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A BIPARTITE STEMMA OF TACITUS' *DIALOGUS DE ORATORIBUS* AND
SOME TRANSMITTED VARIANTS

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1. *Introduction.* So long as transmission of a text proceeded by handwritten copying, a split into two or three branches that emanate from a single copy was a frequent occurrence. Bipartition or tripartition of Latin texts is particularly familiar from the Italian Renaissance.

Partition occurs when a piece of writing is no longer passed on by single copy from single source, but more copies than one are made from that source to be copied in turn. Since the term 'archetype' continues to be used in several connotations,¹ I note that in this paper it is used to define the source where 'partition' first occurred. This process is described with great clarity on the first few pages of Paul Maas' *Textkritik* (1927 and later, E. T. 1958). Likewise the term 'hyparchetype' is here used for one or more of that offspring when it becomes itself first a parent of more progeny. The purpose of determining the features of partition and of archetypal or hyparchetypal readings is to determine what wording may be considered as transmitted, and how its source or sources may be defined.

The stemma here discussed is that of Tacitus' *Dialogus de oratoribus*, a work preserved only in 15th century manuscripts. By archetype as defined above I do not then here mean the celebrated codex Hersfeldensis on which all knowledge of the work as well as of *Agricola*, *Germania*, and Suetonius' fragmentary *De grammaticis et rhetoribus*, seems to have depended, but a late Renaissance copy. For this a case on palaeographical or textual grounds has been made by M. Winterbottom, not without doubts, 1972, 128 (see below, p. 132) and by C. E. Murgia 1977, 357 (see below p. 133 n. 5).


Bipartition was mooted as early as A. Michaelis' influential edition of 1868; the assumed branches of the stemma were named X and Y. A. Gudeman, on the other hand, envisaged a tripartition, and so did M. Winterbottom more recently in his fundamental paper of 1972 on

* The writer is grateful to two colleagues, Professor M. D. Reeve and Dr D. S. McKie, for helpful criticism of an earlier draft of this paper. Neither of them must be taken to agree with any specific views put forward by the writer.

¹ I note two very recent examples of a different nomenclature. D. Bo, 'Le principali problematiche del *Dialogus de oratoribus*' ... (*Spudasmata*, 51), Olms, Hildesheim ... 1993, 84, as well as H. Merklin, 'Dialogus' – Probleme in der neueren Forschung': in *Aufstieg und Niedergang d. röm. Welt*, vol. 33.3, 1991, 2271, use 'archetipo' – 'Archetypus' with reference to the Hersfeld codex. I offer no views here on the substance of Bo's and Merklin's contributions. The reason for that restraint is that in the present paper I am concerned only with the analysis of manuscript readings, and not with the evidence for the discovery of the *Dialogus* in the 15th century and with opinions on manuscripts presumed to be derived from that evidence.

the transmission of the work and in what I regard as the most competent recent edition (although I quarrel not infrequently with his text), the Oxford edition of 1975.² There is at any rate some agreement on what constitutes the basic differences between, on the one hand, the lost cod. ζ (to use Winterbottom's terminology) reconstructed from the extant manuscripts V and E, and, on the other hand, the Vat. lat. 1862, known as cod. B. In passing, however, I note the very different, indeed surprising, agreement among editors over the selection of manuscripts constituting the stemma. If that selection is rightly faulted in three respects, as pointed out in the note below,³ there is surely a good reason for discarding the old nomenclature, XY, altogether. But there is virtually no agreement on a group of manuscripts, analyzed further and subsumed in their entirety under the siglum Γ by Winterbottom; in this paper the group appears as lower-case γ — lower case, because capitals can usefully be restricted to draw attention to preserved and major manuscripts.

The γ group consists (again in Winterbottom's nomenclature) of codd. C and Q, as well as of several manuscripts described as *deteriores* by the same Oxford editor, and derived by him from a lost source ψ, whereas φ is his symbol for the '*consensus codicum Qψ*'.⁴ Gudeman and, with a clearer understanding of the manuscripts, Winterbottom placed γ intermediate

between ζ and B, thus  B γ ζ, a tripartite stemma.

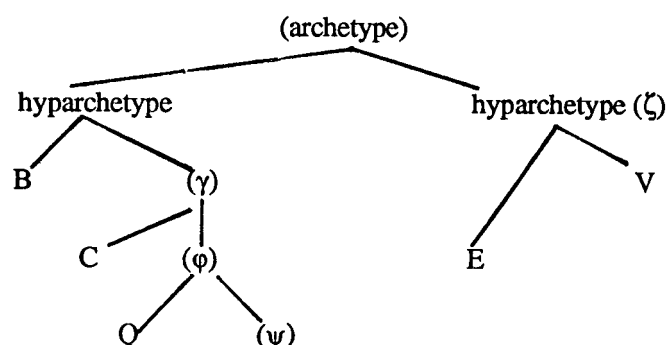
This case for tripartition was contradicted by C. E. Murgia in a strongly-argued review article on the Oxford text.⁵ Murgia recommended a bipartite classification on the basis, admittedly narrow, of the manuscripts' evidence offered in that text, thus⁶ —

² Gudeman, ed. 2, Leipzig – Berlin; Gudeman's tripartition is accepted also by A. Michel, Paris, 1962. For Winterbottom's new departure, see 'The transmission of Tacitus' Dialogus', *Philol.* 116 (1972), 114–28, and his edition in Tacitus' *Opuscula*, Oxford 1975, reprinted 1980.

³ This applies not only to older texts of this century that are still in print or have been reprinted, but even to work of the last twenty or so years, such as D. Bo's Paravia edition of 1974, and H. Heubner's Teubner of 1983, though not to Bo's latest text of 1993, cited above n. 1 (I have not seen his intermediate edition, Turin 1986, in the *Corsi Universitari*). Both scholars (before Bo 1993) continue the unreal combination of a mere copy (the once celebrated cod. Pontani, or b Leidensis Perizonianus XVIII Q 21) with its source (directly or indirectly) cod. B, to build up a 'class', X. Both likewise (again with the exception of Bo 1993) use the once equally celebrated Farnesianus, or c, (Neapol. IV C 21) and cod. Δ (Vat. lat. 4498) to increase the bulk of 'class Y'. These two codices are interpolated and, like b, have been rightly eliminated by Winterbottom 1972, 115–16, for b, and 122, for c and Δ. In this respect Winterbottom had been partly anticipated by earlier scholars, but is not followed far enough by more recent ones. Although it is good to see that Bo 1993 declares himself convinced by Winterbottom's assessment of these codices, important features of the γ tradition remain at issue and are discussed further in the text above.

⁴ For φ see Winterbottom 1972, 123, and Oxford text 1975. We need to note, however, that φ is more indistinct than it might be, since the readings of one of its constituents, ψ, are frequently uncertain. Yet one cannot doubt the close link of Q and ψ. For they tend to stand together against all, in such telling instances as 35.1 in *sc(a)ena*, a conjecture corrupting further the already corrupt text as transmitted: *se in*, or *seni (sem)*.

⁵ Murgia, in his review article on the Oxford text, 'The minor works of Tacitus: a study in textual criticism', *CP* 72 (1977), 323–43, especially 336–8, and 'Notes on the Dialogus of Tacitus' *CP* 74 (1979), 245–49. A more extensive discussion of textual problems was published by the same scholar in the *California Studies in Classical Antiquity* 11 (1979), 159–78. The first of these papers is here referred to as Murgia 1977,



Murgia's arguments as well as those of others, especially R. P. Robinson's earlier case for bipartition — not to mention his own doubts — caused the Oxford editor to leave the matter largely in abeyance.⁷

That strikes me as insufficient. More can be learned from a first-hand study of the manuscripts' evidence — material that was not known to Murgia, and although known to Winterbottom, seems to me inherently to refute his case for tripartition. The suggestion that the γ group of manuscripts shares an hyparchetype with B (which I call β) is not restricted to the three instances by which Murgia sought to demonstrate it (see below pp. 138-139).

In the present paper therefore I offer a new study of the manuscript material.⁸ I attempt to cast fresh light on two major problems arising in this area. In the first place, by presenting a full analysis on the γ group, especially of its chief manuscript, C, I seek to confirm the link of this group with B. This in turn eliminates the claim made for B solely to represent an hyparchetype, cod. b having been eliminated already, since its text depends on B.

