

A PROBLEM IN THE TRANSMISSION OF QUINTILIAN'S *INSTITUTIO ORATORIA*

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IN HIS admirable *BICS* monograph,¹ M. Winterbottom begins with a discussion of the relationship to codex A (Ambrosianus E. 153 sup.) of corrections and supplements in codex Bg (Bambergensis M. 4. 14). Bg displays inherited lacunae, which have been filled by later scribes² (given the symbol G). Scribes also corrected Bg where that original layer of the codex is extant, and these corrections are given by Winterbottom (following Halm) the siglum b. It is natural to expect, and consistent with their readings, that b and G have the same stemmatic relationship. In the remarks that follow, the siglum G is used for both G and b, except where the distinction is important. Everyone agrees that G descends from a codex very much like codex A, which in many sections is the only other witness of authority.³ Winterbottom argues that G descends from a copy made of A while A was in the process of correction (by correctors given the siglum a); therefore in the first six books G usually follows the corrections of a, but after that it usually attests what must have been the original reading of A, before a erased it.⁴

As a proof of his interpretation, Winterbottom cites a number of readings in which G seems to have adopted a conjecture of a. But the very first

1. M. Winterbottom, *Problems in Quintilian*, *BICS* Supplement 25 (1970): 5–17. Winterbottom's text (*M. Fabi Quintiliani "Institutionis Oratoriae" Libri Duodecim* [Oxford, 1970]) is cited as OCT. Other works cited by short title are: E. Chatelain, *Paléographie des Classiques Latins* II (Paris, 1894–1900); C. Halm (ed.), *M. Fabi Quintiliani "Institutionis Oratoriae" Libri Duodecim* (Leipzig, 1868); C. E. Murgia, *Prolegomena to Servius 5—The Manuscripts*, *UCPCS* 11 (Berkeley, 1975); idem, "The Minor Works of Tacitus: A Study in Textual Criticism," *CP* 72 (1977): 323–43.

2. Both the earlier and later parts of the codex are listed (OCT, p. v) as saec. X. In Winterbottom, *Problems in Quintilian*, pp. 5–6, we learn that they are respectively saec. XI¹ and X². I assume that "scribes" is correct, although Winterbottom does not make clear whether the supplements are in one hand or several. I have not inspected the MSS, and am dependent on the reports of Winterbottom and Halm.

3. We are limited to AG as witnesses in 5. 14. 12–8. 3. 64; 8. 6. 17–67; 9. 3. 2–9. 4. 135; 12. 11. 22–31. In other sections G only is extant: 9. 4. 135–10. 1. 107; 11. 1. 71–11. 2. 33; 12. 10. 43–12. 11. 22. In almost all other sections, codex B (Bernensis 351) is available as an important witness. Bg is there a descendant of B, from which Bg inherited the lacunae which G fills.

4. Skepticism towards Winterbottom's proof has been expressed by P. K. Marshall, review of Winterbottom, *M. Fabi Quintiliani . . . Libri Duodecim*, *AJP* 95 (1974): 81. J. Cousin (ed.), *Quintilien, "Institution Oratoire"*, vol. 1 (Paris, 1975), p. cvi, seems to suggest that G may reflect rather correction from a's source. In his *Recherches sur Quintilien* (Paris, 1975), p. 6, n. 2, he states (without notable argument): "Il y a apparence que A a été une source de G, partie plus récente de Bg, mais que G a été néanmoins largement indépendant."

Winterbottom's latest word on the value of G is in his review of vols. 2 and 3 of Cousin's text in *Gnomon* 50 (1978): 686: "b (G) undoubtedly has its interest in giving us a clue as to the readings of A before correction, at least in certain parts of the text . . . and I now regret that I did not use it more for this (one) purpose. . . ."

example cited⁵ requires at least a modification of Winterbottom's interpretation:

5. 14. 21 nisi et propositio uera sit *Victor*: ***** propositio uera sit A cum eius propositio uera non sit a: *niş* cum eius ut propositio uera non sit G

