

The Hersfeldensis and the Fuldensis of Ammianus Marcellinus: A Reconsideration

I INTRODUCTION

It is generally agreed that, of the sixteen extant manuscripts of the *Res Gestae* of Ammianus Marcellinus, only two, both Carolingian, have any independent authority for reconstructing the text. One of these, from Fulda, was rediscovered by Poggio Bracciolini in 1417 (the same year as his recovery of Lucretius), was brought to Italy, and remains in the Vatican as *Vat. Lat.* 1473 = V.¹ The Fuldensis is the ancestor of the fourteen manuscripts from the fifteenth and sixteenth centuries. Poggio also learned of an ancient manuscript of Ammianus at the abbey of Hersfeld, but there is no evidence that he ever acquired it.² In 1533, the abbot of Hersfeld lent this manuscript, which already lacked at least book 31 and the end of book 30, to the publisher Hieronymus Froben of Basel, for use in an edition overseen by Froben's corrector Sigismundus Gelenius (Zigmund Hrubý z Jelení): although Gelenius patently relied heavily both on earlier editions and on his own conjectural skills, unmistakable evidence of the Hersfeldensis' contribution comes in the restoration of a long passage of Greek at 17.4.18-23, omitted by the Vaticanus and its descendants, and of eleven further scattered lines of authentic text omitted by V.³ The reason we need evidence for the Hersfeldensis'

¹ This manuscript can be seen online at http://digi.vatlib.it/view/MSS_Vat.lat.1873/.

² Poggio, *Letters* 1.73 Harth (1.3.12 Tonelli) and 1.28 Harth (1.3.29 Tonelli).

³ Cf. Froben's introduction: 'Nos nacti uetustum exemplar manu descriptum innumera loca castigauimus, lacunas aliquot expleuimus, scribarum incuria praetermissas, et in his uersus interdum integros restituimus, graeca omnia quae uel prorsus aberant, uel sic aderant, ut frustra adessent... reposuimus.' The work ended at 30.9.6, with the editorial note 'reliqua in archetypo desiderantur'. On Gelenius see Petitmengin (2006); and on his method in editing Ammianus most recently Den Hengst (2010). The restored lines of text are at 22.1.2-3, 22.10.3, 22.16.7, 24.6.10-11, 24.7.2, 24.7.3, 25.4.18,

contribution is that no scholar saw it again until six more-or-less complete pages from books 23, 28, and 30 were rediscovered and published in 1876. The manuscript had been taken apart in the late sixteenth century; the pages that were rediscovered had been used for binding account books in the castle of Friedewald, seven miles from the monastery of Hersfeld.⁴ The six pages were found, and for half a century remained, in the archive at Marburg (hence the siglum M); in 1923 they were moved to the Landesbibliothek at Kassel. In the 1980s, part of a bifolium from book 18 of the Hersfeldensis was discovered, also at Kassel, in the binding of a manuscript of Ps.-Paracelsus and other works assembled before 1603. These fragments were published in 1990, without provoking much reaction.⁵

26.7.16, 27.3.3, 30.5.11 (the second passage includes 83 characters, presumably 2 lines; the rest are between 40 and 49 characters long).

⁴ Nissen (1876); Kassel Landesbibliothek 2° Ms. philol. 27. Robinson (1936) published photographs of these pages, but they can now most clearly and conveniently be viewed at http://orka.bibliothek.uni-kassel.de/viewer/image/1336391032501/1/LOG_0000/. Given the discovery of additional fragments (see next note), we number these pages not, as Robinson did, by giving each side a number from I to XII, but rather as the third to eighth of the eight surviving folios: 3r (formerly I) = 23.6.37-41, 3v (II) = 23.6.41-45; 4r (III) = 28.4.21-25, 4v (IV) = 28.4.25-29; 5r (V) = 28.4.30-33, 5v (VI) = 28.4.34-5.2 (the first seven lines on each side have been cut from this folium); 6r (VII) = 28.5.11-61, 6v (VIII) = 28.6.1-5; 7r (IX) = 30.2.5-10, 7v (X) = 30.2.10-3.2 (this folium, which with f. 8 formed the central bifolium of a gathering, has been cut vertically so that about a third of the text is lost, at the start of the line recto and at the end of the line verso); 8r (XI) = 30.3.2-5, 8v (XII) = 30.3.5-4.2.

⁵ The fragments, for which we also use the siglum M, cover individual words from 18.5.1 (1r) and 3 (1v), 18.6.12-15 (2r), and 16-17 (2v); they form a bifolium which, one can calculate from the gap between the passages, would have represented the third and sixth folios of a quaternion. See Broszinski and Teitler (1990), with photographs; shelf mark 4° Ms. chem. 31. Superior images (which

The two manuscripts are very close in both text and date. M, written on vellum in a handsome script, seems to be in the same principal hand in all the extant sections,⁶ in books 18, 23, 28, and 30, with a few unobtrusive changes by subsequent hands.⁷ The page size was originally ca. 31x24 cm with a written area of just under 20x15 cm. There are 24 lines per side (f. 7r has 23), of an average of 46 characters per line, with a range from 35 to 56; the average number of characters per page is just under 1100. The extant text is relatively free of abbreviations other than *q.* for *que* in the later folios, and suprascript final *m*, usually at line end. V is written on vellum (208 folios of an original 210⁸) by a number of different scribes of varying ability, and contrasts in its functional appearance to the calligraphy of the

enable somewhat more letters to be deciphered than by Broszinski and Teitler) can be found at http://orka.bibliothek.uni-kassel.de/viewer/image/1340964087401/386/LOG_0028/. For a fresh transcription see the Appendix below.

⁶ Thus Robinson (1936) 119 and n. 14, contra Nissen (1876) 15, who identified three different hands: however, what he really seemed to identify was different practice in abbreviation (*q.* for *que* is used in the fragments from book 30 but not before). Letter forms and line-lengths (of which more below) suggest the same scribe in all four sections.

⁷ Most of these are not too long after the original (on these more below), but one particular set of corrections in a renaissance hand coincide with Gelenius' edition, are consistent with his handwriting, and include a shared error (M 4v, lines 13-14 (28.4.26): *bos* to *bonum*, *nouunt* to *norunt*, and (the error) *fructuosum sit* to *fructuosum. sic*). In all of our collations, M^a indicates the earliest corrector of M, M^b any of the several later.