In the second place the linking of γ and B under a single hyparchetype raises afresh the problem of transmitted variants — if, that is, we tend (with Winterbottom 1972, 119) to reject large-scale contamination in the upper reaches of the stemma.⁹ *Dial.* 12.1 presents an

the other two as Murgia 1979 (1) and 1979 (2). Reference is made also to his paper in *CSCA* 12 (1981), 221–40 on the length of the lacuna.

⁶ Murgia 1977, 337; 1979 (1), 245.

⁷ Winterbottom in *Texts and Transmission* etc., Oxford 1983, 410–11.

⁸ The paper is based on a fresh collation of the microfilms of the five main manuscripts, V, E, B, C, Q (in Winterbottom's nomenclature) and Wissowa's facsimile of b. At present I am engaged on collating the secondary material, especially Winterbottom's ψ manuscripts, and the early editions. I hope then to consider whether on this basis a renewed study of the codicological evidence, including the discovery of the *Hersfeldensis*, would be worthwhile.

⁹ Whilst accepting Winterbottom's suggestion we are as yet left with the thorny question how far we can reasonably extend the uncontaminated area. Thus the corrections in cod. E present a well-known and still unsolved problem, see e.g. Murgia's comments in *CSCA* 12 (1981), 234–6. Moreover, although Murgia tended to accept Winterbottom's suggestion of an upper area in which one could reckon with transmitted variants (Murgia 1977, 337–8), he raised doubts (ib. 337 and again 1979 (1), 245 n. 3) as to the purity of the hypothetical source common to B γ , and (1979 ib.) of the parent codex of Q ψ , Winterbottom's ϕ (above n. 4). As for Murgia's criterion (the small number of omissions in the higher range, seemingly too small if contamination is to be excluded), see below in nn. 39 and 42. Contamination had already been dismissed, though in somewhat simplistic fashion, by Gudeman, ed. 2, 1914, 125–32.

instructive example. The true reading, *secedit*, is restricted to γ and B^V , whereas the error (by simple haplography *se – ce*) *sedid* is in the rest of the transmission, ζB . On the above assumption, in this case, and other similar cases, we need not allege contamination of γ by B^V or, conversely, of B^V by γ , but rather allow the variant in B to suggest an higher origin for the double reading, as indeed Winterbottom (1972, 117) remarked. There would have been two readings in the transmission, to be traced back to one of the hyparchetypes — in this case γ to β — or upon occasion, when readings are appropriately distributed, be traced back to the archetype. A number of these transmitted variants I believe to be demonstrable, and the second part of this paper will therefore offer a discussion of such variants, hyparchetypal or archetypal. In his remark on contamination (reported above from 1972, 119) Winterbottom warned that this phenomenon, at least in theory, might occur undetected. That is a proper caution to observe. Nevertheless the practical contingencies of this particular transmission suggest that he was right to reject a large-scale contamination in the upper region of the stemma. In some cases, as in the conflations of cod. C,¹⁰ and in the three instances of Murgia's I have discussed,¹¹ my reading of the evidence leads me to exclude the practicability of contamination. In other cases contamination would not be impracticable, but the evidence is best explained without that assumption, as would often have been the case in the *Agricola*, if the variants had not actually shown to be transmitted on the old leaves of the Cod. Aesinas. These results, though arrived at by different routes as it were, support each other.

2. *Codex C*. What he considered the sheer incompetence of cod. C was most strongly expressed by A. Michaelis.¹² He said, 'neglegentissime et tamquam per somnum scriptus est'. This is picturesque language. It invites repetition and, almost inevitably, the invitation has been accepted any number of times. Yet while the scribe was inattentive more often than not, and very stupid into the bargain, he must have been sufficiently awake at times to recognize what his exemplar offered when no other copyist of the same exemplar did; indeed in a few places, no other manuscript is known to have the better C reading, however obvious the reading may now seem. That clearly is the case, even though occasionally deteriores (ψ), or c , or Δ , seem to obtain the same correct answer from borrowed resources, if not conjecture. I have here noted such additional occurrences in ψ but not in c or Δ for reasons touched on above.¹³

Thus C, without visible corrections, offers the following:

¹⁰ Below pp. 135-137.

¹¹ Below pp. 138-139.

¹² His edition of 1868 is mentioned above p. 131. The remark cited in the text occurs at p. XIII of his edition.

¹³ Cf. above p 132. On the other hand Bo 1993, p. 94 (§1), fails, I believe, to make a convincing case for his assumption that cod. C is the parent of the deteriores. Winterbottom 1972, 123, seems to me sound in denying that ϕ (let alone ψ) is a descendant of C. Bo stresses the similarity between Q and B in indicating the lacuna at ch. 35 as a fact supporting his view of C. But that limited similarity can be accounted for in other ways.

- 13.4 *aliquid*; grammar requires the object: *aliqui* cett.
- 17.2 alone (except for interpolated codd., which are here, as at 17.3, 37.3, 37.4, 40.3, disregarded) leaves lacuna for indication of month; <*Decembres*> add. Lipsius.
- 17.3 correctly *uiginti* instead of *decem*, cett.; that is, the number 120 here appears as C&XX. At 24.3 however all manuscripts have the correct number, oddly with the exception of this codex, which, with more usual negligence, offers C&X.
- 23.6 (*sententiarum*) *plenitas* against *planitas* cett., neither of which may be right, though the C reading is preferable in sense.
- 32.4 *detrudunt*: *-dant* cett. The fault may be due to wrong sentence division or simply confusion of *u* and *a*.
- 33.3 Although at 33.2, line 5 (Oxford text), C corrupts *inscientiae* to *scientiae* like all other manuscripts, at 33.3, line 11, C seems rightly to preserve *scientia* against most others, some ψ codd. apart.
- 37.3 (+ ψ) *metellos et metellos sed et* cett.
- 37.4 *de expilatis*: om. *de* cett.
- 40.3 *accepimus*: *accip-* cett.
ne ... quidem: *nec ... quidem* cett.

All these are minor, indeed minimal, examples. Some may be conjectures, accidental or deliberate. In most cases the scribe is likely to have recognized the lettering of his exemplar, and to that small extent did better than his usually more competent colleagues. It requires of course no great mind to correct minor faults of that kind. But that is not the point. I mention these examples in order to improve on Michaelis' impressionistic description 'per somnum'. Even this inattentive and stupid scribe alone got these few minor cases right when better scribes did not. In contrast I count approximately 160 cases where C alone has very ordinary untruths.

The most characteristic features of C, however, is conflation. This is characteristic in the oddity of its results, though not very frequent nor everywhere equally marked; I have noted no more than ten cases on the 42 pages of the Oxford text. Of these I regard three to four as certain or near-certain, and the remaining six to seven as probable. Conflation of double readings — whether transmitted variants, conjectures, or explanatory glosses — is a widespread phenomenon, familiar from many manuscripts, Greek or Latin, and frequently discussed. For that reason I refrain from exemplifying.¹⁴ What appears to be less usual, however, is the curious treatment which C applies to these variants. The scribe seems concerned not to present them as such, as many of his colleagues might have done, either in

¹⁴ I add however that larger explanatory glosses, if there were any, are not in the *Dialogus*, among the material picked up by the C scribe. Moreover, whereas in the present case the conflatory procedure extends selectively over the whole work, elsewhere such procedures are sometimes restricted to part of a work only. For example, at Liv. VII ch. 3 ff., the material that is picked up there, and then conflated with the text, consists of explanatory glosses; its extent is limited to about one half of the book and reappears very occasionally later. Cf. Madvig, *Emend. Liv.*, 2nd ed., 22–3; for a list of instances see Conway and Walters (edd.) books VI–X, Pref. pp. vii–viii.

the margin or between the lines of the manuscript they were writing or correcting — thus, in this transmission, B offered the interlinear¹⁵ variants called B^v by Winterbottom — but almost systematically, to conflate them, often to bizarre effect. In such passages C seems to be writing and annotating the text not on the assumption that one or the other of two competing readings was correct — which would normally be the variant noted — but that both, conflated, were.

The following cases of conflation may now be considered; C's conflated (or probably conflated) version will always be found at the end. It is important again to stress that, with one possible exception,¹⁶ all instances are uncorrected. These are no second thoughts engendered by an additional source; they are what the scribe found in his exemplar on copying it.

