven branches, the production of fifteen copies results in a stemma with fifteen branches, and so on, in the case of "production maximum" the production of seven copies results in a stemma with three branches, the production of fifteen copies results in a stemma of four branches, and so on.

Thus this type of diffusion leads to a considerable reduction in the initial ramification of the stemma, compared with the method of successive copies from a single model. But only to a considerable reduction: not necessarily to bipartite stemmas, unless other factors (nonproductivity of certain copies, rapid withdrawal or rapid destruction of the initial model, etc.) intervene that may in fact have operated in many cases but whose operation has no character of necessity at all and is in any case independent of the mechanism of "production maximum" as such. It also remains to be seen how far such a mechanism, which concentrates the production of many copies into a relatively brief time, corresponds to what we know about the transmission of Classical texts in the Middle Ages. It seems likely that copies of a given Classical text were not produced intensely but were staggered in time, at least in many cases: a manuscript of a work of Cicero, preserved in a monastery, will have been copied once or twice, and the copies will have been

18. As for the improbability, asserted by Maas, that "all these copies [. . .] survived," see what I have already observed in n. 19 about "decimation." Once again we return to the improbable "leap" in frequency between tripartite stemmas and bipartite ones. And it is ridiculous to describe just three copies as "all these copies."

19. Perhaps Castellani 1980 [1957]: 177-83 insists a bit too much on these additional factors. And in any case these do serve to explain a further reduction in the number of branches but not their almost constant reduction to only two.
sent elsewhere; then a third copy will have been made of that same manuscript years later, and so on. The model of “production maximum” is much better suited to the “publication” of a new work by a medieval author (in fact Castellani, a Romance philologist, was thinking of cases of this type) than to the medieval transmission of an ancient text. But even with these reservations, such a scheme remains valid to a certain extent for Classical texts too and can at least make some contribution toward explaining the rarity of stemmas with many branches. The most difficult fact to explain still remains the rarity of stemmas with three or four branches—I hope the reader will forgive my insistence, which may seem a bit obsessive but is, I believe, justified.

Another hypothesis has been proposed by D’Arco Silvio Avalle:21 the bipartition of so many stemmas would depend on a procedure of “embryonic pecia,” by means of which the model was divided into two parts and each of two copyists copied one half and then exchanged their parts; in this way two transcriptions could be performed in the same time in which a single copyist would have performed a single transcription; then the two branches of the stema originated from these two copies once the model had been lost or destroyed. I shall not repeat here at length the objections that I have expressed more fully elsewhere.22 But shall confine myself to a single point. It seems hard to believe (alas, I must return to the leitmotif of all my objections!) that exactly two copies were derived from the model with such consistency and that each time the model was not transcribed again before being lost. In the real system of pecia, which became widespread in the thirteenth and fourteenth centuries for the use of the students of the great medi-
universities, each copyist transcribed a fascicle, not a half manuscript,23 and this would lead to highly ramified stemmas. An analogous procedure of transcription divided up among various copyists was in use in the Hu-

10. Cf. also Balchino 1979: 238–41 on the usefulness of Castellani’s hypothesis with regard to the edition of medieval works; perhaps Brambilla-Ageno 1975: 138 is too quickly negative. On the fact that it is much less applicable to the transmission of ancient texts cf. also Kleinlogel 1968: 72 and n. 1. In any case it should be borne in mind that for Castellani the “production maximum” is not the most important cause of bipartition; he subordinates it to other causes that more specifically concern brief medieval texts and are therefore not applicable to ancient ones (except for documentation, on which see above, n. 16; Castellani 1980 [1977]: 181).


23. Destrez 1935 is still fundamental. Fink-Ertes 1963 is more up-to-date and disagrees with Destrez on various points but, at least according to my impression, does so with a certain ostentation of novelty to which real progress sometimes does not correspond.

manifest age.24 None of this can be imagined in the conditions of slow textual transmission of the High Middle Ages,25 when a limited number of copies seems in general to have been derived from one manuscript, often at intervals of time. Avalle’s “embryonic pecia” seems an artificial expedient, an unattested quid medium [something in the middle] between two essentially different types of production of copies. It must also be added that Avalle himself seems to have abandoned this hypothesis, which he had originally proposed with caution.26

We shall now propose two other hypotheses that fit better with what is known about the transmission of Classical texts, even if they are only suited for explaining certain cases of bipartitism.27 In any case, a separate category is formed by those cases in which the bipartitism really consists of the opposition between a single manuscript (or, more rarely, very few manuscripts) of the High Middle Ages or even of Late Antiquity on the one hand, and a considerable number of more recent manuscripts on the other. Cases of this sort are often cited in Pasquali’s work and in the first volume of the Geschicht der Textüberlieferung [History of text transmission; Hunger et al. 1961–64: vol. 1]: it should suffice to recall Aeschylus, Sophocles, Isocrates, and Theophrastus among Greek authors; Plautus, Terence, Seneca’s tragedies, and Statius’s Thebaid and Achilleid among Latin ones.28 It is perfectly natural that the more recent manuscripts should be united by a certain number of banalizations and conjectural “improvements,” or else by real corruptions, from which the oldest manuscript is immune: for they go back to an edition “revised” in the Carolingian period or even later29 that intro-


25. Some of the essays collected in Cavallo 1977 provide a clear idea regarding these conditions see especially G. Cavallo’s introduction and the essays by A. Pietucci and B. Bischoff. It goes without saying that I ought to cite many other recent and innovative works. many of them written by the same scholars I have mentioned now; but I cannot leave my subject too far behind. In any case, see the full bibliography in Cavallo 1977 and bear in mind that this research is still in full and rapid development.