⁸ A bifolium in the final book (covering 31.8.5-10.18) was lost after extant copies had been made but before folio numbers were added, between the current folios 200 and 201 (Pellegrin (1991), against earlier scholarship which refers to the loss of a folium). This was the middle bifolium of a gathering of three sheets running from f. 199 to the current f. 202: we thank Dr Matthew Hoskin for checking this detail.

surviving parts of M. The manuscript was checked in the period when it was written, and seemingly against the exemplar, since a number of missing lines are restored in the margins, missing words restored in the margins or in lacunae left in the text, and other small corrections carried out. There are also plentiful humanist variant readings, marginalia, and clarifications of punctuation, letters, and word division, in hands including those of Poggio, Niccolò Niccoli, and Biondo Flavio: there is no evidence that the humanist variants are anything but conjectural.⁹ The page size is 28x24 cm, and the written area about 20x18 cm. There are usually 27 or 28 lines a page, with a considerably higher number of characters per line than M and between one and half times and twice the characters per page, depending on the scribe. On the whole, the scribes of V are considerably more prone to use of abbreviations than M, including some curiosities that caused havoc in renaissance copies.¹⁰ One notable feature of both manuscripts is that spaces of varying lengths are left to indicate lacunae in the text, which in V can be of a few letters or several lines (for example, in book 29, V has a succession of lacunae of ca. 3 lines each¹¹). As a rule, the spaces in M have the same basic shape and approximate length as in the equivalent part of V.¹² As far as the date of the two mss is concerned, palaeographical judgments have for the most part placed them very close to

⁹ On second hands from the renaissance see Cappelletto (1978), (1981), and (1983, 165-74; on the conjectural nature of these texts see the brief remarks of Baehrens (1925) 56-57 and Seyfarth (1962) 8-9.

¹⁰ E.g. *Ir* for *autem*, *∅* for *eius*. See Clark (1904) 55-58.

¹¹ Cf. Kelly (2015).

¹² For some exceptions, however, see III.1 below.

each other, with M perhaps associated with a very slightly later period. Thus, Bischoff dates V closely to the first third of the ninth century and M to the first half.¹³

Palaeographical consensus, then, places the two manuscripts close enough in time for such criteria to be effectively ignored in assessing their relationship. As well as being close in date and provenance, they are also very close in their text, being full of shared errors and lacking major divergences. There have always been two main possibilities for their relationship, a topic debated even before the rediscovery of the six pages of the Hersfeldensis¹⁴: either that M and V were direct copies of the same exemplar, or that V was a direct copy of M. The reverse relationship is ruled out by Gelenius' addition from M of authentic passages not in V. (A third possibility, that V is descended from M through one or more intermediaries, has never been seriously entertained.) Both of the main possibilities had their advocates, the former winning the support of Charles U. Clark in his great edition of 1910-1915.¹⁵ In 1936, however, R.P. Robinson published an article in *University of Missouri Studies* which argued in detail for the latter case; and his arguments have been taken as conclusive by all who have considered the question since, in particular the Teubner editor Wolfgang Seyfarth, and have been canonized by Leighton Reynolds' judgment in *Texts and*

¹³ See Bischoff (1998-2014) 3.6862 for V, and 1.1798 for M. On the first discovery of the fragments of the Hersfeldensis, a wider range of possible dates going as late as the twelfth century was suggested by the experts consulted by Nissen (1876) 15-18.

¹⁴ As already clearly expounded by Mommsen (1872) 231-232, among others.

¹⁵ Clark (1910) iv-v; cf. the arguments (incisive but unfortunately curt) of Clark's collaborator Traube (1903), and Clark's own very brief discussion (1904, 62-63). The argument is accepted e.g. by Pasquali (1934) 81 and n.3 (and subsequent editions).

Transmission that ‘Robinson demonstrated as cogently as could be demonstrated that V was copied from M’.¹⁶

Robinson could offer no single knockout argument. His method was a close collation of the passages of the entire extant portions of M against V. In places where they disagreed (trivially for the most part) and one reading was clearly better than the other, he found that M preserved, or was closer to, the truth in 50 cases out of 55. The other five cases are as follows:

prestant V] prestant M (23.6.39, R. v)
applicatos V] aplicatos M (28.4.33, R. lv)
penetrari V] paenetrari M (30.3.3, R. cvii)
ut apparebat V] ita parebat M (30.3.4, R. cxii)
frangeretur (-&ur) V] frangerentur M (30.4.1, R. cxxii)

Rightly dismissing the first three of these as orthographical trivialities, he argued that the other two could be seen as easy corrections. He went on to suggest various places in which idiosyncratic details in the physical appearance of M could have led to error in V, and to argue that other errors in V need not imply an archetype other than M. After some comments on word division (or its absence), he concluded by suggesting that the length of lines of the archetype of V, as inferred from passages of text omitted in copying and then inserted in the margin, was consistent with what can be inferred about the length of lines in M. In what follows we shall add the evidence of the new fragmentary bifolium from book 18, showing that it offers further cases of error in M that are not found in V (Section II; see also the appendix). We shall then go through the case first made by Robinson point by point, and show that V, in fact, cannot be a direct copy of M (Section III); then we shall see what can be deduced about the archetype and the production of our two manuscripts (Section IV); and at

¹⁶ Robinson (1936); Seyfarth (1962) 8 and n.1 with a supplementary argument discussed further below; Seyfarth (1978) vii; Reynolds (1983) 7.

last examine what conclusions arise from revising the theory of the two manuscripts' relationship (Section V).

II THE NEW FRAGMENTS

Since Robinson's case is based on the quality of the text in the six pages of M he knew, the discovery of the new fragments calls for a reappraisal of his argument. Broszinski and Teitler did not conduct such a reappraisal in spite of the fact that they uncovered two more errors in M not in V in a passage of just eighty words in length.

18.6.13 per pabulum V] per rabulum M

18.6.15 lampade V] lampadae M

Lampadae, they claim with some justice, is a mere orthographic error.¹⁷ This is true, though it is indicative of a more elementary confusion than something like *paenetrari* for *penetrari* (30.3.3). *Per rabulum* is certainly contained in M; what V originally read is harder to discern as there is later manipulation. Broszinski and Teitler follow Clark, who thought that V's original reading was *rabulum*. Clark, however, ignored the existence of an erased ascender on the first letter. It seems more likely that V originally contained *babulum*, as Eyssenhardt hesitantly suggested.¹⁸ Regardless, however, the reading *rabulum* in M is revealing; it suggests that the archetype of M and V was in a script where *r* and *p* (and possibly *b*) could be confused.

Moreover, Broszinski and Teitler's transcription is capable of improvement. Based on the high resolution facsimile made publicly available by the Universitätsbibliothek Kassel, we can recover a number of letters that they were not able to see (see appendix below for a full transcription) and have identified one additional error in M which they missed: 18.5.1

¹⁷ Broszinski and Teitler (1990) 423.

¹⁸ Eyssenhardt (1871) 133.

calcitraret V] /trare M. As can be seen in the detail in Figure 1: the *e* is followed by a punctus and then the majuscule *f* of *flexus*. Above the *e* to the right, one can see a mark that could conceivably be Gelenius's insertion of a *t* (compare for example his minute corrections in M 4v, ll. 13-14; see n. 7 above). It is certainly not a contemporary correction.