As regards degrees of certainty, I consider the following cases of conflation as certain:

12.4 *uelis* B ϕ : *uel* ζ : *uelis uel* C

25.2 *illos ... illorum* ζ (as in nearly all cases of a choice between *ille* and *iste*) ϕ recte: *istos ... istorum* B (as in nearly all cases of a choice between *ille* and *iste*): *illos ... istorum* C

39.3 *quam* ζ B^v (<*ante*>*quam* scr. Güngerich): *quando* B ϕ : *quam quando* C

41.2 *quis enim* ζ recte: *quis enim quod nemo* B^vQ: *quidem quod nemo* B ψ (*quid enim quod nemo* b): *quis enim quidem quod nemo* C

The next case is, I think, near-certain:

35.3 *adulescentulos omnes* codd. exc. C recte: *adulescentesulos* C (i.e. *adulescentes + -ulos*. Although the grotesque reading may be due to accidental omission of dots under *-es* to delete the two letters, deliberate conflation by the scribe who wrote *quam quando* and *quis enim quidem quod nemo* is surely more likely).

The remaining cases of conflation strike me as probable although alternative explanations may be argued. I do not propose here to go into details.

28.4 *educabatur ... erat* $\zeta\phi$ recte: *educabitur ... erit* B: *educabatur ... erit* C (probably conflates the tenses)

29.1 *et uirides* B (text uncertain, *et* certainly wrong): *et uides* $\zeta\phi$: *et uires* C (probably an unconsidered conflation of *et uir-* and *et uid-*)

30.2 *uocant* ζ B^v ϕ recte: *uocatis* B: *uocantis* C (probably an unconsidered conflation of *-cant-* and *-catis*)

30.4 *cognouerat* ζ B recte: *cognouit* ϕ : *cognouerit* C (may conflate *-uerat* and *-uit*)

37.5 *habendus* ζ ?recte: *habendus est* B ϕ : *est habendus* C (a sort of conflation of *habendus* and a probably interpolated and floating *est*)

¹⁵ As far as I can make out in my microfilm of B, all variants are interlinear. At 2.1 the true (non- ζ) wording, *semote dictionis* is found, uncorrected, in the text, and in the margin *semote dictionis* is repeated.

¹⁶ A possible exception is found at 37.5, where at any rate the microfilm gives the impression that *est* may have been added at the end of the line.

If this is so, it should be asked — how did the C scribe come by the readings which he then conflated, combining or altering them? Hardly by contamination, though one will have to admit, as Winterbottom 1972, 119, reminded his readers,¹⁷ that theoretically this possibility cannot be excluded. But theory is one thing, the actual behaviour of scribes is another. If it is not contamination that explains the behaviour of the C scribe, an alternative rationale, briefly suggested by others,¹⁸ needs to be argued further: he used variants which he found in his exemplar. Of the instances of conflation in C some have already been regarded as archetypal variants.¹⁹ Although their archetypal character needs further consideration, I too think that they were most likely in the transmission that preceded the exemplar of C. This, I believe, applies to 12.4 *uelis uel*, 30.2 *uocantis*, 39.3 *quam quando*, perhaps others. It particularly applies to the last of those conflated passages, 41.2 *quis enim quidem quod nemo*, where C conflates all the several readings found in the whole of the transmission — a procedure best explained by the hypothesis of transmitted variants.

From a reasoning that can sustain the hypothetical variants common to γ and B it would follow that the number of variants in the putative source β will have been larger than now appears from the only major manuscript that carries such variants, namely B. This is not for us a strange conclusion to draw, if we recall the many variants in the Hersfeld leaves of the *Agricola* preserved in the codex Aesinas,²⁰ and again the considerable number of variants that will lie behind the scatter of readings in the *Germania*.²¹ Now, this state of affairs cannot simply be postulated for the *Dialogus* just because it applies to extant evidence in the *Agricola*, and to reconstructed evidence in the *Germania*.²² That evidence, however, would provide valuable corroboration if we can now claim a reasonable case for inherited variants in this transmission, quite apart from B^V.²³

¹⁷ Cf. Murgia 1977, 337.

¹⁸ Thus Winterbottom, in a different context, 1972, 11, *secedit* B^V γ : *sedit* cett., cited below p. 145. For Murgia's view on variants in this transmission, see next note.

¹⁹ E. g. Murgia 1977, 337–8, on a case like 12.4 *uelis uel*.

²⁰ I put it that way because I am not convinced by D. Schap's exposition, *CP* 74 (1979), 28–42, against the provenance of the old part of the Aesinas *Agricola* from the codex Hersfeldensis, recently favoured also by H. Merklin (cit. above n. 1), pp. 2265–70. But acceptance of the coincidences denied by Schaps seems to me to involve fewer assumptions than does the multiplying of Carolingian copies of *Agricola* to bear out his own conclusion. For a detailed disproof I refer to C. E. Murgia and R. H. Rodgers, *CP* 79 (1984), 145–53; cf. also R. Häussler, *Philol.* 130 (1986), 90, n. 63.

²¹ For double readings in the *Germania*, see Robinson's edition 1935, p. 382 under 'doublets'.

²² Murgia 1977, 336, has already drawn attention to the smaller number of 'doublets' in the *Dialogus* in comparison with *Germania* and of course *Agricola*.

²³ The evidence does not encourage the guess that C had access to B and B^V. Wrong conclusions, therefore, should not be drawn from what seems to be the sole instance of B^V coinciding with C alone, disregarding interpolated codices as well as the conjunction of B^V with Q (e. g. 38.1), or of B^V with CQ (ψ), which is in fact γ . The passage occurs at 27.1, which will be discussed more fully below in the context of transmitted variants. A case, I believe, may be made for the view that the two main competing readings — *aparte* B^VC and *apparate* BQ — will have been variants in hyparchetype β , if not already in the archetype. Such a case is put forward below at p. 147–148.

3. *From C to γ and from γ to B and β .* So far, then, we have seen that the distribution of readings, and conflation of variants (some very probably transmitted), in C suggests manuscript material shared with B, and therefore renders location of γ , together with B, under an hyparchetype β , an arguable proposition. This result covers only a limited amount of evidence. It is borne out, however, by some observations in Murgia's review article of 1977.²⁴

It is on the γ side of the picture that Murgia's results seem to contribute to my argument. In his context he is particularly concerned with three passages which are directly relevant here: 11.1, 10.8, 21.4.

As regards all three passages a convincing case is made by him for the proposition that γ and B share vital manuscript evidence, or, to put it differently, that their readings are to be subsumed under the same hyparchetype β . In the first two cases conjectures are shared by B and γ : an odd guess at 11.1 *parant quid enim*,²⁵ and a grammatical adaptation at 10.8 *expressis*.²⁶ The competing explanation — that these guesses should be thought to have been made independently by each γ and B — need only be stated to be dismissed.

The third passage, 21.4, is the most interesting. It has been explained for the first time by Murgia,²⁷ and in my view explained convincingly. The pronoun, *illae*, together with the severe stylistic comment, *sordes autem ... uerborum*, make surely a convincing form of words; *illae* occurred, probably abbreviated as *i^e*, in ζ , which many older, and some recent, editors have printed. Others have tried to make use of the competing *regulae*, and emend it — with conspicuous lack of success. But quite apart from emendation, what is the route from *ill(a)e* to *regul(a)e*? The route leads through two Renaissance symbols; as Murgia pointed out ingeniously — viz. *i^e* and *r^e* — or rather through a confusion of them.²⁸

We conclude then that the distribution of readings and conflation of variants in C make for the same conclusion as that reached in Murgia's three strong instances. The suggestion is this:

²⁴ See above p. 132-133, with particular reference to Murgia 1977, 336-7.

²⁵ *Dial.* 11.1 *parant quid enim*, $\gamma B = \beta$, spoils further the ζ reading, *parant enim quid E (...quid V)*, which is itself a corruption of *parantem inquit* (excellently emended by G. H. Walther). Murgia discusses the case l. c. 336.

²⁶ *Dial.* 10.8 *expressit* ζ is probably an interpolation (del. C. A. Heumann, and so bracketed in the Oxford text), though still not convincingly explained. Murgia l. c. 337, makes a reasonable case for the assumption that the ζ reading, though a corruption, is primary, whereas β *expressis* seeks to adapt that reading to the grammatical context, but fails to make sense.

²⁷ Murgia l. c. 337 on *Dial.* 21.4 *sordes autem ill(a)e* (ζ) but *sordes autem regul(a)e* (BC, re... Q, sim. ψ) *uerborum et hians compositio* etc.