26. He no longer mentions it in Avalle 1971; on p. 92 he speaks of pecia but without connecting it to the problem of bipartite stemmas.

27. Cf. Pasquali 1952a [1954]: 45–50, 126, 175–80, 254, 113–18, 534–47; Hunger et al. 1961–64: 1.364–65, 127–45, 375–78, 404–6, 409 (this latter work must be used with caution). See in particular Pasquali’s formulation at 1952a [1954] 126. "Like so many other texts, Seneca’s tragedies are transmitted in an ancient manuscript that stands apart and in a large number of more recent manuscripts linked together by close kinship relations." For Isocrates the existence of an archetype can probably not even be demonstrated, as Dieter Ermer points out to me, referring to Sek 1967: 106–7."
duced those changes and from then on constituted a vulgate, but also, un-
surprisingly, preserved correct readings that were corrupted in the oldest
manuscript.\textsuperscript{28,29} In this case the bipartition is really caused by a chronologi-
cal discrepancy—so long as we understand this expression to designate not
a mere interval of years or centuries but a changed cultural environment, the
arrival of those "medieval Renaissances" that occurred several times in
western and central Europe, and even more in the Byzantine Empire. To this
must be added the fact that the group of more recent manuscripts owes its
homogeneity, at least in many cases, not only to medieval or even Human-
ist innovations but also to their shared derivation from an ancient edition
that was different from the one of which a single witness remains.\textsuperscript{30} On
the other hand, given that medieval manuscripts earlier than the ninth century
are relatively rare and manuscripts going back to Late Antiquity are even
rarer, it is not at all strange that for many texts only a single codex vetustior
[more ancient manuscript] has been preserved, which then ends up constit-
tuting by itself a "first family" as opposed to the "second one" represented by the
recentiores.\textsuperscript{29} This was more or less how Marouzeau already ex-
plained the prevalence of bipartite stemmas in the Latin tradition; analogous
considerations are applicable to Greek texts as well, but they cannot
be applied to all Latin texts: Marouzeau apparently limited his hypothesis
too much in one direction and extended it too far in the other.\textsuperscript{29,30}

28. An analogous but more complex case is constituted by the tradition of Hippocrates
see D. Irwin’s interesting note to Timpanaro 1971: 151 n 86a.

29. In cases of this sort scholars have almost always proposed the hypothesis that the
recentiores derived from the most ancient manuscript; this hypothesis is sometimes cor-
rect but more often erroneous (see above, pp. 106, 124 and chaps. 3 and 4 of Pasquali
1952a [1954]).

30. I say "apparently" because Marouzeau’s thesis, which I have reformulated quite
freely here, is known to us only from a brief reference in Dain 1975: 132: 115; "He ex-
plains this biphyl by the fact that one part of the tradition came from the copy that had
been transilluminated around 960 while the other part was derived from pre-Carolinian
copies antedating the transliteration." At the time of the first edition of this work of mine,
Dain and Marouzeau were both still alive; but when I asked them, neither one could re-
member where this explanation had been formulated for the first time and more fully. "I am
ninety-two years old, and I do not remember!" Marouzeau wrote to me with some sad-
The formation of such a vulgate need not be connected in every case with the translitera-
tion, conceived too simplistically as an operation performed once and for all. Such a con-
ception has been shown to be fallacious for many Greek texts and is even less certain for
Latin ones, for which indeed there was strictly speaking no transliteration in the "Byzan-
tine" sense. Those vulgates were formed gradually by processes of contamination and con-
vergence caused by shared cultural environments and hence by the intensification of con-
tacts and exchanges (see also above, chap. 8, n. 31).

31. Dain 1952: 79–85; cf. Dain 1975 [1949]: 112. Yet in 1964, the year in which the
second edition of his Manuscrits appeared, Dain 1964: 111 refused to accord any signif-
cance to the problem of bipartite or tripartite stemmas; but the example he adopts reveals
that he was now thinking of traditions without a medieval archetype (however conceived),
in which the various manuscripts or groups of manuscripts represent the continuation of
different ancient editions. This is certainly the case in many traditions, especially Greek
ones (cf. p. 182), but not in all. So in my view one cannot speak of a "false problem." On the
contrary, as we shall see, the discussion of the rarity of multipartite stemmas makes an
important contribution to the criticism of Lachmannian.