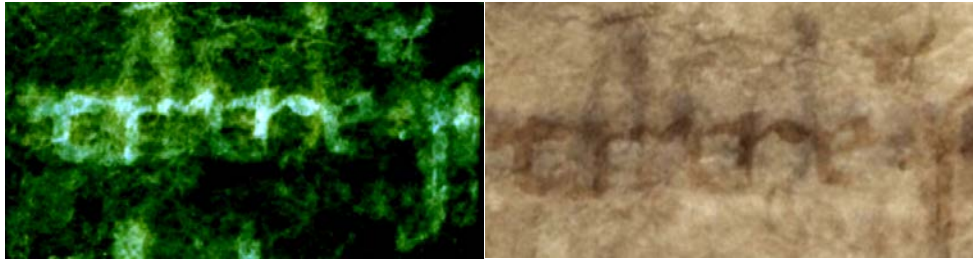


Figure 1: M 1r, detail, with and without colour enhancement

Adding this new error gives us three mistakes in M not in V in the new fragment, about one every twenty-five words. If we add these fragments to Robinson's assessment of those discovered earlier, the proportion of places where V has a better reading than M rises from 5 in 55 to 8 in 58; instead of being ten times as accurate, M is only six times more accurate than V (and we shall later see reasons to further lower the multiple). If the new fragments are more typical of M as a whole than the old, V should have thousands of correct readings not in M. Hence, what Robinson saw as the 'unbelievable accuracy' of M against V does not seem so sure: it might actually be a reflection of varying levels of competence among V's scribes. As it happens, the scribe of V for the passages in book 18 (the second scribe overall, whose contribution runs from 14.6.25 to 19.2.6) does seem to be the most consistently accurate, with comparatively few lines omitted and restored in the margin, and his distinctively spidery hand, easily distinguished from the many other scribes, reappears later correcting the work of his successors. At any rate, the impact of the new fragments should certainly make us examine the rest of Robinson's arguments more closely.

III IS V AN APOGRAPH OF M?

The case Robinson makes is twofold: after demonstrating the overwhelming evidence for M's textual superiority, he tries to show from M that V must have been directly copied from it, using the evidence of letter-forms, physical features, and scribal practice. His arguments have subsequently been supplemented by Seyfarth. We demonstrate that a close examination of the evidence not only does not prove V's direct descent from M, but indeed suggests the opposite.

1. *Lacunae*

Potentially the most compelling of Robinson's arguments is his claim that V omits lacunae that occur at the end of the line in M. Unfortunately, he can only come up with two examples, and one of the examples, at 28.4.26 (4v, l. 13 = R. xli) *bos nouunt* for *bonum norunt*, is so short, only two characters long, that it might not be a lacuna, as Robinson admits (129 n. 53), and even if it is, could, given its brevity, just have easily been missed in the middle as at the end of a line. There are also lacunae at line end in M which are likewise present in V (e. g. M 4r, l. 11, after 28.4.23 *licet* and V, f. 163v, l. 24). This leaves just one case, just above in the same section 28.4.26 (M 4v, l. 12 = R. xxxvii) – and all this single example would signify is that V missed a lacuna in its archetype. The fact that it is at the end of the line in M could be mere coincidence. More troublingly, the reading adopted by both Clark and Seyfarth is a reconstruction by Heraeus, *ancillas suapte natura pallidior spiritu*,¹⁹ a reading that more closely follows V, which has no lacuna between *pallidi* and *aspirati*, than M. Whether Heraeus' conjecture is correct is another matter (no continuous sense can be extracted from the passage), but there is no definitive evidence in favour of the lacuna. It can hardly be incidental that V omits a lacuna where the text possibly requires none. Indeed it seems equally possible that M introduced a lacuna where the archetype had none. Further,

¹⁹ MV offer the words *ancillas capte pallidi aspirati*, M with a five-letter lacuna after *pallidi* at the end of the line, V without lacuna.

there may be other lacunae in M which are not in V; for example, in the last line of f. 7r, M has a space of about six letters after the *punctus* before the new period beginning *Remigius* (30.2.10), which is much longer than the space of one or two letters it usually leaves after a *punctus*. V, by contrast, leaves a space of only three characters (f. 185r, l. 19), as it usually does after a *punctus*. It looks like M has indicated a non-existent *lacuna*, not to be found in V.

2. Letter forms

Some of Robinson's remaining arguments rest on faulty analysis of the writing of M. In 23.6.38 (R. i) *usus*] *usus* M *usue* V, Robinson claims that there is 'a blot or some kind of blemish' (129) on the *s* which makes it look like an *e* (M 3r, l. 4). In reality (as Robinson feared), it is a trick of black and white photography, where the writing on the reverse of the folio seems to bleed through under a flash, and becomes indistinguishable from the writing on the front. The colour facsimiles clearly show that the *s* (an *s-longa*) looks exactly as it should, and V simply made an error, possibly due to confusion of letter forms (in some scripts, *s* can be confused with *e*), but only if V was not copied from M. In 30.2.8 (R. lxxxviii), *si ad ea*, M reads *sieadea* (with a deletion mark under the first *e*) and V *si eadea*. Robinson considers this the 'most convincing single bit of evidence we have' (130). The idea that the scribe of V mistook a deletion mark under an *e* for an *e-caudata* is clever, but improbable. The mark in M (M 7r, l. 18) does not look like a *cauda*, but a *punctus*, separated as it is from the letter. Our eyes, which have been trained on the appearance of the printed page, might consider that a scant difference, but it would have been immediately apparent to a medieval scribe. Robinson does not consider the obvious solution: the archetype read *sieadea* (arising from a correction from *si eadem* to *si ad ea* – confusion of *ad ea* and *eadem* is very common). The superfluous *e* was deleted by M (probably M^a) *ex ope ingenii*; V, less *ingeniosus* by all accounts, hypercorrected it to *e-caudata*. Likewise, in 30.2.12 (M 7v, l. 18 = R. xcvi), the scribe of M had dipped his pen before finishing off the *e* in *urgente*; the result is

not elegant, but it certainly does not look like an *æ*. V's *urgentē* is yet another hypercorrection. The shape of *rg* in 28.4.27 (M 4v, l. 18 = R. xlii) *turgidos* does not look like *ng*, nor does M's *xt* of 28.6.1 (M 6v, l. 2 = R. lxxviii) *textus* look like an *at*. In the latter case, a later crease in the parchment generates the illusion of an irregular letter formation. Indeed, *a* for *x* is an attested confusion, as for example in the manuscripts of Lucretius, 3.596 *exsanguis] easanguis* and 6.385, *extulerit] eatulerit (fa-)*.²⁰ In fact, exactly the same confusion a little earlier in both M and V, 28.4.26 *fatorum] extorruith* MV. Further at the occurrence of the same word *textus* in 28.1.17, V has the *x* superimposed upon another letter, quite possibly an *a*. Hence, neither of these two examples gives any support to the idea that V's readings, *tungidos* and *teatus* respectively, were derived from aberrations in M. In a final case, at 30.3.2 (R. cii) *gestorum*, Robinson finds what he calls 'a very striking bit of evidence' (130), where M's supposed *o* looks like an *e*, thereby giving rise to V's *gesterum*. In fact, M's *o* is simply an *e* (M 7v, l. 23), lacking the extended tongue, to be sure, but formed with a single stroke, unlike the double stroke of M's *o*. The *te* here is identical to the *te* found a line below in *teme(rare)*. Hence, the archetype read *gesterum*.²¹ All of these examples show that Robinson's arguments from letter forms in M simply do not withstand close analysis, particularly with the aid of high-resolution colour facsimiles.