²⁸ Murgia, *ibid.*, quotes no instances from manuscripts in which *r^e* abbreviates *regul(a)e* but refers to the 1901 edition of A. Cappelli's *Lexicon Abbreviatarum*, p. 151 for *i^e = ill(a)e*, and p. 292 for *r^e = regul(a)e* (pp. 177 and 323 in 4th and later editions). There is little difficulty in the case of *illae*, although a full shortening of *illae* as presumed for *illud, istud, id* does not seem to be mentioned there. As for *regulae* the symbol *r^e*, or rather *re^e*, is ascribed by Cappelli to the 15th century, but he hardly ever cites individual cases. Several colleagues, however, have drawn my attention to A. P. M. J. Pelzer's Supplement to Cappelli, Louvain 1964, where in fact a single example from a non-administrative text is given at p. 70, *r^{is} = regulis*, cod. Vat. lat. 951 (15th century), f. 98. This is something to bear out the above conclusion; but more examples would be welcome.

we are dealing with manuscript evidence that is shared between C, or γ as a whole (γ in Murgia's three instances), and B. The shared source of B γ I have called β . On the basis of the instances so far surveyed I regard β as the single hyparchetype on the non- ζ side of the stemma. None of the above C (γ) cases, or of Murgia's triad, call for tripartition. They all are fully explained by a twofold stemma, and in some important cases actually exclude tripartition. The spread of readings in relation to the link between C (γ) and B also raises afresh the problem of archetypal and hyparchetypal variants, to which I adverted earlier in this paper (pp. 133-134).

4. *Further significant errors in B vs ζ .* Out of a larger number of cases which are here irrelevant I select seven where significant errors occur in β . They are the following:

1.3	<i>diuersas quidem causas</i> ζ (V, ... <i>dem</i> E, for explanation see below) recte:	<i>diuersas uel easdem causas</i> B γ ²⁹
2.2	<i>omni</i> ζ (+ N) recte:	<i>cum</i> B γ
16.7	<i>uester</i> ζ (+ ψ) recte:	<i>uidetur</i> B γ
17.4	<i>ei</i> ζ recte:	<i>et</i> B γ
19.2	<i>laudi dabatur</i> ζ ?recte:	<i>laudabat</i> B γ
23.4	<i>prope est ab</i> ζ ?recte:	<i>prope abest ab</i> B γ
27.3	<i>et</i> ζ recte:	om. B γ

The first two passages stand out. The fact that at 1.3 cod. E leaves a space before ...*dem* makes it hard to decide whether V, in writing *quidem*, conjectured the first syllable, or rather recognized *qui* in his exemplar but found it indistinct or in some way tampered with. In any event, the lacuna in E combined with the foolish interpolation in β suggests damage or loss of what precedes ...*dem*. Like Murgia, however, I find it hard to think of anything that fits the case as well as *quidem*. I agree with him also that caution is indicated as to where and how the β text, *uel easdem*, originated. The choice, however, is only twofold. Either we face a foolish interpolation in hyparchetype β , on the cause for which one can argue. Or else we must assume a variant transmitted in the archetype, rightly rejected by V (though leaving an unresolved difficulty in E) but continued without visible doubt in the β codices. A decision would require a more extensive argument than is appropriate here. What matters to my present contention is, (1) the β codices show a single reading and no possible contamination by the ζ (V) text; (2) there is certainly a case for an hyparchetypal conjecture, even if an archetypal variant is hard to establish.

Correct phrasing, *omni eruditione imbutus*, is again offered by ζ in the second passage, 2.2, whereas B γ have the obviously erroneous *cum eruditione imbutus*. Clearly here two

²⁹ Many editors either keep *uel easdem* or imply by obelizing the words that emendation is required; the latter e. g. in Winterbottom's Oxford text. I am persuaded by Muretus' and Lipsius' deletion of *uel easdem*, but also by the retention of ζ 's *quidem*, cf. Peterson ed. 1893; Helmreich, *Burs. Jb.* 72 (1893), 136; Gudeman ed. 2, 1914, app. cr.; Bo ed. 1974, and especially Murgia 1977, 341; 1979 (2), 172.

words or symbols have been confused: $\bar{o}i$ or $o\bar{i}$, meaning *omni* (as found in ζ and as usual in 15th c. script), and $\bar{c}u$ or $c\bar{u}$. This provides a sufficient explanation; but unlike an earlier case, *illae* and *regulae*,³⁰ it is not the only convincing explanation. Symbols for *omnis* and *cum* were current since the eighth century, as Lindsay has established.³¹ In fact because of their age the symbols could suggest a much older corruption. Again, however, this is not the place for discussing variants. All that matters here is an obvious error in idiom which could have been corrected by recourse to a ζ source — but was not. No contamination therefore in the unitary β text, although it would have been tempting had the chance arisen.

Similar considerations apply to the other passages. At 16.7 (passage 3 above) a reasonable case may be made for *uester* in place of [*uidetur*] β (*ur* is common as a symbol for both words) just as at 23.4 there is a reasonable case for *est ab* in place of *abest ab*.³² At 17.4 *et* β is a scribal error for *ei* ζ . 27.3 *et* ζ is omitted by β . 17.2 *laudi dabatur* – *laudabat* seems to me another open case in addition to 16.7 *uester* and 23.4 *est ab* just mentioned.

Since the possibility of contamination of β by ζ has been considered but rejected in specific cases above, it should be noted that the same applies here.

5. ζB vs γ . After stating in my last section the conspicuous lack of influence from ζ on the upper range of the β tradition, we may now search for possible positive evidence, in β , of ζ readings. Such readings would obviously throw doubt on the integrity of the β tradition. In this section I therefore enquire about ζ readings of cod. B, in the next about the combination $\zeta\gamma$.

As for ζB , then, I have noticed a dozen cases where that combination stands against γ . These however need only be mentioned to be dismissed, amounting, as they do, to no more than small slips in γ . They tell us something about the lowish quality of γ , but are not otherwise of interest.³³

6. $\zeta\gamma$ vs B. Next the question arises whether the combination of ζ and γ together confronting B can be explained, most economically, on the assumption which so far in this paper has proved successful: B and γ descend from a common hyparchetype, β , their divergences being due, not to contamination, but either to transmitted variants, or to individual errors of the kind just considered in section 5 for γ . I have noted some 45 passages in which ζ and γ together stand against B — not an inconsiderable number. But quantity is

³⁰ Above p. 138.

³¹ W. M. Lindsay, *Notae Latinae*, 160–174 on *omnis*; 41–43 on *cum*; 43 specially on *cū*.

³² Thus at 16.7 e. g. the editions of Nipperdey, Michaelis, Baehrens, Gudeman (doubtfully), Goelzer (Budé ed., here without a note), Michel, Bo, and the instructive discussion by Murgia, 1979 (2), 175–6. For 23.4 *est ab*, see Murgia 1979 (1), 246–7.

³³ 12.2 *hoc* ζB recte: *h(a)ec* γ ; 14.4 *plurium* ζB recte: *-imum* γ ; 18.4 *uideretur* ζB recte: *uidetur* γ ; 19.2 *cum condicione* ζB recte: *condicione* γ ; 19.4 *paucissimi* ζB recte: *-ma* γ ; 20.1 *infirmirate* ζB recte: *infinite* γ (*infinite* C); 23.4 *prope* ζB recte: *proprie* γ ; 26.6 *plurisque* ζB recte: *plerisque* γ ; 32.4 *domina* ζB recte (i. e. *dñā*): *dicunt* γ (i. e. *dñā*); 33.2 *ingressuri* ζB recte: *ingressi* γ ; 37.5 *in* ζB recte: *om.* γ .

not all; the kind of divergence must be noted as well. Virtually all B readings that differ from ζγ do so for the worse.³⁴ They do not really differ in quality from the instances just scrutinized in section 5, though there are more than four times as many. They amount to no more than small slips in B. To put it differently, it is not ζ and γ that combined call for comment — they just continue inherited and correct readings which happen to coincide in such cases. It is B, the putative congener of γ, that stumbles so often. If the quality of γ was rightly described as low in the preceding section, that of cod. B is no higher. But, moreover, it will be seen that B is idiosyncratic as well: *meliusculum feci*, as he says about his copy in the *subscriptio*, is hardly a fair assessment.