32. Irigoin 1944: 213–14. A "fluid archetype" is also hypothesized by Reynolds
1965: 56.

33. We have run into cases of this sort with regard to ancestors that are preserved and therefore allow the descriti to be eliminated (pp. 471, 49; and many other examples
could be cited). But entirely analogous damage might have occurred and did in fact occur
to archetypes in the Madvigian-Lachmannian sense, that is, to lost manuscripts, beginning
with Lascaris (see immediately below).
APPENDIX C

when in reality it is worth "two against one." 34 We have already drawn attention to this danger above (chap. 6, n. 13), and we have explained that major lacunae or transpositions that can be attributed to the loss or displacement of leaves or fascicles are not enough by themselves to define a subgroup.

With this hypothesis of the "mobile archetype" we have entered into the field of those manuscript traditions that have a bipartite appearance, although in reality they were tripartite or had even more than three branches. But this hypothesis, like Marozew's discussed just now, only explains a limited number of cases. It is not reasonable to suppose that between the first transcription and subsequent ones almost all archetypes underwent corrections so numerous and so extensive as to produce an "apparent sub-archetype." Moreover, it is easy to identify the case of later material damage to the archetype, and most manuscript traditions are free of it. There must be other reasons for this "deception," which produces errors of classification and makes bipartite stemmas seem much more numerous than would seem probable. We shall go on now to examine them, beginning with the most banal and avoidable ones and finally arriving at the ones that are harder to avoid and all in all more frequent.8

In my opinion, the old custom of classifying manuscripts not genealogically but axiologically by dividing them into the two categories of meliores and deteriores may have contributed toward increasing the number of bipartite stemmas beyond due measure. We have already seen how the young Lachmann still followed this custom, and how some of the first genealogical classifications (e.g., Madvig's of the manuscripts of De fribus) originated in earlier axiological classifications and inherited their bipartite structure.35 It is likely that scholars often merely transferred into the genealogical domain the old bipartition based on a judgment of value, and therefore derived from the archetype two apographs, a "good" one (the work of a stupid and faithful copyist) and a "bad" one (the work of a deceitful interpolator), from which the two races of the meliores and the deteriores would have originated. The deteriores in particular will often have been considered too hastily to represent a single class from the genealogical viewpoint as well; it will be remembered that Moritz Haupt, like Lachmann, exorted scholars not to waste their time classifying Humanist manuscripts.36 His exhortation was largely followed, not only because such manuscripts were distrusted but also precisely because they are more contaminated and therefore harder to classify than medieval ones. Sometimes the old editors added a third group of mixti alongside the two families of meliores and deteriores; 37 but tripartite classifications of this sort were destined to be short-lived, since later editors ended up either adding the mixti to the family of the deteriores or else noticing that almost all manuscripts are "mixed," that is, contaminated (we shall return to this point shortly).38

Another cause of erroneous bipartite classifications is partially connected with the preceding one but is more strictly derived from a logical mistake:39 the tendency to identify one class of manuscripts α on the basis of shared characteristics and then to call it everything that in reality is merely "non-α." There is a danger of falling into a similar error not only in textual criticism but wherever classifications need to be made: Aristotle already fought against it in zoology.40 If a certain number of shared corruptions defines a family of manuscripts, the lack of those corruptions does not define another family: so after having identified a family α it will be necessary to see whether the other manuscripts are connected by shared innovations in their turn, or whether instead they constitute different groups, or whether, as is also possible, they are so contaminated that their derivation from one or more subarchetypes cannot be detected. An error of this sort was committed by Heinrich Schenkl when he divided the V class of the manuscripts of Calpurnius's eclogues into two subclasses v and ω: Cesare Giarratano (a Classical philologist who may not have had the gift of genius but was rigid and scrupulous as few others) observed that a real family but v was a heap of manuscripts not united by particular affinities. Schenkl replied that he had indeed used the sign ν in the sense of "V minus ω: but even then he refused to recognize the illegitimacy of a stemma traced out according to such criteria.41 The mistake Mario Casella committed in his attempt to classify the manuscripts of Dante's Commedia was analogous: the family α which he believed he could identify "is characterized in exclusively

34. I do not understand the reply of Kleinlogel 1968: 796 in this point. He objects against me that the two more recent apographs have the value of two against one "only [...] when the change of condition consisted in a purely mechanical corruption." But this is the very case I was considering above; and the fact that this is not a purely theoretical case is demonstrated by the example of Lucretius, discussed in chap. 6, n. 13.
35. See above, pp. 77-78, 92-93, 98.
36. Above, chap. 3, n. 15.
37. So, e.g., Goecen 1809-13: vi; Nipperse 1847-56: 1.46-47 (see above, p. 96 and n. 18); and Orelli 1825-28: 4.6.
negative terms, as not β. Here Bédier would indeed have been right to point to Schenkl and Casella as two victims of the dichotomic aberration! Only, such an aberration is not without remedy.5

But the most serious and insidious causes of "apparent bipartition" are contamination ("horizontal transmission," the copyists' conjectural activity, and, even if to a lesser extent, polygenesis of innovations. For the sake of brevity I use the term disturbances to indicate all three of these phenomena, although I distinguish them whenever it is necessary to do so for my argument.