The rest of Robinson's arguments from letter forms are simply *ad hoc* explanations of how particular corruptions could have arisen if V were copied from M, without providing any proof at all that V was in fact copied from M. Their probative value is nil, and since the

²⁰ See Merrill (1926) 43 and 100. Other cases may be found in Pliny the Elder, 29.72 *auxilium] aua illum*, 34.87 *perixyomenon] perlayomenon* and 35.174 *in expolitano] in neapolitano*. We thank anonymous referee of this journal for pointing out these readings.

²¹ This reading (R. cii) should thus be subtracted from the list of fifty where Robinson saw M as superior to V.

archetype did in fact have the same text, and fairly similar line length (see below), such explanations could just as well fit the archetype as M.

3. *Punctuation and word separation*

Punctuation and word-separation can be dealt with briefly. Since the pioneering studies of Parkes and Saenger, we have a much more nuanced and accurate understanding of their employment in Carolingian manuscripts.²² Punctuation and word-division in ninth- and tenth-century manuscripts depend on how the scribes literally read the text, that is, how they grouped together units of words, as they orally pronounced the lines they were writing. No evidence can therefore be gleaned from these areas to establish the priority of M, and the new fragments offer cases where M divides and V does not (2v, l. 14, in appendix below) and vice versa (2r, ll. 2, 9, and 15).

4. *Stichometry*

Robinson's final argument rests upon stichometry, or the line lengths in V's archetype and M. We are in the unique position of having a very good idea of V's archetype's line lengths, due to the negligence of V's scribes and the diligence with which it was corrected. For example, on f. 92r, l. 10 (22.8.14), V has the nonsense *admaxionem*. But the corrector has deleted *adma* with deletion points, and supplied in the upper margin, *h[oc est] ad mare ionium permeavit; dextrum igitur infle*. What the corrector is telling us is that the original scribe skipped a line, i. e.:

ad ma
re ionium permeavit; dextrum igitur infle
xionem . . .

Further evidence for line lengths in V includes other marginal additions that have the same general length, but do not break up individual words. Finally, we also have the evidence of

²² Parkes (1993) and Saenger (1997).

Gelenius' edition, in which he supplied (as Froben tells us) 'whole lines' which were missing in V from the Hersfeldensis.²³ Taking all these categories together Robinson gathers 59 examples of lines from V's archetype, falling within the range of 35 to 49 characters per line, with an average of 42. Likewise, he counts the number of characters in the lines from the extant fragments of M, and finds 264 lines, with an average of 46 characters. That, according to Robinson, is close enough to assert the identity of M with the archetype of V, considering how small a sample from the whole we have access to.²⁴

With the important exception of this final assertion, we have no quarrel with Robinson's methodology. He was meticulous in taking account of erasures in the main text as well as marginal lines to work out the precise length of lines of the archetype preserved in V's margins. He was also prudent in excluding some omissions restored in the margins that were anomalous, mostly shorter ones that could be explained as cases of *saut du même au même*,²⁵ and likewise in distinguishing between omissions that broke up words as against those that did not, though in practice the range and average length of the reconstructed lines prove to be virtually identical. After checking his work, adding a line from M and another line from the margins of V that he missed, and a further three cases where transpositions in the text, corrected by the scribes, can clearly be seen as the result of skipping a line, we

²³ See n.3 above.

²⁴ Robinson (1936) 136-139.

²⁵ E.g. at 17.11.1, where a 57-character marginal passage is also found accurately in the text; at 21.9.5, 22.15.3, 30.8.11, he left out of consideration passages of thirty letters or under, all explicable as *saut du même au même*. He also discounted two longer passages (16.9.3, 116 characters; 20.3.9, 190 characters).

reached a very similar result to his.²⁶ From 64 lines we found the same range of 35 to 49 letters and calculated an average length for lines in the archetype of V of 41.8. Of course some of these omissions may have occurred in other ways than a single line of text being omitted, not only the obvious *saut du même au même* but also the copyist's eye dropping a line in the middle of the page. Reassuring, however, is the fact that, if cases where *même au même* is a likely explanation are excluded, the average line-length of our sample is virtually identical.²⁷ Similarly, if we consider only cases where the omission of a line included breaking up a word – and therefore almost certainly a product of line-omission – it produces a similar range of 35 to 46 and a very similar mean line-length of 41.5. LINE-LENGTHS PAL
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In the case of M, we can add line-lengths from the newly discovered passages: these are reconstructed, in that the lines are only partial, but it is clear the missing text can vary only minimally in length from that preserved in V. Adding 38 lines, we reach a total of the

²⁶ The omitted line restored by Gelenius is at 22.1.2-3 (43 characters). The omitted marginal line from V is at 22.12.6: *sua diuersa oria* [i.e. *diuersoria*] *portarentur petulantes ante*. The three other cases are 1) 21.1.2, where the scribe skipped from the word *alliceret* to the word *prior*, supplying later in the sentence the omitted words *in concordiam an terroris incutiendi gratia lacesseret*, and using supralinear marks to indicate the transposition (48 characters: obviously a case of *saut du même au même*, but easier if the two similar words were at the end of a line); 2) 21.2.20, where in similar fashion *-ebus exploratiuos* [i.e. *exploratius*] *spectato negotium Mamertino* was omitted, the initial *di-* of *diebus* was joined to the *tum* a line down which was read as *dirum* (41 characters); 3) 22.4.6, where the scribe appears to start copying the same line he has just copied (*sit uideri subiecto adeo autem ferox erat in suos illius*, before deleting it again: 47 characters). There may be other such passages that we have yet to identify.

²⁷ 41.76 against 41.78.

304 extant or reconstructable lines of M with an average length of 46.2 letters, with a range from 35 to 56.²⁸ It would, of course, be possible to calculate the length of lines of M on the basis of the amount of text on known or reconstructable pages. Here one could add text lost in between closely positioned fragments, not only between 1r and 1v, 2r and 2v, and 5r and 5v, but also the complete missing bifolium between ff. 1 and 2, and the missing folium between ff. 5 and 6. Factoring in those pages would give an average number of letters per page of 1094 and of just over 45.5 characters per line;²⁹ but since we do not ultimately know what was or was not in those lost folios, it seems best to stick to a calculation based on lines actually preserved or closely reconstructable.

Where Robinson went wrong, however, was in his impressionistic response to the results of his survey on line-lengths: that averages of 42 and 46 were close enough. The sort of problem we have here requires a relatively straightforward statistical procedure. We have two samples – the lines from V’s archetype and the lines in the extant fragments of M – and we want to determine whether those two samples come from the same population, that is, whether V’s archetype is statistically identical to M in line length. The classic formula is Student’s *t*-test, since it is reasonable to assume that the line lengths in a manuscript would follow a normal distribution (M, unlike V, seems to be written in a single hand throughout)

²⁸ In this measurement lines with gaps marking lacunae are excluded from the count. However, where part of a line is missing but the text can be supplied from V (as in ff. 1, 2, and 7), the reconstructed lines are counted. In no case does this lead to reconstructed lines of a length outside the known range.