My citation is based on C. Halm's apparatus, in which * indicates an erased and illegible letter. Winterbottom agrees, but less clearly. The printed reading was adopted by Halm from the quotation by Julius Victor (fourth century). A must have had something similar, but now only the final *t* of *nisi et* is visible after erasure by a. Winterbottom claims that G's reading does not indicate knowledge in G of A's original reading (which, apparently, could not be read after a's correction), but that the *nisi* of G arose from a careless repetition of (*h*)*is* in the preceding line. This explanation does not work, since it fails to explain *ut*—clearly the remnant of the original *et*.⁶

G has a conflation of the correct *nisi et* (corrupted to *nisi ut* or *nisi ul*)⁷ with a's correction (*cum eius . . . non*). Unless G is independent of A, the most reasonable interpretation is the following. G descends from a copy of A made before A was corrected, with A and its copy reading *nisi et*, *nisi ut*, or *nisi ul*. This copy (or some intermediate descendant of it) was subsequently corrected to the text of a (probably from codex A after correction, but possibly from some copy of the corrected A).⁸ The alternative is that G is basically independent and descends from A's source (probably A's exemplar, which Winterbottom believes to be the source of some corrections in a, and so still to be available in the same scriptorium as A and the scribes of a), but through a copy (or other descendant of A's source) which had been corrected to the text of a (as above). That is, to some extent at least, G does descend from A after correction,⁹ but it remains to determine whether Aa is its sole authoritative source, or G also has a source that is independent of A.

Both alternatives present serious problems. The problem for those who believe that G is independent of Aa is to find enough examples of unique transmitted truth in codex G; for I do not doubt that Winterbottom is correct in claiming that the readings of G which he discusses in *Problems in Quintilian*, pp. 11–12, are or could be medieval conjectures. To state the problem the other way around, those who would claim that G is independent

5. See Winterbottom, *Problems in Quintilian*, p. 8.

6. Winterbottom, *Problems in Quintilian*, p. 8, explains *ut* as a separate error (dittography after *eius*). Not only is the error improbable, but it is poor method to resort to two separate (not to say unlikely) explanations when a single cause of error is before our eyes which easily explains all the phenomena.

7. *Nisi ut* would suppose a progression *NISIET* > *NISIIT* > *nisiit* > *nisut*. The error would be aided by false division: the preceding word is *uera*, and the scribe misdivided *ueranisiit* as *ueranis ut*. It would be useful to inspect A to see if the codex shows word division between *uera* and the erasure. The reason for uncertainty about the specific course of error is that the simple error *ut* for *et* is also common in Latin MSS.

8. The other possibility, that the copy was corrected to the source of a's readings, does not accord well with the examples which Winterbottom offers (*Problems in Quintilian*, pp. 8–10) of misunderstood corrections of A.

9. For convenience, I shall refer to A after correction as Aa.

of Aa must explain the apparent lack of unique error in Aa.¹⁰ The argument must be that the combination of a's care in correcting A, and the care of interpolators in G's tradition in correcting it to the text of Aa, resulted in the passage of very few unique authoritative readings to G. This is conceivable, but it does not provide us with a basis for further argument.

The main obstacle to denying independent authority to G is its performance in transmitting Greek words. Greek words are highly resistant to contamination except in the rare instances where a scribe fancies that he knows some Greek.¹¹ The scribes of a (or A?) have attempted some corrections of A, and in the process almost always departed further from the truth.¹²

7. 4. 14 μεταστασιν] μετασασιν G μεταθεσιν ex μετα***σιν A¹³
8. 3. 52 ομοειδεία] ομοειδια Π ομοεια Ria G ομοειλ*** A ομοεολογια a¹⁴
9. 3. 68 αντανακλασις G αντιπεκδοσις in ras. a
9. 3. 80 ισοκλων (prim.)] υσοκμλον G ισοκομμον (ομμον in ras.) A
9. 3. 80 ομοιοτελευτον] ομοιοτελευστοη G ομοιοτελον (λον in ras. mai.) A
9. 3. 94 προσαποδοσιν] προσαποοσσιν G προσαρπασιν (ασιν in ras.) A¹⁵
9. 4. 36 συναλιφαι] συσαλιφλι G σιναλλογιφαι (σινα in ras.) A
9. 4. 66 et qui] εσ qui G και τοις in ras. a

10. On the need to find a normal quantity of error in a codex to which we would deny the standing of archetype, see Murgia, "The Minor Works of Tacitus," pp. 325-326.