I intend to insist here not on the general difficulties that these phenomena cause the textual critic—I would merely be repeating what is to be found in every good manual of textual criticism, and Pasquale's book can be said to be a full critical examination of "disturbed" traditions, each with its own particular problems—but rather on the mechanism by which they lead scholars to interpose fictitious subarchetypes between the ancestor and its descendants and hence to confer a bipartite appearance upon manuscript traditions that in reality had three or more branches.

Let there be given a tripartite tradition: from an archetype ω let there be independently derived three copies that produced three streams of tradition ωφβ by successive "vertical" transmissions. If at a certain time a process of horizontal transmission intervenes, because of which a certain number of errors of β are transmitted to γ or vice versa, or else if a copyist of the α branch corrects a good number of errors of the archetype by felicitous conjectures, the shared errors of φγ (which in the former alternative had originally been errors of β alone or of γ alone and in the latter one were originally errors shared by the whole tradition) will be attributed to a subarchetype, and the tripartite tradition (fig. 1) will assume a deceptive bipartite appearance (fig. 2):

```
          ω
           \
          /  \
         /    \ 
        α     β  γ
          \
           α
     fig. 1
```

```
          ω
           \
          /  \
         /    \ 
        α     β  γ
          \
           α
     fig. 2
```

The two phenomena can also be associated with one another, and this will certainly have happened in many cases given four branches of tradition


41. Castellani 1980 (1957): 181–82; these phenomena had already been mentioned in passing by Bédier and Fourquet, both cited by Castellani.

42. Cf. above all Segre 1961; Avalle 1961: 159–78 (Appendix 1, "On Some Remedies against Contamination"); more briefly Avalle 1972: 83; Olken 1970 (θ one to D. Inner my knowledge of this work, which reexamines a text already edited by Lachauer).
most completely the distinction between coincidence in conservation (which in itself proves nothing) and coincidence in innovation;\textsuperscript{43} and I have already indicated the reasons why I think (and others agree) that clinging to Dom Quentin's method is a bad way of escaping the difficulties of Lachmann's method (see above, chap. 4, n. 18).

Another, much surer criterion was already enunciated a long time ago, was repeated with particular rigor by Ulrich Knoche and Pasquale,\textsuperscript{44} has been reaffirmed, as we saw just now, by Castellani in general and by all those who have recently written treatises on textual criticism, and is recognized as fundamental by Avele himself: corruptions and, even more, lacu-

\textsuperscript{43} Timpanaro 1056: 339 and n. 14.

\textsuperscript{44} See above, p. 357; and Pasquale 1953 (1934): xvii. "Also errors that would seem to us to be obvious ones are often penetrate into manuscripts by collation. It is only the lacu-
nas that, at least as a rule, are transmitted directly." This principle would seem to be formulaed almost too restrictively. And yet some cases, naturally very rare ones, can apparently be documented in which even omissions that destroy the meaning have been transmitted by contamination: cf. Willis 1972: 22, who mentions the case of a copyist of Macrobius's Saturnalia (1.6.14), who "has performed the remarkable feat of interpolating an omission," deriving the omission itself from another family of manuscripts (the case is highly likely, even if not quite certain, since macron is not found in the text). The reviewer of forgottenandfollowed by perrunsettius runs a certain risk of being omitted independently by two different copyists. It would be worth developing further an interesting point made by La Penna 1964: 369: "The need for a critical apparatus, for a recension must have existed in each of the medieval compilers of variants, even before the Humanistic ones; sometimes they are not for themselves not only variants which yield a plausible meaning, as is their custom, but also meaningless variants; for the variant that serves to correct the text, or that produce another acceptable text, and which could be called 'correction variants,' are sometimes added others that serve to show how the text is transmitted elsewhere, and which could be called 'apparatus variants.'" Well, let us suppose that an "apparatus vari-
ant," noted down by a learned copyist in a manuscript that is not longer extant, has been mistaken for a "correction variant" by a later, ignorant copyist who has introduced it into the text; the result will be an error of the sort that we are accustomed to attribute with cer-
tainty to vertical transmission, and that instead has been transmitted horizontally (there is a case of this kind, even if it is somewhat more complicated, in Timpanaro 1978: 408–10). Marginal and interlinear "apparatus variants" are also found in ancient manuscripts: see the case, cited by Pasquale 1953 (1934): 236, of P. Oxyxyxuchus 1477 (a passage from Plato's Phaedrus) furnished with variants "that cannot be conjectures, because too often they make the text obviously worse." Papyrologists will be able to cite other examples; it is likely that some variants of this sort have insinuated themselves into the medieval tradi-
tion, or into one part of it.\textsuperscript{46} On the possibility of the horizontal transmission of evident errors, see also Pasquale 1953 (1934): 87. An extremely interesting example is furnished by the readings of the second hand in the Ambrosian-Vatican palimpsest of Frosten (Mi-
lan, Ambros. E.247 sup. + Vatican lat. 5750). Cf. Zetzel 1980 (pp. 49–57) on Frosten: a precious collection of material, not always well interpreted.\textsuperscript{47}