²⁹ Page lengths are (* indicates some degree of reconstruction) 1r 1100*; 1v and four additional intervening sides 1098* each; 2r 1055*; 2v 1048*; 3r 1040; 3v: 1009; 4r 1044*; 4v 1129*; 5r 1190*; 5v 1101; two sides between f. 5 and f. 6, 1111* each; 6r 1193; 6v 1165; 7r 1055* (NB only 23 lines); 7v 1100; 8r 1020; 8v 1118.

and the t -test has been shown to be fairly robust even in non-standard populations.³⁰ Further, we have relatively random samples: the lines of V's archetype are from throughout the text, and the fragments of M, including the new ones, are from four different books, which means that progressive variation throughout the codex should not affect our sampling. The randomness of our samples more than compensates for the small proportion of the whole they represent. The t -test is used to calculate the probability of a null hypothesis that two samples represent the same population; if the t value exceeds a certain threshold, we reject the null hypothesis. So in this case, our two populations are the line lengths of M and the line lengths of V's archetype, and the null hypothesis is that of Robinson, that they represent the same population, i. e. that M is V's archetype. For our test, the number of samples in our first set (lines in the fragments of M, including the new fragments) is 302 ($n_a=302$), and in our second (lines from V's archetype) is 56 ($n_b=56$). Performing a t -test ($t=8.367$; $p[\text{robability}] < .001$ two tailed) shows that it is extremely unlikely that the two samples derive from the same population.

But the t -test assumes normal distribution - which is a reasonable assumption, but one that has never been demonstrated for characters per line in a manuscript. Hence, we can also use a Wilcoxon rank-sum test, which determines the same thing, but does not make any strong assumptions about the distribution of the population.³¹ Like a t -test, however, the Wilcoxon rank-sum test tests the null hypothesis that the two samples come from the same

³⁰ Any standard handbook of statistics will describe Student's t test; an accessible overview can be found in the *Encyclopedia Britannica Online*, s.v. 'Student's t test' (<http://www.britannica.com/science/Students-t-test>).

³¹ Once again, any standard handbook will describe this test; see, for example, Ott and Longnecker (2016), 315-24.

population. It returns a W value, which, if is used to reject the null hypothesis if it exceeds a set value. For this text, our sample numbers are the same ($n_a=302$ and $n_b=56$) while the median values are 45.5 for n_a and 41.0 for n_b . Running the test shows us that the two groups differ significantly ($W=13841$, $p < .001$ two tailed). Hence with more than 99% certainty we can reject the null hypothesis that the two samples come from the same population; the chance that the lines from V's archetype were originally in the Hersfeldensis is less than one tenth of one percent. Hence, the stichometric arguments that Robinson found persuasive not only are not as sound as he believed but actually very strongly indicate the opposite, that V was not copied from M.

5. *Length of gatherings*

At this point, one related argument, adduced not by Robinson, but in his support, needs to be confronted. Wolfgang Seyfarth, later to edit Ammianus, argued in 1962 that conclusive support for V's dependence on M could be found in an oddity of book 29. In that book, two lengthy passages of text have been transmitted the wrong way round in the Vaticanus: the order is 29.1.1-17, 29.3.4-29.5.39, 29.1.17-29.3.4, 29.5.39-end. This is a mechanical error caused by two quaternions being bound in the wrong order at an earlier stage of transmission. If one starts from the assumption that that earlier stage was the Hersfeldensis, the fact that Gelenius was first to correct the mistake may look like confirmation: but in fact, the problem should not have been beyond a good scholar and it is rather more surprising that it was not already solved by Accursius, who published his edition, containing the *editio princeps* of book 27-31, a few months earlier than Gelenius in 1533. Seyfarth's argument is rather more sophisticated. He calculates that the length of the reversed passages is almost exactly what one would expect for a quaternion of the Hersfeldensis, which should have 17,664 letters. His estimate is that the putative quaternion 29.1.17-3.4 contains 17,690 characters. The second quaternion is harder to calculate, given the regular

series of three-line lacunae that punctuate it at beginning and end, but Seyfarth allows 1,242 characters (27 lines of the Hersfeldensis) for the lacunae and subtracts to reach a figure of 16,422 characters as target length for the remaining text, which is then matched by his estimate of the length of the text of the passage as 16,356 characters long.³²

There are two problems with this reasoning. First, his calculation of the length of the quaternions, carried out by counting lines in Clark's edition and multiplying by an estimated average, is slightly awry: 29.1.17-3.4 is in fact not 17,690 but 18,220 characters, which would imply an average of 1140 characters per side, within the range of known pages of the Hersfeldensis but markedly higher than the average.³³ Given the good reasons we have adduced to calculate that the archetype of V had an average of 42 characters per line, it seems likely that we should in fact imagine a manuscript with slightly shorter lines than the Hersfeldensis but rather more lines per page: probably 27 rather than 24. Seyfarth's calculation merely shows that the archetype had pages of a similar length to M's, if perhaps slightly longer.

6. *A curious error in V*

Further evidence for the format of the exemplar of V can be gleaned from comparing V with the new fragments of M. V has a curious mistake in book 19: it comes a page after a new scribe has taken over, aptly described in Clark's apparatus (19.2.6) as *scriba multo neglegentior*. After miscopying the first syllable of the word *pallente* (19.2.14, *bal-*), the

³² Seyfarth (1962) 8 n.1.

³³ This calculation was made on the basis of electronically counting letters in a transcription of V, expanding abbreviations to accord with the practice of M but counting & as one letter, and including punctuation only if found in V itself. We deliberately leave the second quaternion out of account, since lacunae make counting its original length a circular calculation.

scribe suddenly starts, at f. 59r, l. 14, to copy a passage from the previous book, 18.6.20-22 (*Syriis—mente quadam*). The passage is copied haphazardly for 12 lines before suddenly the scribe stops and starts again on the word, *pallente*, that he should have been copying at the start. There is no deletion of this repeated passage in V, with the result that several of its descendants copied the repeated passage and it even appeared in the first four printed editions, before being removed by Accursius and Gelenius. In what circumstances could such a bizarre error have occurred? The most plausible explanation would seem to be that the scribe stopped at the end of a page, and having not properly marked the place returned after a pause to open the exemplar, or find the exemplar open, at what was in fact the wrong page. If the exemplar was unbound – and perhaps this is likelier – the scribe may have reached the end of a quaternion and then resumed copying the start of the same gathering rather than the next one; the 17,400 characters' distance between the two passages is in fact close to the length of the two gatherings in book 29 that we have seen were misplaced at some point in transmission.³⁴

One result of the discovery of the new fragments of M from book 18 is that we can calculate where in M 18.6.20-22 stood: it would have been about two thirds of the way down the seventh folio, recto, of a quaternion. If it had been at the top of a page, or even more, at the start of a quaternion, it might have acted as a powerful argument in support of V's derivation from M. Conversely, the actual situation implies that V is probably not a copy of M. Of course, it cannot be ruled out that the dislocation happened earlier in the tradition, but the fact that this error coincides with a change in scribe in V, who would not have recognized the text from the earlier book, and who at any rate is markedly unreliable, strongly suggests that its genesis is in V itself.

