THE TILIANUS OF AUSONIUS

'There are two families of manuscripts of Ausonius, whose importance has been variously judged by scholars .... The second family ... goes back to the manuscript Vossianus Q 107 (Tilianus) of the fifteenth century, from which many other manuscripts and the editio princeps of Girardini depend.'¹) This pronouncement, some compliment to a 15th-century manuscript that omits large tracts of Greek preserved by a 14th-century relative, merely pushes to the limit a view in which editors of Ausonius are united, that within the second family 'primum sibi sine dubio vindicat locum Tilianus'²). It is time they were disabused.

The family consists principally of 21 manuscripts, only one earlier than the 15th century, that except for accidental omissions have the same collection of works in the same order³); I will call their immediate source Z. Later hands in the Tilianus have spread confusion by adding to this collection Ep. 24 and a garbled version of the Ordo Urbium Nobilium; the script of Ep. 24 struck Peiper as an imitation of longobardae litterae by the first hand⁴), and since then the rest of T has basked in reflected glory. The truth about the additions went on record in 1971⁵), and I have recently shown that the garbled version of the Ordo derives by way of the ed. Mediol. 1490 from Ambros. B 24 inf., a manuscript not of Ausonius but of a 14th-century chronicle⁶). In content, therefore, T is an ordinary descendant of Z augmented


³) Schenkl sets out these works on p. xix; for XXVII 2–11 read XXVII 3–11. Schenkl's edition is much easier to use than Peiper's, mainly because it has running titles, and my references follow it throughout.


by later hands from two sources independent of Z, and its reputation must stand or fall by its text of the works that it shares with all the other descendants of Z.

Schenkl, who knew more than Peiper about the descendants of Z, reports them most fully in the *Gratiarum Actio* (VIII), a good choice not only by reason of its length but also because the kind of error most useful for stemmatic purposes occurs more readily in prose. Unfortunately the one specific reading from which he drew a stemmatic inference, the absence of 1.4–7 verum ... ago from seven manuscripts (p. xxix), will not support it, because the passage fell out through homoeoteleuton and could have done so more than once. His other remarks about the family of Z, and indeed most of the remarks in his introduction about the relationships of manuscripts, are surprisingly primitive even for the 1880s: anything between two extremes is 'mixed' (p. xxix), and any agreements establish a close connexion (ibid. on C). Further study of Z waited until pupils of S. Prete began to write dissertations on the tradition of selected works; but to them too stemmatic method is a closed book. The only contribution made by the latest editor, A. Pastorino, is the invention of new symbols (p. 156). Hence this article.

Relationships within a family are most easily established with the aid of manuscripts outside it, and I made some progress along these lines. Two readings in works transmitted not by Z alone but also by V(P) point to a conclusion that can be sustained elsewhere:

\begin{verbatim}
Epigr. 87.2 saltavit T + 2 mss.: saltabat V cett.
XXVI 2.42 florent T + 2 mss.: fulgent VP cett.
\end{verbatim}

The two manuscripts that agree with T are these:

a) Perus. I 102, chart.

b) Laur. Ashb. 1732, membr.

Other agreements in error are *Epigr.* 19.3 iam, 59.4 haec, *Ep.* 22.1.8 adevebi, 16.1.14 et, VIII 1.6 loco, 7 tacens, 4.6 bene officii, 13 [ab], 5.11 debere deus, 6.15 subdito, 18.7 meminisset (et), XXVII 4.4 haberet, 10.2 omne viget, XXVI 1.5 amorchotheron, 21 nam, 2.43

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8) Schenkl calls the Perusinus p², which would wreak havoc in collation alongside p. The symbol for the Ashburnhamensis is Pastorino's.
9) The regularity of Ausonius's clausulae shows that this is an error.
arces, XXVIII 1.6 enim, IV 3.45 sacrae\textsuperscript{10}, XIII 1.4 eo, 2.42 capias, 45 b perlege quodcumque est memorabile ut tibi prosit, 93 inuerer, XXV 1.6 proferre. I will give the symbol $\Sigma$ to the source of Tra. If we had no other manuscripts, $r$ and $a$ would do better service than $T$, which suffers from numerous omissions and other errors; but $T$ is much the most conservative of the three. One example from many:

\begin{center}
XXI 2.69–70 post Marco tutela datur, qui scita Platonis flexit ad imperium, patre Pio melior 69 qui scita VBW: quaesita Z 70 flexit VBW: felix $T + 1$ ms.: serus ra cett.
\end{center}

The other honest manuscript in the passage I have just cited is $k$, Mus. Brit. King’s 31 (membr. a. 1475). It has four relatives, none of which offer more than excerpts:

- d Vat. Barb. Lat. 135, chart.
- g Guelf. 10.9 Aug. 4°, chart.
- j Guelf. Gud. Lat. 145, chart. a. 1445\textsuperscript{11}
- $\mu$ Ven. Marc. App. XII 8, chart.