\textsuperscript{45} For Greek texts, see, after Maas 1935 and 1936, also Erbe 1959: 99–100. For Latin ones see, e.g., Musari 1970: xxii–xxiii, but I can think of examples that reflect the recent tendency of scholars to reject the "residuities" that have long been considered as such. For example, see also Pasquale 1953 (1934): 248, 304–5, 318–19 (but on the tradition of Thucy-
dides see the recent work by Barnet, Alberti, and Kleinlogel, and briefly summarized in Alberti 1970: 10–11, 381–85, and elsewhere. Cf. also Andria 1943: 468; Di Benedetto 1965 (see the index under "contaminazione").

\textsuperscript{46} E.g., Pasquale 1953 (1934): 48, 49, 304–5, 318–19 (but on the tradition of Thucy-
dides see the progress later achieved by Barnet, Alberti, and Kleinlogel, and briefly summarized in Alberti 1970: 10–11, 381–85, and elsewhere. Cf. also Andria 1943: 468; Di Benedetto 1965 (see the index under "contaminazione").

\textsuperscript{47} In German, the term Fremdzeugen [foreign reading], coined by Fränkel 1964: 78 and n. 1, can be useful for indicating a reading that derives from a line of tradition oth-
erwise unknown to us.
of apparent bipartite stemmas: since the damage that goes back to the archetype has been corrected in $\alpha$, such damage will be attributed to a subarchetype, and the stemma will once again take on the false appearance of figure $\text{a}$ on page 176; and what is worse, if the error remaining in $\beta$ is of the sort that cannot be healed conjecturally, it will all the more have the appearance of an unquestionably genuine conjunctive error.

And yet the difficulties we have reminded the reader of, serious as they are, do not always prevent the construction of a plausible stemma based on rigorous criteria, that is, solely on "significant errors," excluding all coincidences not only in correct readings but also in banalizations, in "errors with the semblance of truth" that can easily be transmitted by collation, and in errors that can be polygenetic, and taking account also of the possibility of conjunctual activity on the part of the copyists. The possibility of "extra-stemmatic contamination" will always remain, alongside less frequent risks like those indicated in note 44; but if this is limited to a few cases it will not completely alter the fundamental outlines of the stemma; whereas if it is systematic it will be recognized as another source of tradition that can be indicated with figures like this (which have already been used by a number of editors):

$$\begin{align*}
\alpha & \quad \omega \\
\omega & \quad \beta
\end{align*}$$

But it is here that the most insidious dangers begin. Let us suppose that a stemma with three branches has been produced on the basis of "significant errors" alone. What is to be done now with the coincidences in insignificant errors? We cannot either attribute them with certainty to vertical transmission or exclude the possibility that they might be the "residue" of errors already present in the archetype and corrected conjuncturally in one branch of the stemma—this is precisely what their insignificance consists in. But we cannot even exclude the possibility that such coincidences derive from vertical transmission, that is, that they go back to a subarchetype. To put this point better: we can exclude this possibility only if it happens to contradict a stemma that has already been constructed on the basis of significant errors alone. If, for example, a manuscript $A$ shares lacunas and other gross errors with $B$ but only a few banalizations or "pretifications" or unimportant alterations with $C$, we will certainly attribute these latter to disturbances. But if $A$ does not visibly share significant errors with $B$ and shares only insignificant errors with $C$, or else if it shares more insignificant errors with $C$ and fewer ones with $B$, we can neither affirm nor deny that $AC$ are derived from a subarchetype.

These are the cases in which the philologist who wishes to reduce the disturbances to a minimum and to present in a stemma as systematic as possible an image of the manuscript tradition will "verticalize" the insignificant errors too (or the largest group of them) and will postulate a new subarchetype, which can but need not ever have existed.

This is the criterion of the "most economical hypothesis" theorized by Avalle (with a certain orientation of analogies with the physical and mathematical sciences) but already broadly followed in practice by Classical and Romance philologists. Recently Irigoin too has reaffirmed the importance of reconstructing the skeleton of the vertical transmission as far as possible, in an important article that was inspired by the legitimate desire to react against Dawe's exasperately and exaggeratedly antistematic tendency.

In itself, the criterion of the most economical hypothesis is probabilistic and entirely reasonable. Since, as Pasquale reminded Schwartz (see above, p. 156), vertical transmission is a constant fact, while disturbances are extremely frequent phenomena that exist in all or almost all manuscript traditions but are nonetheless always desultory and accidental, every single coincidence in innovations (even in insignificant innovations) has a greater chance of deriving from a lost shared model than of being due to contamination or polygenesis or a copyist's corrective activity. But if we prefer the hypothesis of vertical transmission every time, the total picture of manus-

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48. Cf. Avalle 1961: 172, 194 (and already 1957: 64); 1972: 82–86. In this last work Avalle (perhaps answering implicitly my objections in Timpanaro 1965: 597) vigorously asserts that the principle of the "most economical hypothesis" should not be followed to the detriment of stemmatic research performed with strict criteria, and in particular should not lead to an undue increase in the number of subarchetypes. An excellent warning; and yet I have the impression that in this way the principle itself is invalidated, and survives more as an "epistemological coquetry" than as an instrument that can be used for textual criticism.