³⁴ Above, Section III.4. The size will be still closer if one allows for the space taken up by the heading of book 19, quite plausibly on a fresh page.

The case for V being an apograph of M does not stand up to close scrutiny. Robinson and Seyfarth have compiled an impressive number of different and independent pieces of evidence, but none of them withstand detailed analysis and one of them – the stichometry – virtually proves the opposite, that V was not directly copied from M.

IV EVIDENCE FOR A SHARED ARCHETYPE

The evidence indicates that V was not copied directly from M. By itself, this fact does not prove that M and V are both copies of a single archetype, although some of the features we have examined seem to point in that direction, since, as we noted above, there remains the theoretical possibility that V is copied indirectly from M, that is, through one or more intermediaries. In the section, we will examine the positive evidence that M and V are both copied from a common archetype.

1. Palaeographical evidence

Robinson, in searching for cases in which errors in V could be explained palaeographically from M, overlooked the more compelling palaeographic evidence that M and V share a common archetype. In fact, we have already seen one case where M and V seem to display parallel confusion (*a* for *x*). More examples may be added:

28.5.1 (R. lviii) intento] intento M invento V

30.3.2 (R. ci) moxque veris] /que veveris M moxque veteris V

V commits a similar error in both these cases (in the latter case, the iterated *ve* is probably archetypal). In the first, V reads *te* as *ve*, and in the second *ve* as *te*. This suggests that the archetype of V was in a script in which *ve* and *te* were liable to confusion. Insular script provides the most obvious solution, with its characteristically fine crossbar on the *t*, and the

rounded first stroke common to both *t* and *v*.³⁵ Were the script fairly compressed, it would be easy to see the rounded first stroke of the *e* as the second stroke of a *v*.

It is also clear from Robinson's collation that V frequently confuses *a* for *o* and (more rarely) *o* for *a*:

- 23.6.40 (R. ix) abolito] abolito M obolito V
- 28.4.26 (R. xxxviii) consumpto] consumpto M consumpta V
- 30.2.5 (R. lxxxv) instrumenta] instrumento M instrumenta M^b V
- 30.4.1 (R. cxix) navabatur] navabatur M novabatur V
- 30.4.2 (R. cxxv) imperiale] imperiale M (M^a?) imperiole V

Insular script, and other scripts with a full round *a*, offer a convincing explanation for this error. But this error would not have consistently arisen from the Caroline *a* of M, with its small, frequently open, and sometimes almost imperceptible loop. Rather, we know this error derives from the script of the archetype, because on one occasion V gets it right and M gets it wrong, and in several further instances both M and V misread an *a* for *o* or vice versa:

- 23.6.41 (R. xii) amnes] omnes M amnes M^aV
- 28.4.23 obliquato] aliquato MV
- 28.4.27 Micionas] micaunas MV
- 30.4.1 intestina] intestino MV

The same thing can be said of the error in the new fragment 18.6.13 *per pabulum* V] *per rabulum* M.³⁶ The potential for confusion between *p* and *r* is known to be a symptom of the archetype. Whatever V originally read, M certainly saw an *r* where there should be a *p*.

The fact that here we have M making this error probably independently of V shows that V was copied from the archetype not from M.

Perhaps even more significant is V's error in 28.5.11 (R. lxvii) *ne nuda*] *nenuda* M *renuda* V. Confusion of *n* for *r* is one of the most common of all errors in minuscule, but here

³⁵ Traube was the first to propose an insular stage in the transmission of Ammianus; that stage has survived the rigorous interrogation of Dumville (1995) 211-2, who adds albeit that it was likely a continental Anglo-Saxon insular script.

³⁶ See Section II above.

M uses the majuscule form *N*. V is certainly capable of gross misreading, but it seems very suspicious that the scribe just happened to make an inexplicable slip in precisely a context where we might expect a simple palaeographic mistake. Taken together with Robinson's other examples of letter confusion (R. xlii, *ng* for *rg* and R. lxxviii, *at* for *xt*, discussed above) which actually do not seem to have an origin in the script of M, these cases suggest that many of V's errors can be explained palaeographically, but only if its archetype is *not* M.

2. *Multiple hands around lacunae*

Further, close examination of V indicates that at some points there are clear variations in the writing around the lacunae. This is most evident on f. 175, both on the verso and the recto, where the writing is clearly different around the two long lacunae.³⁷ On 175v, the main scribe left a large gap of at least seven lines. Subsequently, that scribe or another returned to the passage and copied parts of four of the missing lines. Such a practice would be very difficult to account for if the archetype was a manuscript like M with the lacunae already indicated; if, however, the archetype was physically damaged, it would make perfect sense for the scribe to have recourse to a more experienced colleague, or to return later to the same passage to see what more could be deciphered. If M were V's archetype, it must have suffered additional physical damage in the very brief period between its writing and V's. Such a supposition is extremely unlikely, especially when closer attention is paid to the relative dating of the two manuscripts.

3. *Correcting hands in M*

If V were copied from M, it must have been copied after the first corrector (M^a) was at work, as attested by the following:

23.6.41 (M 3r, l. 34 = R. xii): amnes] omnes M amnes M^aV

³⁷ The same can be seen in lacunae in the *Historia augusta*, Pal. lat. 154r-154v and Bamberg, Msc. Class. 54, f. 153r, at the beginning of the lives of the *Galieni duo*.

23.6.43 (M 3v, l. 10-11 = R. xvi): Artacana] arteana M artacana M^aV
 28.5.2 (M 5v, l. 14 = R. lix): cum milites] conmilites M, cum milites M^aV
 30.2.11 (M 7v, l. 5 = R. xci): praefectus] praefectos M, praefectus M^aV
 (M 7v, l. 6 = R. xcii): luis] ruis M, luis M^aV
 30.3.4 (M 8r, l. 16 = R. cxiii): quantoque] quante quae M quanto quae M^aV
 30.3.5 (M 8r, l. 22 = R. cxvi): tutius] tutius M totius M^aV

Now this corrector is not identical to the main scribe. For example, in M 4r, l. 6 (28.4.22), the corrector inserts another *i* over *ii* in *regiis*. Surely he did not think that the correct reading is *regiis*; rather, as Robinson suggests, he read the word as *regns*, which clearly needs an *i*. This misunderstanding of the text points to a certain distance between the corrector and the main scribe: the difference between *ii* and *n* is perfectly clear in M, since the way they are written is very different, with a prominent curve at the top in the *n*. As Caroline minuscule matured, however, the forms of *ii*, *n*, and *u* became increasingly assimilated, leading ultimately to the distinction of *i* with a stroke.