11. For the mechanical habits of medieval scribes in copying Greek, see Murgia, *Prolegomena to Servius* 5, pp. 174, 180, and pls. 5-13, where it can be seen that scribes sometimes copied even the form of the letters before them. Although knowledge of Greek is rare in medieval scribes, it is manifested by the Irish scribe of codex B of Servius (the well-known codex Bernensis 363). The same hand is found in Codex Basiliensis A.VII.3, a Graeco-Latin psalter. The scribe of Bernensis 363 readily betrays his knowledge of Greek not only by correcting obvious errors that are found in closely related MSS, but by his learned blunders: the scribe is capable of miscorrection. It may be noted that this scribe may have operated in northern Italy (at least some texts in the codex are related to Milan, and the family of Servius to which it belongs is north Italian), and that both A and G of Quintilian are claimed to be written in Italy (so Bischoff as reported by Winterbottom, OCT, p. ix). Therefore, the possibility of scribal knowledge of Greek cannot be dismissed out of hand, although it is manifested in less than two percent of medieval MSS that I have examined.

12. In the following lists, I report Greek in minuscule letters in order to facilitate typesetting. In medieval Latin MSS Greek words, when not transliterated, are regularly written in uncial letters. In the following examples, G's readings illustrate common confusions of Greek and Latin uncials: α, δ, and λ; ι and τ; ν and η. In 9. 3. 80, G's μ for ω reflects the similarity of ω to an upside-down uncial m. In 9. 3. 94, G's σσ would in uncials look little different from ω, for which it must be an error (i.e., G attests προσαποδωσιν, with confusion of δ and ο). Other confusions reflect similarity of pronunciation, ει and υ being pronounced like ι.

13. Halm reports A as agreeing with G. Winterbottom reports "(μεταθεσιν A, sed -θε- in ras.)." What one would like to know in this and the following examples is whether the correction is by the hand of A, or by one of the correctors called a, or perhaps even by a later hand. I have drawn all the reports from either Halm or Winterbottom, but without any confidence whether each correction should be attributed to A or a. Cf. n. 14.

14. I follow Halm. Winterbottom reports (instead of Halm's reports of A and a) simply ομοεολογια ex corr. A. Where Halm reports a in the following examples, Winterbottom uses many different symbols: 9. 3. 68 post corr. A, 9. 3. 80 and 9. 4. 36 A (corr.), 9. 3. 94 in ras. mai. A, 9. 4. 64 a in ras. He nowhere in his edition explains what he means by these symbols, and whether A (corr.) or such means anything different from a. Attributions to A rather than a should mean that the correction is by the original hand, but it may be that Winterbottom is simply avoiding committing himself on the date of the corrections. It is difficult to date securely corrections of Greek uncials in Latin MSS, particularly when collating from facsimile.

15. I follow Winterbottom's citation (see *Problems in Quintilian*, p. 16). Halm reports ω for σο in G (a minor difference), and π for ρ in A—the latter a crucial difference, since the omission of π by G could be coincidental haplography, but the omission of ρ requires either knowledge of Greek or independent testimony.

It is clear that the corrector(s) of A knew some Greek, both too much and too little. All of the errors in G are easy errors such as are commonly found in Latin scribes ignorant of Greek—errors of carelessness or of confusion of uncial letters. The errors in A (a), however, all manifest movement toward genuine Greek words or syllables—a clear indication of knowledge of Greek. This is what we regularly find in the few instances when we encounter a Latin scribe who knows Greek: his knowledge more often leads him astray than aright.¹⁶ This after all is no different from performance in conjectures. We detect a scribe who makes conjectures because his plausible mistakes outnumber his brilliant emendations.