One group of instances derives from misreadings of letters which then lead to erroneous conjectures. In each case, as spelt out in the initial example, the truth in ζγ (or, 23.3, closer approximation) is placed first, the error (or, 23.3, greater error) in B comes second.

5.1	<i>modesti</i> ζγ recte:	<i>moderati</i> B
10.4	<i>elegorum</i> (but <i>-arum</i> V):	<i>elegiorum</i>
18.6	<i>ueniam</i> :	<i>uenias</i>
19.5	<i>et</i> :	<i>aut</i>
(23.3)	<i>non firmitate</i> corr. Acidal.:	
	<i>infirmitatem</i> ζγ:	<i>infirmitatemque</i> B)
26.6	<i>certe</i> :	<i>certo</i>
26.8	<i>deminuta</i> :	<i>di-</i> (+ E, aliquot ψ)
29.3	<i>inuenies</i> :	<i>inuenires</i>
36.1	<i>cogitare</i> :	<i>cogitant</i>
40.4	<i>bene</i> :	<i>bone</i> (+ ψ)

ζγ deal competently with symbols (or likely symbols) whereas B mishandles them:³⁵

6.4	<i>quemcumque</i> ζγ recte (exc. <i>quacumque</i> ψ):	<i>quandocumque</i> B
7.4	<i>nomina</i> :	<i>non</i>
21.9	<i>non</i> :	<i>nec</i>
26.5	<i>uelut</i> :	<i>uult</i> (+ aliquot ψ)
28.1	<i>et</i> (exc. aliquot ψ):	<i>qui</i> ³⁶
32.3	<i>non</i> :	<i>nec</i>

³⁴ Exceptions where B scores against ζγ are few and far between in number and amount to little in quality. Thus 6.2 *non ... non ... non* B recte: *non ... non ... neque* ζγ; 22.2 (orthogr.) *iis* B recte (sim. *alibi*): *hi(i)s* ζγ; and a trifle more importantly, 24.2 *nostris* B recte: *uestris* ζγ; 27.2 *perstringet* Lipsius probably right, *perstringat* B is less likely, but not impossible: *perstringit* ζγ (though *perstrigit* C omits the *n* symbol); 32.5 *ergo* B recte: *ego* ζγ. I omit a few cases where readings in either ζ or γ are not clear-cut. But even so the specimens of virtue in B are minute.

³⁵ This category may possibly apply also to a well-known crux, 11.2, where a textual problem is still unresolved: *in nerone* ζγ could be closer to the original reading than *in neronem* B.

³⁶ *et* often appears as 'ampersand' 'ε' or '&' or the like, e. g. in cod. C in this passage. It will have been this symbol that was mistaken for *Q* by the scribe of B, hence *qui* — an instructive case.

33.5	<i>perceperis:</i>	<i>percipis</i>
34.7	<i>hodieque:</i>	<i>hodie quoque</i>
35.1	<i>crasso:</i>	<i>claudio</i> (B pr.)
36.1	<i>nihil (2):</i>	<i>uel</i>

Erroneous separation of words is noted at 35.4 *per fidem* ζγ recte: *perfidie* B (+ aliquot ψ): *perfide* aliquot ψ; 26.4 *posse* – *post se* is similar, but a little more complex.³⁷ Standard types of accidental omission occur 24.3: B omits *in* before *tantum*, 28.6 omits *rem* before *militarem*, 31.2 omits *ipsa* before *in*, and omits *in* before *uitiis*, 32.5 writes *ego* for *ergo*, 39.1 *ridear* for *rideatur* (-entur Q). Against the correct order in ζγ, B reverses words 22.1: *etatis eiusdem*, 22.2 *iam senior*. And B's quirk of choosing *iste* instead of *ille* has attracted some comment.

This evidence should put a different complexion on the numerical preponderance of departures by B from the ζγ norm. The examples now surveyed show that, as was argued at the beginning of this section, there is some point in attending not just to the number of divergences between ζγ and B but to the kind of divergences. B is not any less likely to derive, together with γ, from the putative hyparchetype β because readings diverge quite often. The common kind of errors committed by the scribe of B, and some quirks such as his idiosyncratic prejudice in favour of *iste*, provide explanation. Each of the individual passages listed above needs to be considered on its own merits. In many cases a simple slip by B will have to be stated. In others the network of readings will help us to recover a variant which hyparchetype β either transmits from the archetype or, occasionally, adds by conjecture to the stock. But, in collating the microfilms, I have noticed no case where B's vagaries exclude derivation from β in common with γ.

To complete the record, I merely mention a few more cases whose somewhat greater complexity would demand more discussion than is apposite here. At 26.4 the verb *posse* is required, and so ζγ offer; but Q² miscorrects to the nonsensical *post se*, which is the B reading. 26.2 is more complex still: ζ and the main structure of γ, namely CQ, rightly put *actores*, 'pleaders'; almost certainly, therefore, that was the γ reading; but again Q² miscorrects to *auctores*, which is in B but here also in ψ, the γ *deteriores*. Q² cases, though few in number, have to be discussed on their own.³⁸ Contamination cannot be excluded either for ψ or Q², but in view of relationships on this side of the stemma, a variant in our putative hyparchetype β should be considered in both passages. The suspicion of variants arises also at 30.2 *habuerit* ζC recte: *habuit* Bφ,³⁹ and 31.5 *permouendos* ζφ recte:

³⁷ Below on this page with n. 38.

³⁸ The evidence is narrow. Q² and B, all others excluded, seem to combine in these two passages only. In any case corrections in Q are rare. In the microfilm I have noticed no more than four further instances, all duly marked in the Oxford text: 20.4 ζCQ² falso, 23.5 BCQ²ψ recte, 25.1 BCQ² recte, and 30.5 ζBQ² recte.

³⁹ A variant caused by the omission of an *er* symbol seems to me likely. Interpolation in φ, the source of Qψ, is rejected by Winterbottom 1972, 123, but considered probable by Murgia (on, to me, dubious grounds) 1979 (1), 245 n. 3. For a similar disagreement as regards γ, see below n. 42.

promouendos B: *com-* C.⁴⁰ A celebrated crux belongs to the category here under discussion but has its own difficulties: 38.2 *sicut omnia alia pacauerat*. If that form of the text is correct, as I hope to show elsewhere, the manuscript evidence will point to a variant in this hypothetically bipartite transmission.⁴¹ It is hoped that a sufficient number of convincing cases has now been presented to demonstrate the connexion of B and γ . I have also sought to present some new evidence in favour of Murgia's notion that B and γ derive from a shared hyparchetype which I call β .⁴² A diagram of the kind of stemma here envisaged has been drawn above. If it has been correctly drawn, the stemmatic links thus established should now guide discussion of the second of my subjects — the likelihood of transmitted variants as an alternative to contamination in this respect.⁴³ I put it that way because this analysis must not be taken to deny the possibility of contamination in other respects. Each time a case for or against will have to be made.

7. *Variants in codex B (B^v)*. The only one of the known major manuscripts of the *Dialogus* that contains a sizeable number of variants is the Vaticanus lat. 1862 or B. This codex plays a large part in the non- ζ area of the stemma; my earlier argument in this paper derives B together with γ from an hyparchetype which I have called β . B is undated, but will come early in the transmission that seems to have started in 1455. If, as I believe with Winterbottom 1972, 115–16, and others, the codex Pontani, Leidensis b, is derived from it (though perhaps at a remove), some time before (lower-case) b will have to be allowed for (capital) B, which however cannot have been long, since the Leidensis b, the earliest dated codex of the *Dialogus*, is itself expressly dated to 1460.

The B^v notes could, but need not necessarily, be the scribe's own. The handwriting does not seem to me to give a clear lead. More importantly, however, the character of the B^v evidence suggests the strong likelihood that these are early notes in close connexion with the β - γ context. A comparison with the most interesting of annotated, if secondary, codices — I

⁴⁰ The muddle between the prefixes, *per- pro- con-*, seems caused by earlier damage done to the beginning of the word and a subsequent variant or variants to cover up the fault.

⁴¹ To indicate what I have in mind, I note without much argument that the manuscripts have *sicut omnia alia pacauerat* V γ (*sicut alia omnia pacauerat* E) but *sicut omnia depacauerat* B (the verb an hapax): The split in the reading is likely to have arisen from an accidental omission of *alia*, though added as a gloss leaving doubt concerning its position; hence *omnia alia* V γ recte and *alia omnia* E. For *ali(a)* into (*di-*) *de*, see Gudeman ed. 2, ad 1., hence *omnia alia pacauerat* made into *omnia depacauerat*.