That creates a palaeographical problem. As mentioned above, V is relatively securely placed by Bischoff around the year 830, while M is placed nebulously in the first half of the ninth century.³⁸ Palaeographers seem to agree that the two manuscripts were written close together, separated by no more than a couple of decades. The date of the writing of M, however, is actually itself irrelevant to that of V, since, on this theory, V could only have been written after M was corrected. Therefore, the date of the first corrector (M^a) is the relevant point, not the date of the main hand. The letter forms of M^a suggest a date somewhat later, no earlier than the middle of the ninth century. In two places above (28.5.2 and 30.2.11 = R lix and xci), M^a has supplied a *u*. But the shape of his *u* is not the earlier wide and rounded *u* found in M and V, but verging on the later two-minim style, where there are two descenders faintly joined by clubbing at the bottom.

³⁸ See at n.12 above.



Figure 2: M 5v (colour enhanced) and M 7v, details of M^a's *u* and M's next *u* in the same line

This finding is consistent with the case discussed above (*regiis* at 27.4.22) where the corrector misread the main text of M in precisely such a way. Such a judgment is subjective surely, and we ought not rely too much on such fine-grained paleographical analysis.

Nonetheless, the fact that V could only have been copied from M after correction makes an already tight chronology less probably. Even if it is just barely possible in palaeographical terms that M was written before 830, it beggars belief to suggest that it was also corrected before that date. *A fortiori* the notion that M was written, corrected and copied, and that that copy in turn begat V, is chronologically preposterous.

But, if V is not a descendant of M, how do we explain the close agreement between M^a and V? The obvious answer is separate dependence on the archetype, but several pieces of evidence militate against this conclusion. An interesting case can be found at 30.4.1 (M 8v, l. 10 = R. cxx): *arctoum] areto um* M *aroto um* M^a *utroto um* V. The scribe of M made a simple slip of *e* for *c*, while the scribe of V seriously misread his archetype's *arc* as *utro*. Nonsense that may be, but at least it contains the recognizably Latin word *utro*, which is more than can be said for M's reading. M^a, however, changes one bit of nonsense into another by replacing the *e* with an *o*. Robinson thereby concludes (with some hesitation) that V must have been copied from M after its correction. But then where does M^a's *o* come from? He suggests

rather wildly that the archetype must have had so ill-formed a *c* that the scribe first read *e* and then changed his mind and made it an *o*. In any number of scripts, *c* can be mistaken for *e* and *c* can be mistaken for *o*, but it is unlikely that a single *c* could be mistaken for both. Rather, as elsewhere, M^a for whatever reason has imported its *o* from V. Hence, this case is very much like 30.3.5 (M 8r, l. 22 = R. cxvi): *tutius*, where M^a replaced a good reading in M with an error from V. The same can probably be said of 28.6.5 (M 6v, l. 23 = R. lxxxiii) *ducens* M, *duces* V. The correct reading is *ducens*, and that is what M seems to have contained, albeit with an untypical *ns* ligature perhaps occasioned by the line end. Unreported by Robinson and the editions is M^a's deletion mark under the first bar of the *N*; he also seems to have written over the *s* of the ligature with another, clearer *s*. This suggests unavoidably that M^a reads *duces*, the same erroneous reading found in V. Hence, all the evidence comes together to suggest that M was corrected from V – intelligently on the whole, but with a few tell-tale cases of imported corruption. In addition, four of the seven agreements between M^a and V against M (three of them true) occur in the same passage (30.2.11-3.5); three of the five errors in M not in V are found in the same passage. This clustering suggests very strongly that the scribe of M was not working to his normal standards as he copied those two folios, and his contribution is only a little more accurate than the scribe of V at this point.³⁹

There exist, however, a residue of instances where M^a provides readings not in V. They are not as numerous as Robinson supposed, but still present a potential obstacle to this conclusion:

- 28.4.22 (R. xxiv) regiis] regiis MV regnis M^a
- 28.4.25 (R. xxxii) nec ante] negante MV necante M^a
- 28.4.26 (R. xxxv) profusus] profusus MV profusus M^a
- 28.6.4 (R. lxxxii) referti] reverti MV recerti M^a

³⁹ This scribe's contribution parallels folia 4 to 8 of M in books 28 and 30. This scribe displays a shifting *ductus litterarum* and is much less accurate than some of his fellows, in particular the second scribe, whose work is superior to that of M in the new fragments. But he is also not the worst scribe at work in the codex: that crown certainly belongs to the third scribe, who starts at 19.2.6, discussed in III.6 above.

30.2.10 (R. xc) favisse] fuisse MV favisse M^a

Of these five readings, one turns truth in MV to error, another exchanges one error for another, and three are probably correct. It does not seem inappropriate to ascribe such a record to the talent of the corrector. His successful guesses are hardly brilliant, but they do show an awareness of grammatical problems in corrupt passages. Hence, the existence of non-V corrections by M^a does not preclude the hypothesis that he had V at his disposal. Instead, that there are vanishingly few correct V readings *not* imported into M by M^a makes it very likely that V is the origin of M^a's readings.

This conclusion is important for gauging the relationship between the two manuscripts. If M was corrected from V, then V has preserved many more correct readings than has been credited to it and M does not quite deserve the plaudits it has received. M still offers a vastly superior text to V: even when we add to V's credit the places where the corrector of M found the right answer in V (and to M's one where the corrector removed the correct reading), we still have only 14 places where V has a correct reading not in M, as against 49 places where M has the superior reading. However, the overall accuracy of M is enhanced thanks to the diligence of its corrector, since it also transmits the majority of V's best readings as well.

V EDITING AMMIANUS

Gehen wir von den kleinen Stücken aus die sowohl im Vaticanus (V) als in den Marburger Fragmenten (M) erhalten sind, so zeigen zunächst die Lesarten deutlich, daß weder M aus V, noch V aus M abgeschrieben ist, sondern beide Handschriften derselben Vorlage (y) folgen.⁴⁰

So Ludwig Traube in 1903. In this article we have taken issue with the view of R.P. Robinson on the relationship of the Hersfeldensis and Fuldensis of Ammianus, generally accepted since

⁴⁰ Traube (1903) 444, quoted also in Robinson (1936) 128 n. 52.

1936; both by refuting Robinson's arguments and by adducing new ones, we have shown that the two manuscripts should be seen as *gemelli*, copies of the same parent. We shall have something to say about that parent and about the history of the Hersfeldensis elsewhere; but in this conclusion it will be useful to consider what differences this conclusion makes to the study of Ammianus and of the history of texts.

First, an important consequence of this reassessment is that the dating of the two manuscripts is no longer constrained by the requirement, somewhat against the instincts of palaeographers, that M predate V. The date of the hand of M (and of its corrections) can now be revisited as a wholly palaeographical question.

What difference should it make to editions of Ammianus? Not a great deal. In purely presentational terms, the fact that M does not constitute the archetype means that when both mss are extant the apparatus ought to include the readings of both whenever they diverge, something which Seyfarth does not always do.⁴¹ In passages where M is extant, it retains its status as the more reliable manuscript, even if not the archetype; but in almost all divergences between the two, that does not affect our judgement in practice. In most of the work, where M is not extant, the effect on the text is still more limited. Logically, our reassessment has the consequence that readings of M preserved in Gelenius are not necessarily the readings of the archetype. The problem is, however, that, with few exceptions, the readings of Gelenius that differ from V cannot be demonstrated to represent those of M,⁴² and even in the exceptional

⁴¹ For example at 28.4.26, Seyfarth prints the readings of M, which for him is the archetype, and gives only the readings of V (which he judges erroneous) in the apparatus. An editor should include both authoritative mss: cultis **MG**, cultis et **VEA**; et Trulla **MG**, et contrulla **V**, et cum Trulla **EA**.