G on the other hand nowhere gives such evidence of knowledge of Greek.¹⁷ There are some orthographical confusions which indicate ability to pronounce Greek letters, but there are no miscarried conjectures in Greek words, or apparent conflation of correct and incorrect versions. There are nevertheless a considerable number of instances in which G's version of Greek words is more accurate than the versions in A.¹⁸

No great problem is presented by G's more accurate versions of 6. 3. 70 *σχήματα*, 7. 4. 7 *ἀντίθειν*, 7. 4. 8 *ἀντέγκλημα*, 8. 2. 5 *κατάχρησις*, 9. 3. 54 *κλῖμαξ*, 9. 3. 80 *ὁμοιοτέλευτον*: correction of common rhetorical terms is within the capacity of many medieval scribes, and the fact that most of these are nevertheless corrupt in G can be explained by Winterbottom's theory of recorruption by G after correction by an intermediate scribe between G and A. More serious problems are created by 9. 3. 94 and 9. 4. 36 (cited above), and by the following:

- 8. 3. 59 ἀποίκονομητον G αποινομητον A
- 9. 3. 87 ἀνθυποφοράν] ανουποφοραν G αποιποφοραν A
- 9. 4. 73 πᾶσι καὶ πάσαις] πασικαιπασαιε G πασικαιε A

Not only are the corrections of A more difficult for the above, but for some we would have to suppose that once corrected the Greek words were recorruped back to the very text that must have existed in A's exemplar. In 9. 3. 87, G shares the error O for Θ with A while nevertheless having correctly N and T for A's Π and Ι. In 9. 3. 94 the erasure of the end of the word in A may have been prompted by such a corruption as G displays; yet, if

16. A good example can be drawn from Servius Auctus on *Aen.* 12. 139. Here, where Servius says "deus autem vel dea generale est: nam quod Graece *δέος* [est], Latine timor vocatur," Servius Auctus has "deus autem generale nomen est omnibus: nam deus Graece dicitur *φόβος*, id est timor." For *δέος* most Servian MSS have *deos* or *deus*. The Compiler of Servius Auctus, or perhaps his source (cf. Isid. *Orig.* 7. 1. 5), rewrote Servius to supply the word which he knew as the Greek equivalent of *timor*, thereby showing both how much and how little Greek he knew. Another family of MSS (γ) has emended *deos* to ΘΗΟC, thereby indicating knowledge of the Greek equivalent of *deus*—as might be expected of a monk even if he knew virtually no Greek. The γ family of Servius is mostly north Italian.

17. I speak here of G proper. There is some evidence that the scribe of b may have known some Greek (see Winterbottom, *Problems in Quintilian*, p. 17), but all such evidence points to knowledge in the scribe himself, not in his source. Winterbottom does not believe that the scribe of G himself knew Greek, but rather his source (since the Greek has been supposedly recorruped in G). Since it is not believed that G descends from a MS written by b, knowledge of Greek by b would not provide an explanation of G's readings.

18. These are cited in Winterbottom, *Problems in Quintilian*, p. 16.

Winterbottom reports the readings accurately, A has an error P for Π which is not shared by G. In 9. 4. 36, the corruption in the beginning of the word displayed by G may again have been what prompted the correction in A; but in the last half of the word A displays an apparent conflation not shared with G. A's reading seems to result from a conflation of G's text with a conjecture such as *συναλλογαι* or *συναλογια*—not good Greek, but more familiar in its roots than what G displays.¹⁹

The reading which seems to render untenable a theory of complete dependence of G on A is 9. 4. 73. Even if we suppose that there existed a scribe in G's tradition who was capable of correcting *πασικαιε* to *πασικαιπασαιε*, can we really believe that he could have corrected rather to *πασικαιπασαιε*? or that, if he corrected to *πασικαιπασαιε*, *πασαιε* was accidentally corrupted back to *πασαιε*, the very reading which must have been in A's exemplar? Can we believe that a scribe who was willing to emend so boldly would never make a mistake (in sharp contrast to the performance of A and its corrector[s]), but would always produce a text identical with what must have existed in A's exemplar—a text often unintelligible, but nevertheless closer to the original? These are not reasonable interpretations of the evidence. The reading *πασαιε*, taken by itself, could be explained as resulting from a conflation of a conjecture *πασαιε* and the original *ε*. But it is odd that a conflation would produce a text indistinguishable from what must have been before the scribe of A. It is even odder if we note that there is no other sign of conflation in Greek in G. The scribe of b has created conflations in his correction of Bg, though he does not transmit conflations. And even A seems to display a conflation in 9. 4. 36. But there is no objective evidence of correction in G's tradition—no conflations producing readings that could not have been in A's exemplar, no miscarried conjectures. The only way to interpret G's performance in Greek as indicating basic descent from A is to find a massive conspiracy of error in the collations of Halm and Winterbottom. I doubt that that could be true, particularly in 9. 4. 73, where the issue is not the distinction of hands.