⁴² Murgia, however, in spite of making an excellent case for such an hyparchetype, does not exclude the suspicion of contamination. He notes (1979) (1), 245 n. 3) that C, Q, and the other codices have many omissions; yet γ 'omitted nothing but one monosyllable, 37, 5.5 *in*'. The evidence however is not quite complete. Setting aside parts of words that do constitute omissions not infrequently found in γ (e. g. 18.4 *uidetur* γ for *uideretur*, 33.2 *ingressi* γ for *ingressuri*) there is, too, 19.2 *cum*, omitted in γ before *condicione*. These are small slips of little importance. But the nature of the criterion seems to me quite a different matter. The criterion is hard, if not impossible, to quantify. The same question arises over Murgia's remark, 1977, 337: 'But this relationship is unlikely to be pure. If we were pure, the common source of B Γ would have omitted nothing but two monosyllables: 12.2, 20 *ex* and 27.3, 8 *et*'.

⁴³ See above pp. 133-134.

mean the Leidensis b — reveals that the conjectural-emendatory strain of b is almost entirely absent from B^v. I note one possible and one definite instance of emendation in B^v: 15.2 *inquirō* B^v may be meant to improve the correct reading, *conquiro*; 38.1 ⟨*horas*⟩ is required by the sense and is found added not only by B^v but also by Q, individual corrections most probably — a view strengthened by doubts as to the placing of the word in the sentence. That amounts to little. Rather what the remaining B^v instances display is a jostling to and fro between a small number of readings in a tight little setting. The setting must be defined as that of β. By this definition I mean that the bulk of the B^v instances is best explained as the reaction of the corrector to variants pre-existing in hyparchetype β, though some of them may be archetypal. What these readings give us is part of the early β tradition in the making. In saying this, we would be glancing up the stemma. As for a glance down the stemma, we note that B^v cases do not seem to display a sustained adherence to any individual manuscript source at our disposal; least of all do they recall late thoughts in the game characteristic of the *deteriores*. In view of these findings further examples will not here be required. They may easily be supplied from the critical notes of the Oxford text but would not add materially to the stemma so far drawn.

8. *The probability of transmitted variants in this tradition.* In a previous part of this paper I have suggested that the presence of old variants should not surprise us in a tradition that goes back to the cod. Hersfeldensis. But scholars have pointed out that the incidence of variants cannot be thought to be necessarily equal in the several components of that codex. Compared with the *Agricola*, the evidence for variants in the *Germania* is smaller; it seems to be smaller still in the *Dialogus*, to mention only the Tacitean writings so transmitted.⁴⁴ I now propose to consider some transmitted variants. The subsequent discussion will not make use of the types of old variants in the *Agricola* and *Germania* known from cod. Aesinas in order to infer similar types in the present work. Rather I shall address myself solely to the *Dialogus*, and aim at distinguishing the evidence for likely hyparchetypal variants from the evidence for archetypal ones. This is important for the status of the variants — archetypal ones have sometimes been claimed too readily — and has not been attempted except for a few scattered cases.

9. *Hyparchetypal variants.* There is a temptation when a variant is explained as transmitted, to jump to conclusions and regard it as archetypal. But that is not necessarily the case. Archetypal origin of a variant is often difficult, and at times impossible, to establish. In two types of cases variants will not lead back beyond the hyparchetypal stage, β or less often ζ. One is weak evidence that frustrates further enquiry. The verdict must then be that

⁴⁴ It should be remembered in this connexion that, owing to derivation from the Hersfeldensis, the possibility of variants arises in all the writings originally contained in that codex. The manuscripts, however, in which these variants actually occur overlap in some cases only and, inconveniently, their sigla do not square throughout. Thus the Vindobonensis 2960 appears as W in the *Germania* but as V in the *Dialogus*.

archetypal origin is possible but not necessary. That verdict must be given more often than is desirable. The other case is positive evidence more or less strongly suggesting that a variant was actually generated by the scribe or corrector of an hyparchetype. The outcome of this kind of enquiry will therefore be that the probable number of archetypal instances is likely to be larger than can be demonstrated.

A case in point occurs at 11.1. The passage has been discussed, above p. 138, as contributing to a case favouring bipartition of the stemma. I now address myself to the variants that may here be discerned.

11.1 *parantem inquit me ... accusare oratores* etc.

parantem inquit scr. Walther: *parant enim quid* (*quid* V) ζ:
parant quid enim β

Faulty word-separation in or before the *Hersfeldensis* will have resulted in turning *parantem inquit* into *parant enim quit* (*quid*), miscorrected to *enim*, added perhaps above the line, or in the margin, so that its position relative to *quit* (*quid*) became indistinct. ζ, as not rarely elsewhere, will have set down what he thought he saw in his exemplar, though it here makes no sense: *parant enim quid* (*quid* V); β on the other hand misplaced *enim*, so as to obtain *parant quid enim*. Perhaps he wished (mistakenly) to restore *quid enim*; perhaps the archetype had already a double reading to that effect. Such a double reading in the archetype is possible. But it need not be assumed, and cannot be enforced, in order to explain the difference of the hyparchetypes, ζ and β. In cases of this kind caution should make us refrain from going beyond the certainties of hyparchetypal evidence.

I suggest that a similar caution should be observed in many other cases of hyparchetypal variants, from which I select the following.

12.1 *secedit animus in loca pura*

secedit B^vγ: *sed* ζB

No one doubts the truth of *secedit*; omission by haplography of *ce*, after *se*, produced *sed*. The transmitted text clearly was that faulty verb *sed*. B's lucky fault shows that *sed* was on both sides of the stemma, not only in ζ but also in β. B however offers not only the fault, but also (if we abide by what was suggested above in section 8) in B^v, the correct variant *secedit*, and it is *secedit* that appears in C and lower down in the rest of the γ manuscripts. Winterbottom, therefore, in 1972, 117, was not only right in saying that 'the double reading *may* (the italics are mine) go further back', but he could have been more definite. We can therefore trace the variants back to hyparchetype β, where the scribe himself may have generated the correct variant. ζ's error, together with that scribe's apparent unwillingness to record variants, here frustrates what would otherwise be a reasonable guess — that the correct variant goes back to the archetype as well.

At 12.4 it may seem at first sight that there is a clear-cut distinction between the two hyparchetypes: β offers the required and obvious-seeming verb, *uelis*, whereas ζ, often much more competent, has the incomplete *uel*. But if our argument on the conflated readings of C (here *uelis uel*) prevails, this would presume the reading *uelis* on the β side of the stemma

also. Murgia 1977, 337, senses the presence not only of a variant, but an archetypal one: 'in an ordinary tradition we would readily brand a conflated reading like C's *uelis uel* ... evidence of contamination in C. In this tradition it could be taken as indication of an archetypal variant'. I am persuaded by the first half of the sentence, against contamination, but have my doubts on the second. There may not have been a variant in the archetype, but simply a physical blemish. Faced with the corrupt *uel*, the β scribe may himself have generated the 'variant' *uelis*, that is, he may have emended but carried on the original fault, *uel*, as well. That is the minimum requirement. What is certain is the hyparchetypal variant. The archetypal one is possible, but cannot here be enforced.

A similar but separate argument needs to be made about 30.2, where C seems to conflate the two extant readings, *uocant* (the right reading) and *uocatis* (the wrong one), to *uocantis*. The simple and true text is, *expetuntur quos rhetoras uocant*. This time, however, B has a reading which makes grammatical but no other sense (*uocatis*); but it also has a variant, B^v, which offers the true text, *uocant*. Not only that — but ζ has the same true reading, and there is (rightly) no acceptance of direct influence of one upon the other. But, whereas the true text, *uocant*, seems to present itself independently on both sides of the stemma, the erroneous one, *uocatis*, does not. Now *uocatis* might be branded as simply one of B's vagaries, were it not for the fact that C's conflation, *uocantis*, seems to presuppose *uocant* as well as *uocatis*, and we have argued above that C does not appear to take readings straight from B, but is another independent representative of hyparchetype β . Nor, in view of *uocant* and *uocatis*, is it likely that *uocantis* is an independent error in cod. C — *satis* at the beginning of the same line in the codex has been pointed out to me as a possible influence. But that, if it led anywhere, would be more likely to lead to *uocatis*. In this passage, as in others, C seems to have combined two readings thus imputed to β — *uocatis* + *uocant* = *uocantis* — without being able to make sense of the combination. It is possible that the double reading had come to β from the archetype — in which case the more competent scribe of ζ would have done what he apparently did often elsewhere: he made the right choice but did not carry on the variant. This conclusion could be drawn with safety only if the fault were represented on both sides of the stemma. The evidence therefore is again insufficient and we cannot enforce the step from hyparchetype to archetype.