49. Irigoin 1977: "Without permitting himself to be led astray by superficial and contradictory links, he [i.e., the textual critic] must try to discover underneath them the constatist of the 'vertical' tradition with all the means available to his scholarship and to his native talent. 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" (144). There is nothing really erroneous in this formulation (but in fact there is: there is also the case of "total pre-traditional contamination") but anyone who sets out to do stemmatic work in this spirit will end up verticalizing everything that can be verticalized and admitting disturbances only as a last resort. And by multiplying the subarchetypes he will naturally obtain a bipartite stemma.
script traditions will certainly be distorted, because we have thereby reduced to zero an event that will have happened in a minority of cases but will nonetheless almost certainly have happened, and in a rather large minority of cases. For example, instead of 60 percent of coincidences due to vertical transmission and 40 percent due to disturbances (percentages that obviously are hypothetical and unfortunately cannot be tested, but all the same seen plausible), we shall obtain 100 percent of coincidences of the former sort—with an evident error. The main reason (not the only one, as we have seen) for the improbable prevalence of bipartite stemmas consists precisely in this paradox: if in every case of coincidence in innovation one prefers the more economical hypothesis, the majority becomes a totality, while the minority is cancelled out. Naturally the paradox only persists because in textual criticism, unlike other situations in which a statistical prediction can be tested empirically post eventum, no similar verification is possible: no one can tell us whether certain presumed lost subarchetypes really existed or not.\footnote{50} And if we studied this question in greater depth, we would discover that this serious limit to the application of statistical methods is found in many other diachronic disciplines as well, in which it is not possible to perform repeatable experiments and test their outcome.

These considerations of ours are substantially corroborated by the book of Alberti's I have already mentioned, even if, as I have indicated, Alberti dissects more than once from my earlier statements regarding stemmas that at the time I presumed to be multiparte. To a hasty reader (especially to one who does not read closely the book's brief but fundamental last chapter), Alberti's treatment might seem to confirm the thesis of "extreme bipartitism."\footnote{51} Certainly, as we have seen (pp. 160f.), he demonstrates the fragility or improbability of most of the few multiparte stemmas that scholars of Greek and Latin manuscript traditions have hitherto hypothesized. But, as he himself takes care to emphasize (Alberti 1979: 93), only very rarely does he replace them with bipartite stemmas: instead he speaks for the most part of "probable contamination, which has rendered the relations among the various branches uncertain but without eliminating them" (1979: 93); in other cases he concludes that contamination has completely obscured the

51. As is well known, the problem of the existence or nonexistence of a medieval (or late ancient) archetype is rendered difficult by the contradictory situation we find in many surviving texts: on the one hand, the existence of gross errors and above all lacunas, and sometimes also dislocations of whole passages, in all the extant manuscripts, which make it hard to exclude the existence of a single lost ancestor that exhibited that damage; on the other hand the fact that the ancient tradition (papyri, quotations in other authors, etc.) often agrees in inferior readings now with one part of the medieval tradition, now with another. Pasquale 1912a (1934) is full of examples of this sort, which have increased even further in the last decades. Against the hypothesis of the "archetype with variants" (to which Pasquale still granted too much), cf. D. Di Benedetto 1965: 145–46, 149–52. It has been possible to demonstrate in many cases that the errors shared by our whole tradition were of a sort judged "tolerable" by ancient and Byzantine philologists (for Europe, but with observations that can be applied to other manuscript traditions as well, see D. Di Benedetto 1965: 165–93; for Hebrews Theologum, Arrighetti 1961: 266–79 has cast doubt on the existence of an archetype with good arguments). A loose conception of the archetype, not as a unique exemplar but as a "kind of text," as a proto-medieval vulgate already contaminated by horizontally transmitted errors, is proposed by West 1973: 41–42 and Winant 1979: 79. In my opinion, the concept of archetype formulated by Dain and taken up again by Frings (also in Frings 1977) has not been very useful and has also caused terminological confusion. I am sorry to see how little the difficulties regarding the concept of archetype pointed out by Eugenio Grassi have been taken into consideration, except for D. Di Benedetto (cf. above, p. 18). In the face of so complicated a problem, which in many aspects concerns Romance texts as well, it seems strange how hastily a scholar as learned and competent as Franco Beinnibilla Ageno (1975:38) perceptibly asserts that there always was an archetype, relying on arguments of statistical probability that unfortunately recall the ones Mazz used when he tried to demonstrate the numerical prevalence of stemmas with two branches. Prof. Beinnibilla Ageno kindly informs me by letter that her ideas regarding the archetype only concern traditions of vernacular texts with specific characteristics: despite the title of her article and some formulations it contains, they are not intended to set forth a theory covering all cases. She also informs me that she intends to return to this subject with more detailed arguments.\footnote{52}
not in the sense of making them become tripartite or multipartite, but rather
in the sense of introducing elements of doubt, due above all to contami-
nation."