On the basis of readings discussed so far, we posit that G descends from A's exemplar²⁰ (let us call that exemplar *a*) through an intermediary or intermediaries, which we will call C (one intermediary, or all collectively). C was corrected by a corrector or correctors, which we will call *c* (one or

19. This reading makes me wonder whether the conjectures in Greek manifested in A and *a* were not already to be found written as variants in A's exemplar. The mind which would interpolate the Greek stem *λογ* into (9. 4. 36) *συναλιφαι*, and (8. 3. 59 and 9. 3. 87) turn *ανο* into *απο*, does not seem noticeably different from the mind that turned (7. 4. 14) *μετασασιν* into *μεταθεσιν*, (8. 3. 52) *-ιλεια* into *-ολογια*, (9. 3. 80) *-κμλον* into *-κομμον*, (9. 3. 80) *-τελευστον* into *τελον*, and (9. 3. 94) *αποσσοσιν* into *-αρπασιν*. It is a mind concerned with perceiving common Greek prefixes, roots, and syllables, regardless of the sense required by the context. But without the codex before our eyes, we can do no more than speculate. It may be that a glance at A would immediately suggest a different interpretation. The conjecture *προσαρπασιν* can be more easily explained as arising first in A (after an error *ρ* for *π*?). If A and *a* are copying corrections from a common exemplar of A and G, we must suppose either that they were added to that exemplar after G's source had copied it, or that they were so written that G's source was able to and did ignore them all.

20. G's source could be further removed from A, but the further removed, the harder it becomes to explain the lack of important readings in G not shared with Aa.

all). Corrector c used Aa as his exemplar, and corrected C so thoroughly that in its Latin text Cc often resembled Aa more closely than α .²¹ But c did not know Greek, and so attempted no corrections of C's Greek, even by contamination. Therefore the Greek text transmitted to G betrays G's basic relationship, while the Latin text gives the deceptive appearance of descent from Aa.

But can G, in the above circumstances, transmit in Latin no major variant that is not, or was not once, in Aa? I would not have thought so, but we seem to be forced to that conclusion. When compelled to choose between the improbable and the untenable, we must prefer the improbable. I think that other possibilities, such as that G is independent in the Greek, but dependent in its Latin, are so unlikely as to be not worth serious consideration.²²

Let us see then if the poverty of important variants in G can be explained. G's descent can be represented stemmatically as follows:

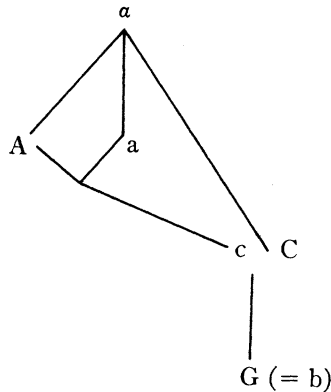


FIGURE 1

21. I use Cc for C after correction, as Aa for A after correction. A theory that c corrected not from Aa but from a's source would not account for all the errors and conflations in G which Winterbottom records in *Problems in Quintilian*, pp. 8-10. Some of the errors which Winterbottom there records nevertheless could have been committed by C in copying A's exemplar: notably the omissions in 6. 3. 99 and 7. 2. 23, since A's exemplar could easily have had the same lineation as A (as A₁ displays similar lineation to B in Chatelain, *Paléographie*, pl. 179A and 179B).