In a fourth example a conflated C reading is also likely to lead the way to an hyparchetypal variant, though here we have a textual complication as well. Most editors print at 39.3 *saepe interrogat iudex quando incipias et ex interrogatione eius incipiendum est*. The manuscripts divide over *quando* B ϕ : *quam* ζ B^v; and again C conflates *quam quando*. Although an archetypal variant, *quam quando*, is entirely possible, indeed not at all unlikely because of the long tradition of abbreviating *quam* and *quando* (Lindsay, *Notae Latinae*, especially pp. 219–23), yet again it cannot, I think, be enforced since here too hyparchetype β could well have generated *quando* as a variant, in addition to the certainly old reading *quam*. The reason for the possible conjecture in B would have been precisely what has moved so many modern editors to print *quando*: it looks right though, I think, it is not. B and, lower down the

stemma, φ (= $Q\psi$) then picked up one of the readings, *quando*, but B^V preserved *quam* as well, while C, as in other places, conflated both, *quam quando*. On the other side of the stemma, ζ would not then have omitted the variant, as the scribe will have done in certain other instances, but preserved what was on that assumption the only reading available in the archetype *quam*. There is no need here to go into the difficult textual puzzle which prompted R. Güngerich's very persuasive emendation $\langle ante \rangle quam$,⁴⁵ since the textual problem does not change the stemmatic case before us.

In my next passage ζ shows up less favourably. At 16.2 '*aperiam*' *inquit* '*cogitationes meas*' there can be no doubt that *cogitationes* BC and some dett. (ψ) must be right and *cognitiones* ζQ and some dett. (ψ) wrong. The scatter of the readings being what it is, we can say with assurance only that *cogitationes* cannot be traced upwards beyond β , from which the apt reading, or correction, passed to its progeny but, in the sequel, provoked disagreement. Archetypal origin of the erroneous variant is possible but no more — in which case ζ again made a choice but this time a wrong one.

27.1 (26.8 ... *eloquentia*.) '*parce*' *inquit* *Maternus* '*et potius exsolue promissum*'
parce Andresen: *aparte* B^VC : *apparate* BQ (*appara te* b):
aproperate ψ : *aperte* ζ

Here *scriptio continua*, antecedent to *Hersfeldensis*, will account for the fault underlying the whole tradition: '*parce*' *inquit* *Maternus* '*exsolue promissum*'. Scholars are obliged to Andresen for his elegant emendation, but to Gudeman for explaining the chief cause of the corruption.⁴⁶ The final letter of the preceding word, *eloquentia*, in unpointed script, accounts, Gudeman rightly suggested, for the faulty beginning of the first word, emended from *aparte* et sim. to *parce*. It should be remembered that ends of speeches, or ends of what we now take for granted as 'chapters' do not necessarily receive special treatment in early transmission. Transliteration to minuscule will most likely belong to the same stage as word separation; *c : t* is a well-known minuscule error, offered by all manuscripts. On the other hand, since (as several readers remind me) *par* and *per* may appear abbreviated by the same symbol in Renaissance scripts, this symbol will account for *aparte* B^VC and *aperte* ζ . In turn *aparte* was miscorrected to *apparate* and further to *aproperate*. It is suggested therefore that *aparte* was the substantive text, certainly in the archetype, and probably, for the reasons just stated, inherited from the *Hersfeldensis*. The miscorrection, *apparate*, is found in B and Q, which suggests the high probability that hyparchetype β had the double reading *aparte*: *apparate* (or *appara te*). These variants could well have been inherited from the archetype by β , or else the latter reading could have been generated by β as 'emendation' of *aparte*. If the variant was generated by β , the hyparchetype on the other side, ζ , simply adhered to the only available

⁴⁵ The emendation is proposed in Güngerich's commentary, 1980, and adopted in H. Heubner's edition, Teubner (Stuttgart) 1983.

⁴⁶ Andresen, *Acta Soc. philol. Lipsiensis* I (1881), 134, preferred the simple *parce* to Michaelis' cumbersome proposal (below) because *magis arridet*. All others at the time retained the nonsensical letter *a* in their proposals: Michaelis ed. 1868 (app.) 'fort. at *parce iis*'; Usener, *Kl. Schr.* III, 13 n. 4, *ah parce*, Halm at *parce*.

reading but got it slightly wrong: *aperte*. If on the other hand the variant was inherited from the archetype, ζ, as apparently in other cases, opted for one reading and omitted the other. That is possible, no more, whereas the β variant ranks as a minimum requirement.

At 29.2 the second word of the sentence, *parentes improbitati neque modestiae paruulos adsuefaciunt, sed...*, is obviously corrupt. The erroneous word, *improbitati*, must have been carried from the archetype to both sides of the stemma: VE = ζ and B (arguably from β). Baehrens' and Vahlen's identical correction, *non probitati*, was independently made in 1881,⁴⁷ and soon generally accepted. It is right in thought, Tacitean idiom (*non ... neque (nec) ... sed*), and palaeography (*n'*, an ancient *nota* for *nec* (Lindsay, *Notae Latinae*, p. 132), into *in* or *im*). As in the preceding instance the original fault probably antedates the archetype, deriving from mistaken word separation, and compounded by a wrongly resolved symbol. There is a miscorrection, *nec improbitati* in B^VCQ, slightly but not fully corrected to *nec probitati* lower down the stemma in the ψ codices. The main miscorrection, *nec improbitati*, could once more be inherited by hyparchetype β in addition to the archetypal error, *improbitati*. In which case ζ would yet again have opted for one of the variants only. But here too we cannot disprove the other possibility — no more than one (erroneous) reading in the archetype, which ζ did not here recognize but β did; β then mended the to him unmendable. Hence, in that eventuality one certainty only, an hyparchetypal variant.

There are other passages, and not a few of them, where similar justified questions will lead to similar justified answers eschewing cross-contamination in the upper ranges and strongly suggesting hyparchetypal variants in a stemma which I have sought to prove bipartite. By the same token however the evidence as it stands allows us in principle to extend the variants further up the scale to the archetype but the constraints of the evidence have often been seen to rule out the same security of conclusion for the archetype that seemed to apply to the hyparchetypes.

10. *Archetypal variants*. The view was expressed in the last section that often archetypal variants are possible but, unlike hyparchetypal ones, are not open to demonstration. One may wonder for that reason whether they can be demonstrated at all. I believe they can; but the constraints of our evidence almost necessarily reduce the number of archetypal variants that can be proved, in comparison with hyparchetypal variants. It is therefore incumbent on those who believe that they can be demonstrated to do so.

12.4 *nec ullis aut gloria maior <erat> aut augustior honor*

ullis ζC: *ullus* BQ: *illud* ψ

maior scr. Lipsius (iam V² mg.): *more* EBQ (*mor* b), *spatiis relictis* (*clamore* b mg. ψ): *in ore* V (*spatio relicto*) B^V

<erat> suppl. Ritter (*Rh. M.* 21 (1866), 541): *alii alia*

⁴⁷ E. Baehrens ad l., J. Vahlen, *Op. Ac.* I, 145.

First, doubts as to the grammar of the sentence will have engendered the variant *ullus*, instead of *ullis* ζC, which may or may not have been archetypal; uncertainty is caused by the absence of *ullus* in ζ. Secondly, since the contraction *mor* does not seem a conventional shortening of *maior*, a corruption of *maior* is indicated. Hence it will have been the erroneous *mor* that was transmitted before it was turned into a pair of variants, *more* vs *in ore*, in the archetype. From there it will have gone to both sides of the stemma, represented as *more* by E and BQ (but *clamore* emendation in b) and as *in ore* by V and B^VC. Moreover a lacuna is indicated after *gloria* also on both sides of the stemma, C excepted, and is therefore likely to pre-date the hyparchetypes. The addition of *erat*, which I favour in spite of Gudeman's and D. Bo's notes, is not relevant to the stemmatic question.