So must we abandon altogether the attempt to trace out stemmas (cf.
Munari). 60 No, except when the tradition is totally disturbed. Instead,
above all, we must continue Alberti's work by examining many stemmas
that are asserted to be certainly bipartite. I do not exclude 61 the possibility
that such an examination might sometimes correct earlier errors of classi-
ification and lead to the construction of multipartite stemmas still suffi-
ciently solid despite some inevitable traces of disturbance; 62 in this regard I
would be a bit more confident than Alberti. All the same I too believe that
results of this sort will be quite rare. I think instead that in more cases those "el-
ements of doubt" to which Alberti refers will induce us to propose two or
three stemmas as equally probable or one as just a little more probable than
the others 63 and in the prolegomena of the editions or in separate works to
state clearly which subarchetypes can be postulated with almost absolute
certainty, which ones with a good probability, which ones only with con-
siderable uncertainty. Sometimes it will also be necessary to trace out stem-
mas of only one part of the tradition but to give up on them for another part
that is too disturbed. 64 In this way the user of a critical edition will know for
the practical purposes of constituting the text to what point the agreement of
certain manuscripts invests a certain reading with "authority" (an au-
thority that is always relative and in need of confirmation). 65

Finally, a last point. In his notorious treatment of stemmas with two

62. Sometimes a "third branch" can be obtained by examining a series of corrections
derived from a lost manuscript: cf. Naeo 1966. In other cases (as, for example, for Thuc-
dydides; see above, n. 46) an analogous technique, which for Greek texts can also take ad-
vantage of papyri, makes it possible to reconstruct lines of extra-stemmatic tradition, even
if only partially.

63. This custom is already followed by some scholars (e.g., La Penna 1957: colvi), but
in my opinion it should become more widespread: not so as to derive from it the purely
skeptical and destructive consequences Bédier was aiming at when he had fun tracing out
the ten possible stemmas of the Lai de l'Ombre (many of which, in fact, were unhoused:
Castellani 1988 [1917]: 182–92), but in order to distinguish better the various possibil-
ities and the various degrees of probability. Editors still have recourse too often to the two
extreme solutions: either to trace out with excessive confidence a single stemma or to ab-
sort from any discussion of the genealogy of the manuscripts.

64. This kind of solution, in my opinion, can be recommended, for example, for the
Ephemenon of Dictys Cretensis (L. Septimian), where family C can be reduced to a fairly
rigorous stemma, while family E is too highly disturbed: cf. Timpanaro 1978: 357–442,
and now Eisenhut 1975, who partially takes account of my observations but does not
abandon the attempt to trace out a complete stemma.

or more branches, at the end of the passage we cited on page 161, Maas
wrote, "In the later subbranches it would certainly have been easier to pre-
suppose the existence, and survival, of three copies from the same arch-
type; 66 but in these cases the editors were often able, without doing any
harm, to avoid adding more than two of these copies in order to recon-
struct a hyperarchetype of no stemmatic importance."

Two objections come to mind upon reading these words. Above all, it is
curious that Maas envisioned the case of "later subbranches" only in a man-
uscript tradition that presents more ancient witnesses and ones sufficient for
reconstructing the archetype. It did not occur to him that many traditions of
Classical texts (those going back to a manuscript discovered in the Hu-
manistic age, copied or even reproduced in print by scribes or philologists
of that age, and then lost) consist entirely of "later subbranches," which
scholars must of necessity not "avoid adding," at least if they do not wish
to give up reading those texts; it did not occur to him to investigate to find
out whether such traditions too would turn out to be prevalently bipartite
or not. 67 Second (and this was observed by La Penna 1964: 374), even in-
vestigations of recent manuscripts not useful for reconstructing the archetyp-
type are interesting for the question of bipartite stemmas, in order to de-
determine whether the greater production of copies that began with the start of
the Humanist age in fact produced an abundance of stemmas with more
than two branches. 68

Now, Alberti draws attention to the fact that, among the few manuscript
traditions that remain multipartite even after his strict examination, almost
all "are represented by rather recent manuscripts"; 69 so too, though he has
not exhaustively studied the recent ramifications of traditions that also pre-
sent older manuscripts, he does mention the "lower part" of the stemma of
Aristotle's Metaphysics, in which Bernardinello's research indicates a fla-

tion of as many as nine branches, and that of Cicero's De legibus, in which
the extremely extensive stemma traced out by P. L. Schmidt presents "rami-
fications with eight, nine, eleven branches." 70

65. "Easier," as the reader will recall, because of the absence of the medieval "poor-
ity" of the manuscript tradition (cf. above, pp. 167–68).