Note that under my explanation α and A are housed in the same scriptorium, thereby permitting C to be copied from α and corrected from Aa. With Winterbottom's explanation, α and A are likewise housed in the same scriptorium. Winterbottom believes that a corrected A by comparison with α , and that C copied A while this correction was still in process (therefore, while α was still available). Both explanations suppose an identical availability of codices. Winterbottom must explain why, if α was available, the scribe of C never looked at it. I must explain why a scribe would correct C to Aa, rather than simply to Aa's exemplar. My answer is that c wished to avail himself of a's conjectures and of the other editorial changes of Aa (word division, punctuation, orthography).

22. I here talk of an early medieval tradition. In Renaissance and late Gothic MSS, scribes frequently omitted the Greek, leaving blanks to be supplied later. In such MSS it would not be unusual to find a different basic tradition for Greek and Latin. Although we cannot rule out a similar situation for an early tradition, contact with an independent tradition for Greek would be unlikely to leave the Latin text unaffected. We also cannot rule out the possibility that G is dependent for Books 1-6 (where we have insufficient Greek as a control) but independent thereafter. But such a theory would not eliminate the difficulties which I enunciate.

According to strict stemmatic theory, one might not expect G ever to be correct against the consensus of A and a, since it is at best (when it follows C) only one witness against two, and sometimes (when it follows c) a mere *descriptus*. In practice, it is not so easy for a corrector to remove every error in a codex.

What concerns me most is the lack of major omissions in Aa relative to G. All scribes make not only errors, but also major errors. The scribe of A is no exception. But A has been very thoroughly corrected, perhaps in several stages of correction—that is, A seems to have been compared to an ancestor of itself not once but several times.²³ Winterbottom claims that every fifth word, so to speak, has been subject to correction.²⁴ This is an exaggeration, but a glance at Chatelain, *Paléographie*, pl. 174, shows that correction is very frequent. Among corrections depicted, several omissions have been filled. It is not necessary, then, for one who believes in G's independence to find the scribe of A unbelievably accurate. A made a normal number of errors of all sorts, including omissions and other major errors. But the thoroughness of correction has left few readings transmitted by G that cannot be found in either A or a, or be supposed to have originally been in A before correction. G could not have descended merely from A before correction (since it avoids many errors of A that have been corrected by a), or merely from Aa after correction (since it transmits many readings that must once have been in A, but have been erased or obscured by a).²⁵ But only G's text of Greek words gives convincing evidence that G could not have simply descended from codex Aa both before and after correction. The Greek text shows that G's independence of Aa is by right of basic descent, rather than by contamination, since Greek words are much more resistant to change by correctors, and since conflations are lacking in G.

Another factor that may have acted to eliminate unique transmitted text in G is the correction of G's ancestor (C) to the text of Aa. How thoroughly such correction can influence a later descendant can be observed from Halm's codex M. Winterbottom has established that M is a descendant of Bg after correction and supplement by b and G.²⁶ Bg is a descendant of B, but its text was corrected by b to agree with a text related to Aa (our hypothetical Cc). When this text omitted words transmitted by B and Bg, the corrector b deleted them in Bg. M usually accepts the text as corrected by b. So in the first thirty-two sections of 11. 1, M follows b in omitting the following: 2 *et* (ante *uim*); 2 *etiam numerisque*; 4 *in tertio*; 6 *nobis*; 7 *commune*; 8 *quid deceat . . . nam*; 9 *ad*; 12 *cum*; 13 *uel* (prim.); 18 *in*; 21 *de . . . tamen*; 24 *et* (ante *o*); 25 *in* (ante *et uita et gratia*); 26 *et* (post *me*); 29 *more ac*; 30 *etiam*; 32 *propositum*; 32 *in* (ante *adulescentibus*).²⁷ The only deletion

23. I gain this impression from the variance in appearance in corrections visible in Chatelain, pl. 174. Note that Winterbottom's analysis also supposes multiple correction, since G sometimes transmits readings erased in A, and sometimes transmits rather a.

24. OCT, pp. viii-ix.

25. See Winterbottom, *Problems in Quintilian*, p. 6; OCT, p. ix.

26. M. Winterbottom, "Fifteenth-century Manuscripts of Quintilian," *CQ* 17 (1967): 364. The descent is not direct, but through codices F and T.

27. Citations of M are from Halm.

not listed by Halm as followed by M in this section is in 24 "o fortunatam natam me consule Romam," where b alone deletes *natam*. Here meter and the notoriety of the line have prevented acceptance of the omission.