\textit{kjg} omit XXIV 2.32–3 (d is absent), $k$d XXV 1.4 \textit{iisdem} and 4.1 \textit{pro-} (jg are absent); kjdg read XXVIII 1.31 turturis, 3.2 \textit{val(l)avi}, 3 admittitur, 4.13 \textit{Virgilium}, djg \textit{Epigr.} 9.5 \textit{dividit}, 12.3 \textit{oppressit}. Though I have not investigated $\mu$, I see no reason to doubt Schenkl’s assessment. I will give the symbol $\Lambda$ to the source of the group, but djgm offer so little of the collection that I shall talk almost exclusively about $k$.

The most sharply defined group of manuscripts is also the largest:

- b Vat. Barb. Lat. 150, \quad v Vat. Lat. 1611, chart.
  membr.
- l Laur. 33.19, chart. \quad w Vat. Lat. 3152, chart.\textsuperscript{12}
  \quad m Magl. VII 315, membr. \quad s Valentianus 141, membr.\textsuperscript{13}

\textsuperscript{10} This reading is shared by Ven. Marc. App. XIV 230, which contains only IV 3. Schenkl’s apparatus shows that it resembles $T$ (47 mixteque, 79 nec for \textit{baec}).

\textsuperscript{11} Schenkl calls the Barberinianus b\textsuperscript{2} and this manuscript g\textsuperscript{2}, but cf. n. 8.

\textsuperscript{12} Schenkl calls it v\textsuperscript{2}, but cf. n. 8.

\textsuperscript{13} This is its number in M. Gutiérrez del Caño, \textit{Catálogo de los manuscritos existentes en la biblioteca universitaria de Valencia} (Valencia 1914) I p. 49. For an illustration see T. de Marinis, \textit{La biblioteca napoletana dei re d’Aragona III} (Milan 1947) pl. 28, and for further information J. Ruysschaert, ‘\textit{Minia-'}
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It makes e.g. the following innovations in works transmitted by both Z and V: Epigr. 50.1 dignissime, XXVII 6.9 auro pretium 13.11 saepe hoc, XI 2.7 locuples, 44 parens. In the Gratiarum Actio it omits 1.4–7 verum ... ago and 3.12 enim, and its other innovations include 4.9 gratiam rectius, 6.4 aetas et fortuna tua, 10.5–6 ut ... ut ... ut ... quid ... quid ... , 17.15 natura. Relationships within it are established by omissions in the Gratiarum Actio as follows:

all except p: 2.2 tam, 6 tali, IX quas, 3.6 nec, 10 maxime, 5.13 debeo, 10.12–13 quis ... nuncupavi, 11.12 me ... prae-statur and haec est;

all except pv: 1.1 etiam, 2.6 docet securitas, 4.14 veteres illi, 6.18 quod, 13.10–11 et ... omnium, 14.12 haec, 15.11 stilo, 23 iam, 16.3 cuius, 17.5 de, 18.26 praeceptorem;

all except pv1: 2.7 testimonium, 10 omnes, 4.6 viam, 7.2 -dem, 12 -modi, 14 ergo, 9.10 sic², 10.3 sollemnis, 12.9 quam, 13.15 nunc, 15.16–17 et foris ... possem, 17.14 devo-tos, 18.19 quae;

xs(bmuy): 1.10 -ere nesc-, 2.17 et, 4.7 ipsi, 16.10 olim;

1: 3.7 aut aliena imposuerim, 16.23 humanitas, 17.11 instaurare;

w: 8.7 non splendida, 20 ditas ... quam, 16.15 stirpes ... ardebant, 17.7–8 salutiferae ... ut;

s: 7.14 te, 8.13 quam, 13.6 inter, 18.7 absentem.

I mention bmuy in brackets behind xs because I have not seen them. I have suggested elsewhere that bmu were copied from x, and I do not doubt that m and u at any rate were¹⁶). I am less sure about b, written by G.M. Cinico and therefore unlikely to be Florentine. It may be a manuscript much like s, which is very close to x without being a straightforward copy of it: IV 3.78 om. x, hab. s¹⁷). The scribe of s also wrote y; though the illu-


¹⁵) See J.J.G. Alexander and A.C. de la Mare, The Italian manuscripts in the library of Major J. R. Abbey (London 1969), pp. 77–8 and plate XXXIV.


¹⁷) Corrections made to x by another hand often appear in the text of s. If, as I suspect, s derives from a corrected copy of x + x², the corrections in this copy cannot have come from anything more respectable than the
stration of f. 1 in the catalogue reveals differences from s, it presumably belongs to this branch of the tradition 18). Unless I have underestimated y, all five of the calligraphic manuscripts in the group, bmusy, do nothing more than clutter it. Of the remainder, l and w are miserable products even in comparison with their sources, though I have more to say later about w 2. Quite the opposite applies to p and v: in both I found very few Sonderfehler. Indeed, on the evidence of the Gratiarum Actio lwx and the calligraphic manuscripts could derive from v and v at a pinch from p. Another part of the corpus forbids so simple a conclusion. From its beginning to roughly the end of the Epigrams vl share numerous errors absent from pwx, e. g. the omission of 83 and 4.7–8; towards the end of the Epigrams, therefore, a new exemplar was adopted by v, l, or the source of wx, and I cannot immediately determine which 19). Not that the matter has any importance: p by itself would do an excellent job of representing the group, and supported in emergencies by v it can refuse all offers of assistance from other quarters. It is a pity that Schenkl was so poorly informed about it 20). Let the source of the group be called $.