⁴² However, when Gelenius' readings are 1. the same as or very similar to those of V, 2. different from those of the previous printed editions, and 3. impossible to conjecture on the basis of those editions, we can identify his use of the Hersfeldensis.

cases (restored lines of the archetype or the entire Greek text of the obelisk inscription), Gelenius' tendency to bold emendation needs to be taken into account. We can note where readings are taken from an earlier edition, and are therefore less likely to represent M, and we can often identify Gelenius' own emendations from prose rhythm, since Gelenius was either unaware of the *cursus* or unaware of its regularity. The truth is, however, that the vast majority of times that the readings of Gelenius are accepted it is because of their superiority rather than because of their source.

Nevertheless, a better understanding of the archetype and of the likely types of corruption does have the potential to improve our texts of Ammianus. One important area is lacunae, where we can now presume that the lacunae indicated in V are the scribes' attempts to represent what they saw in a damaged archetype, and that they are not filtered through another manuscript. In cases like 28.4.26, where M has a lacuna and V does not (discussed in Section III.i above), we can choose which to believe.

Although the editorial gains are limited, this argument does touch upon questions and fields broader than the relationship of the two manuscripts to which we owe the preservation of Ammianus. That the exemplar of the *Res gestae* was copied twice within several decades in a very limited geographic area is striking. Producing even one such copy would have required a significant investment of resources – both in time and in materials – given the length of even the extant portion of the *Res gestae*; producing two indicates a very serious commitment to the study of Roman history. In fact, many late-antique histories were transmitted in the milieu of Fulda and Hersfeld in the first half of the century: the so-called *Epitome de Caesaribus*, the *Historia Augusta*, possibly Suetonius, the first six books of the *Annales* and the *opera minora* of Tacitus, to name a few.⁴³ Elucidating the relationship

⁴³ See the list in McKitterick (2004), table 9.1. On the relationship of scribal practices in the transmission of the *Epitome* and Ammianus, see Stover (forthcoming a).

between the two manuscripts of Ammianus – and particularly the fact that one manuscript was used to correct the other – sheds further light on this scholarly and scribal milieu.

Further, the methods we have employed to arrive at these conclusions will, we hope, be of use in the study of other textual traditions.

APPENDIX

Diplomatic Edition of the New Fragments of M

Thanks to the high-resolution colour facsimiles made available online by the Universitätsbibliothek Kassel, we have been able to discern more of the text in some places than Broszinski and Teitler (1990); in others, we were not confident that what they printed was actually visible. In one place, as explained above (Section II), we disagree with their reading.

The text in grey, a corrected version of V, is purely indicative of characters wholly irrecoverable because of abrasion, trimming, or physical damage. The text of the fragment is printed in bold. Individual letters are printed when enough survives to distinguish them; those that can be seen but not securely identified are italicised.

M 1r (18.5.1)

- 1 **trare.** Flexus in blanditias molliores confessusque
- 2 **debitum** per concludia in nomen fisci translatum iam
- 3 **queausurus** inmania

1 -traret V, *edd.*, trare M; a small mark above and right of the *e* could possibly represent a t; if so it is in a second hand. Could represent correction in Gelenius' hand; cf. M 4v, lines 13-14.

M 1v (18.5.3)

Tamsapore **habitis** 1
qui tractus omnes aduersos ducis potestate **tunctueba** 2

M 2r (18.6.12-15)

1 **hensum. Cum** ego rapido ictu transirem interrogassent
 2 **quisnam** profectus sit iudex audissentque Ursicinum pau
 3 **loante urbem** ingressum montem Izalam petere occiso in
 4 **dice** in unum quaesiti complures nos inrequietis cur
 5 **sibus sectabantur** quos cum iumentu agilitate prae
 6 **gressus apud** amudin munimentum infirmum disper
 7 **sis perrabulum** equis recubantes nos securius in
 8 **uenissem porrecto** extentius brachio et summitatibus
 9 **sagico** tortis elatius adesse hostes signo solito demon
 10 **strabam** Isdemque iunctus impetu communi ferebar
 11 **aequo iam fatisciente** terrebat autem nos plenilunium
 12 **Noctis & planities** supina camporum nulla si occupas
 13 **s& artior casus** latibula praebere sufficiens ubi nec arbo
 14 **res** Nec fructa nec quicquam praeter herbas humi
 15 **lesuisebatur. Ex** cogitatum est ergo ut ardente superpo
 16 **sita lampadae** & circumligata ne rueret iumentum
 17 **solum quodeam** uehebat solum sine rectore leuors
 18 **us ire** **permitteretur**

2 quisna[m] p[ro]fectus V, prouectus Valesius, edd. 4 Line almost lost due to the cutting of the folio.
 5 bant[ur] 7 per pabulum V edd. In Clark's view, V's reading was corrected from *rab*-; there is
 certainly an erased ascender, possibly an added descender; Eyssenhardt read *babulum* [ut vid.],
 corrected to *pabulum*. 9 sagi contortis V, edd. 10 –strabam. Isdemq; V 13 artior V, edd.; the ms is
 damaged where the *a* should be. 15 –les uisebatur V 16 lampade V, edd

M 2v (18.6.16-17)

Meiacarire nomine uenissemus, cui **fontes dedere uo** 1
 cabulum gelidi lapsis accolis omnibus **solum inremo** 2
 to secessu latentem inuenimus militem **quioblatus** 3
 duci et locutus uaria prae timore ideoque suspec 4
 tus adigente metu qui intentabatur **pandit rerum** 5
 integram fidem docetque quod **apud parisios na** 6
 tus in Galliis & equestri militans **turma uindictam** 7
 quondam commissi facinoris timens **ad persas abier** 8
 at profugus exindeque morum probitate **spectata** 9
 sortita coniuge liberisque susceptis **speculatorem** 10
 se missum ad nostra saepe ueros nuntios reportas**se** 11
 at nunc se a Tamsapore et Nohodare optimatibus 12
 missum qui cateruas ductauerant praedat**orum adeos** 13
 redire quae didicerat perlaturum. **Post haec ad** 14
 iectis quae agi in parte diuersa norat **occiditur Pro** 15
 inde curarum crescente sollicitudine **indepassibus ci** 16
 tis Amidam pro temporis copia uen**imus ciuitatem** 17
 postea secutis cladibus inclutam quore**uersis** explor 18

4 Line wholly lost due to the cutting of the folio. 8 p[er]sas abierat V 9 spectata V; there is an erased
 letter preceding the final *a*, but it is beyond recovery. 12 It is unfortunately impossible to see whether
 M read *Nohodareo*, the erroneous reading of V. 14 , posthaec V 17 *imuns*, the reading of
 Broszinski and Teitler, is a typo.

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