66. It is superfluous to add that, in the case of traditions containing medieval manu-
scripts, Maas's contempt for "a hyperarchetype of no stemmatic importance," although
slightly moderated by his warning that editors can neglect them "often" (and not "al-
ways"), is excessive and reveals once again his prejudicial distrust for the recentiores (see
above, chap. 5, n. 34).


See also Finner 1972: stemmas in pp. 116–17, 120.
tions subarchetypes" or made it impossible to disentangle the manuscript traditions with absolute rigor.⁵⁰,⁵¹

"I still believe substantially that my observations in this Appendix C are correct. But it is my duty to indicate that the discussion will continue, and that in particular Michael Weitzman will go on in the near future to use statistical arguments (much more sophisticated than Mass's, as is only obvious) in order to argue that there is nothing strange in the preponderance of bipartite stenmas. On this point and on others objections will also be made against me by M. D. Rhee, with whom I have had a fruitful epistolary exchange, and to whom I am indebted for corrections and suggestions, as I have already indicated at the end of the preface (but, at least for now, we are not in agreement on everything). With regard to what I interpret as Mass's failure to understand Pasquale, I had written that Mass "understood nothing of Pasquale's work and perhaps did not even have the desire and the patience to read it" (chap. 3, n. 35). Prof. Rhee informs me that he possesses a copy of Pasquale's book densely annotated by Mass; hence that hypothesis of mine was entirely unfounded. I do not know the character of Mass's annotations; corrections of mistakes on the occasion of further examples besides the ones Pasquale cited? objections on principle? In any case, I hope that Rhee will publish them soon. Naturally the most interesting ones would be the objections on principle. The fact that Mass did not make them public remains strange. For the present, on the basis of his "Retrospect 1956" I remain convinced that "Mass understood nothing of Pasquale's work," even though I recognize that I expressed myself with excessive severity (in any case it should be obvious that with the word understood I meant, and mean, "knew how to evaluate the criteria of the work and Pasquale's concept of the history of traditions," certainly not understood in the literal sense). I know well that Mass was a great philologist. His best work, in my view, is to be found in the notes on individual passages of ancient authors now collected in Mass 1973 and in the contributions he made, with equal acumen and modesty, to many editions of the Biblioteca Oscanorum, of which he was an invaluable proofreader. His contribution to Greek metrics is also of great importance, even if the rigor of his formulations is too often an end in itself and is sometimes more apparent than real. For me, the Mass of the Textual Criticism is the weakest one. But, as is obvious, the discussion on this point too is anything but concluded.⁵²"

To me it seems necessary to make a distinction here. Filiations as rich as those with eight or eleven branches do indicate that the production of copies in the Humanist age increased so conspicuously that not even disturbances have succeeded in reducing them to those holy two copies, even though disturbances doubtless became more frequent in that age, given that Renaissance copyist-editors conjectured more often and more felicitously and collated more than medieval amanuenses had done.⁴⁹ So here Maas's reference to "much-read texts" becomes valid—for the very first time. But none of the multipartite manuscript traditions, "all recent," that Alberti scrutinized presents more than three or four branches⁵⁰ and in these cases, as Alberti himself recognizes (1979: 95 and n. 18), Maas's opposition between "poor" and "rich" traditions, once more, can have no validity, for the reason upon which we have already insisted too much (esp. p. 167). At least provisionally I would suggest a different explanation: these are traditions in which there has been a relatively brief interval of time, a relatively small number of intermediate links, between the archetype and the extant copies, so that although ceteris paribus disturbances acted with greater intensity in the Humanist age than in the Middle Ages, they ended up being a bit less intense than in traditions in which the archetype produced descendants for centuries and the first copies were already subject to extra-stemmatic contamination. But let us not forget that to these few examples of tripartite or quadripartite Humanist traditions other traditions can be opposed, bipartite ones (think only of Catullus) and above all highly contaminated ones (think only of the integri of De oratore, or of Tibullus). Fundamentally, if we disregard a few exceptions, not even the Humanist age represents a sudden break with regard to the multipartition of stenmas. Here too disturbances, and in the first instance contamination, have either created "ficti-

⁵⁰ Naturally, without wishing at all to cast doubt on the seriousness of Bernardinelli's work and, even more, of Schmid's, it would not be a bad idea to see whether the "nine" and "eleven branches" really are free of coincidences in innovation among smaller groups of manuscripts, due to contamination or other disturbances. (As Schmid himself points out to me, he does speak of contamination in the course of his book, 1974: 237, 345, 359, 386, etc.)

⁵¹ Poe for Corycian's Dialectes (or at least for one of them, studied by Perussi-Timpanaro 1956), four for Callimachus's Hymns, three for Gale's On Diagnosis from Dreams, three for most of Pausanias's Description of Greece, three for Sextus Empiricus's Outlines of Pyrrhonism, and three for Libanius's fifty-first oration (but where one of the manuscripts goes back to the tenth century). Alberti is inclined to hypothesize a multipartite stenmas (but apparently one with few branches) for Cicero's Brutus (Alberti 1979: 74–75). In other cases, ones that are more uncertain or are limited to brief texts, the stenmas would always have three or four branches.