20.5 *exigitur enim ... poeticus decor*

exigitur scr. Lipsius (*exiget* iam c^tΔ):

exercitur V pr. B: *exercetur* V corr. EB^Vγ

In this passage the spread of the variants is good, though the strength of the variants small. The original error for *exigitur* was *exercitur* (*exigitur* – *excitur* – *exercitur*), antecedent to both the ζ and β traditions. The (false) correction *exercetur* too seems to be antecedent to both traditions: E V corr. in one tradition and B^Vγ in the other, and we discount contamination in this part of the stemma. Hence the conclusion that *exercitur*, var. *exercetur*, showed up not only in the two hyparchetypes but in the archetype itself.

In two other passages the variants are likely to go back to the archetype: there is a good spread of readings: 9.4 *pr(a)ecepta* – *percepta* and 20.6 *pr(a)eueniunt* – *perueniunt*. But the initial-letter suspensions \bar{p} and \underline{p} (for *prae* and *per*) are so usual that their argumentative weight must be considered small.

The archetypal origin of these variants carries conviction because of the wide spread of the evidence on both sides of the stemma. But I have noted several times that the textual discrepancies themselves tend to be small.⁴⁸ It may therefore be desirable to conclude this section with some more complex cases.

25.1 *sequar praescriptam a te, Materne, formam*

praescriptam a te BCQ² recte: *praescriptam et* V:

a te praescriptam Q¹ψ (= φ): *et praescriptam* E

The passage is disfigured by two different sets of variants — one of wording, another of placing.

The fault of wording arises from a confusion of *a te* (*ate*) and *et*. The reading *a te* is found in all the β codices, but not elsewhere; it appears to be right. On the other hand *et* occurs only in ζ, as far as primary codices are concerned; *a te* may have been turned by inadvertence into *et* or a ligature for *et* (E or &, cf. n. 36). It need not go further back than hyparchetype ζ.

⁴⁸ There are other small-scale cases where nevertheless the distribution may well favour archetypal origin, such as 20.4 *audire* – *adire*, or 25.2 *com(m)inus* – *quominus*, or 28.4 *dicere* – *discere*, or 34.3 *exprobret* (*exprobaret*) – *exprobet*.

The fault of placing however is primary and must go back further. It applies to both textual variants, and is distributed equally on both sides of the stemma; *a te* or *et* is found either placed correctly after *praeceptam* (although *et* makes no sense, whichever way it is placed) or incorrectly before it. The only explanation of this displacement which seems acceptable is that *a te* (*et*) was omitted by an oversight after *praescriptam*. It will then have been added, as an apparent variant, say, at a place above *praescriptam*, or in the margin, which could have been, and duly was, interpreted in two incompatible ways — either to come (correctly) after, or (incorrectly) before the participle. Since both placements are equally represented on both sides of the stemma, this disagreement, on the principle here adopted for the upper codices, will go back as far as the archetype. To invoke two displacements independently made as a coincidence on each side of the stemma seems to me as little convincing as a similar contingency in an earlier case.⁴⁹

In the next passage again we have to deal with two parallel sets of variants:

20.3 *nec magis perfert (uulgus) in iudiciis ... antiquitatem quam si quis in scaena Rosci ... exprimere gestus uelit.*

in scena E: *in caena* V

in sc(a)enam BvCQ: *in c(a)enam* Bψ

The manuscripts offer two pairs of variants: one, the basic difference between *sc(a)ena* and, with omission of the stroke for *s*, *c(a)ena*; the other a difference between *in* + ablative or accusative, that is, a suprascript stroke on *a*, placed or omitted. I suggest that the archetypal origin of the basic variant cannot be in doubt; it is equally present in the different groups of codices on both sides of the stemma. The secondary fault, an accusative, *sc(a)enam* or *cenam*, probably amounts to no more than a false analogy with the preceding *antiquitatem*; it was certainly in β, and may be no more than hyparchetypal. But we cannot tell, for as in most cases where ζ makes the right choice, the scribe may or may not have jettisoned an archetypal variant. In sum, the basic variant strikes me as certainly archetypal; the secondary belongs at least to one of the hyparchetypes.

I end with a very complex passage. Here there is at least a case for the assumption that two of the readings go back to the archetype.

41.2 *quis enim nos aduocat nisi aut nocens aut miser?*

quis enim ζ: *quis enim quod nemo* B^vQ:

quidem quod nemo Bψ: *quid enim quod nemo* b: *quis enim quidem quod nemo* C

Editors now agree that the two initial words are all that is required before *nos aduocat*, and they print accordingly. But of primary codices V and E alone⁵⁰ restrict themselves to these two words: this in fact is the ζ text. All other codices have a badly interpolated text. Can we account for these interpolations?⁵¹

⁴⁹ Above p. 138.

⁵⁰ We may discount c and Δ, two interpolated codices, for which see above p. 132 with n. 3.

⁵¹ The question was shrugged off by Güngerich (posthum. comm. 1980).

It would appear that *quis enim* and *quidem* are competing readings. If my discussion of cod. C is right⁵², *quis enim* (in addition to *quidem*) was in hyparchetype β without suspicion of interpolation. Whether ζ on the other hand knew *quidem* cannot be known. The most likely assumption is that ζ omitted what he rightly considered an error, in this and in a number of other passages. With *quis enim* certainly, and *quidem* possibly, known to ζ , and with both readings known to β , it is reasonable to ascribe both the genuine and the false readings to the archetype. No other explanation fits the facts equally well.

The scribe of β , however, carried on not only *quis enim* (right) and *quidem* (wrong) — the latter probably just an old corruption of the former — but furthermore the apparently meaningless *quod nemo*, which, in different combinations, is in all extant codices on the β side. A plausible way to account for these two words was put forward by Vahlen in 1877⁵³; *quod nemo*, perhaps phrased *i. q. nemo*, was intended as an explanatory gloss, but later foisted on the text, answering as it were the rhetorical question: *quis enim (nisi) ...?: nemo (nisi)*, 'no one (except)'. But it has been suggested to me by one of the referees that if there was a gloss at all, it would have taken the form of a paraphrase (*quis enim: quod nemo*); more likely still, he thinks, *quod nemo* could be a conjecture for an error *quid ·n·*. I agree that a paraphrase would be a more likely explanation than Vahlen's interrogative structure, but find little rationale for the conjecture. We can then ascribe to β *quis enim* and *quidem* as well as *quod nemo*, which are continued by B^vQ as *quis enim* and *quod nemo*, by B and ψ as *quidem* and *quod nemo*, and by C (above p. 136) as an actual conflation of the three incompatibles *quis enim – quidem – quod nemo*. *quod nemo* cannot be traced back further than hyparchetype β . What matters more is that, for the reason set out above, *quis enim* as well as *quidem* seem to precede the hyparchetypes.

11. *Summary*. Three groups of manuscripts have lately been formed — in the terminology of Winterbottom's edition of 1975 (which occasionally changes the symbols of his fundamental paper of 1972): (1) ζ , (2) a group represented by cod. B, and (3) an intermediate group (Γ , in this paper γ). Yet Winterbottom's tripartition rests on the independent standing of γ , which has already been refuted in three important instances in favour of bipartition by Murgia 1977.

The present paper offers, for the first time, a full analysis of γ , and especially of its chief manuscript C. It comes to the conclusion that γ has no independent standing as an hyparchetype. Rather γ and B are shown to be tied closely together. In many cases the link can be explained only by being subsumed under an hyparchetype common to both (β); elsewhere they are best so explained. Such indeed are the two types of evidence one would expect to find if a case for or against a partition is to be made. What remains to be scrutinized is the soundness of interpretation of each passage that comes into consideration.

⁵² Above pp. 134 ff., the present passage at p. 136.

⁵³ J. Vahlen, *Commentationes in hon. Theodori Mommseni*, 666.

If a bipartite structure may thus be argued, contamination in these cases in the upper region of the stemma need not be accepted, as Winterbottom 1972 had already suspected. Hence the question whether the evidence is strong enough to replace recourse to contamination in this area by a reasoned claim for 'transmitted variants'. The writer believes that the evidence is strong enough. To avoid, however, dubious conclusions which have at times been drawn in favour of archetypal variants, he distinguishes between archetypal and hyparchetypal variants (likewise for the first time over the whole field of the evidence) and considers what, he thinks, are the best instances of either kind. This requires careful interpretation in an uncharted area.

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