If then the hypothetical codex C was corrected by c to agree with Aa in the manner that Bg has been corrected by b, any omissions perpetrated by A which were not corrected by a might yet have been imposed on C by c, producing in G a text which rarely transmits words omitted by Aa.

But there is a limit to this argument. G often seems to transmit improbable errors of A which a has corrected by erasure. Unless c operated, as Winterbottom postulated that C did, while a had not yet completed his work, or (as is quite possible) was less effective in detecting variations of a letter or two than lengthy omissions, his attentiveness is likely to have been a less important factor than the attentiveness of a.

There is a further act of contamination that is theoretically possible. Just as c corrected C to agree with Aa, a could have further corrected A to agree with Cc. Such cross-contamination would be virtually undetectable, since it is not needed to explain the resemblance of G to Aa.

For the editing of Quintilian, the analysis of the relations presented here is likely to make little difference. Winterbottom has reported the readings of G when A has perished or is obscure or erased, as well as those unique readings of G that have found favor with previous editors, but has accepted G's divergences from A into the text only where he judged them to be good conjectures. Since G rarely offers transmitted readings not available in A or a (except where A has perished or is illegible), the selectivity in reporting its readings is appropriate. And if there are good readings of G that have been unjustly slighted by Winterbottom, they do not leap to the eye.

The importance of the analysis is rather for the student of the theory of stemmatics. It is salutary to identify circumstances in which a codex may falsely give the appearance of being a *descriptus*.

Another aspect of the analysis of interest to the student of textual transmission is the implication that the text of Quintilian is founded on medieval conjecture to a much greater extent than has been believed. It often happens that a gives a correct reading where A and G share an obvious error. If Winterbottom's stemma were correct, the natural assumption would usually be that a transmits *a* accurately, while A has an accidental error, which was copied from A by G. Now it appears that usually the consensus of AG establishes *a*.²⁸ There are then three hypothetically reasonable explanations for a's correct readings: (1) they are conjectures of a; or (2) they indicate descent of a not from *a* but from *a*'s source;²⁹ or (3) *a* itself had corrections or variants which a reproduces. I can think of good arguments both for and against hypothesis (2), but I lack the firsthand contact with the MSS

28. I have not forgotten that when G follows c it is a *descriptus*. But I doubt that c operated before a corrected A. Therefore erased or obscured readings of A found in G probably reached it from *a*.

29. I include under this correction to a contaminated source: e.g., a source contaminated with B or its family. But for this last to be true, A must sometimes be right where Ba share errors.

to pursue the question properly.³⁰ Hypothesis (3) cannot be dismissed as a partial explanation, but is unlikely to supply a complete explanation—the quantity of readings in question is too great in which A and G agree on nonsense, while a alone has the plausible reading. Hypothesis (1) is, by everyone's agreement, sometimes correct. Winterbottom regarded many of a's readings as conjectures, and so readily claimed G as a descendant of Aa when G reproduced them. So he called a's reading at 5. 14. 21 (examined at the beginning of this paper) "typical of a's independent conjectures."³¹ I agree. We catch a in conjectures when he errs, as in most of his corrections of Greek words. With a recognition that readings of AG or Ab were likely to have been in the text of a, the way is clear to give a's scribe credit also for a good many correct conjectures.

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30. The most important argument in its favor is Winterbottom's claim (*Problems in Quintilian*, p. 7): "It can be said in general . . . that a is so often right against A¹G that it must be using something more impressive than an exemplar of G." A proper evaluation would weigh the quantity and difficulty of a's corrections against the quantity and quality of his erroneous but plausible readings (such as 5. 14. 21, with which this paper began). It is noteworthy that Winterbottom's statement refers only to the *quantity* of a's unique correct readings, and does not claim that any could not have been reached by conjecture. They reflect generally change of implausible readings to plausible ones, and not, as we often find when a scribe has corrected from a diverse source, the presenting of implausible variants. The general pattern suggests conjecture, but a definitive assessment is risky without control of the totality of evidence.

31. *Problems in Quintilian*, p. 8.