$ and $ produced hybrid offshoots:

- E ed. Ven. 1472
- Rav. Class. 120, cart. 22)
- e Esc. S III 25, membr. Lisbon Ajuda 52 VII 47, membr. 23)
- h Harl. 2578, chart. Clumber Collection, membr. 24)
- Holk. 324, membr. 21)

source of lwxs; cf. XIII 2. 60 deverbia pv, de nergia l, de verbia w 2 in lac., d .... gia x, denergia s. A page of b can be found illustrated in I.M. U. 5 (1962), plate XV; the text shares omissions with xs.

18) Epigr. 1. 3 compte recurs in x. The manuscript was lot 2936 at Sotheby’s on 4. 6. 74, and nothing came of my admittedly half-hearted attempt at running it to earth.

19) A gathering ends in w after Epigr. 92, in x after 95. 3, in l and v nowhere hereabouts; perhaps this points to the source of wx. The last errors of lv are 91. 3 sit haec et and 92. 4 daxomenas, the first of lwx 30. 3 nominat (30 follows 97), 100. 1 posses (possis pv), IX 16 matremque deumque, though only the last of these three need be significant.

20) ‘Descripsit hunc codicem accuratissime F. Corradini, professor Patavinus’ he writes (p. xxiii), but his own description is far from accurate: for f. 1–6 and f. 7r–26r read ff. 1 v–12 v and ff. 13r–64r.

21) See the catalogue of S. de Ried (Oxford 1932), p. 27.


All but e certainly derive from E\textsuperscript{28}). What I know about e suggests that it does too\textsuperscript{28}); Schenkl’s main argument to the contrary (p. xxvi) rests on the misconception that E repeats \textit{Epigr.} .41 after 92, and his apparatus records only one difference in the \textit{Gratiorum Actio}, 12.9 \textit{alius} for \textit{alium}. E itself, which reads with \textit{\Phi} e. g. VIII 4.9 \textit{gratiam rectius}, 6.4 \textit{aetas et fortuna tua}, shows the influence of a or a manuscript very much like it: 4.20 \textit{humani \langle} \textit{generis}, 13.5 \textit{sortiri}, 18 \textit{intus}. In origin it was probably a descendant of \textit{\Phi}, because where \textit{\Sigma} and \textit{\Phi} disagree over the order of words it only once follows \textit{\Sigma} (\textit{Ep}. 16.1.12). Being hybrid it is best ignored, even though it may have been a good representative of \textit{\Phi}: 16.15 \textit{seminaria} pE \textit{semina} vlwxsa\textsuperscript{27}). Two sets of excerpts derive from it:

\begin{itemize}
  \item Ven. Cig. Corr. 858, chart., ff. 2or–36v
  \item Ven. Cig. Corr. 2546, chart., ff. 658r–661v
\end{itemize}

Both were unknown to Schenkl, and I have seen only partial microfilms at the Institut de Recherche et d’Histoire des Textes\textsuperscript{28}). The latter reads with E \textit{Epigr}. 11.6 \textit{fortunat(a)e}, 41.1 [\textit{est}], and the former, which includes XX and Sulpicia, no doubt derives from Ugoleto’s edition\textsuperscript{28}).

Two manuscripts remain:

\begin{itemize}
  \item M Magl. Conv. Sopp. J 6 29, membr. s.xiv\textsuperscript{30})
  \item \textit{\lambda} Laur. 51.13, membr. a. 1490
\end{itemize}

24) Dr de la Mare very kindly brought this manuscript to my attention; it was lot 932 at Sotheby’s on 6. 12. 37, and in 1954 it belonged to J. I. Davis. After Ausonius (ff. 1–70) it has the \textit{Consolatio ad Liviam} (ff. 71–80) and Gregorius Tifernas (ff. 81–103).

25) On h, the Holkhamicus, and the Ravennas, see Schenkl, p. xxvi, 303; on the Lisbon manuscript, Stachniw p. 190. The mere contents of the Clumber manuscript are decisive (cf. n. 24).

26) I have a microfilm of ff. 1r–6r (\textit{Epigr}. 1. 1–19. 5).


28) I should like to thank the staff of the Institut, and in particular Mme J. Fohlen, for their helpfulness in giving me access to their files and microfilms.


30) Schenkl wrongly says that M omits \textit{Epigr}. 83 (p. xxi).
As Schenkl said (p. xxi), \( \lambda \) was copied from \( M \) before \( M \) lost leaves. Where \( M \) is missing, another descendant, \( h^2 \), offers a check on \( \lambda \) in a few places\(^{31}\).

For practical purposes, then, each of the four groups that I have distinguished can be represented by a single manuscript. \( M \) stands alone in any case, and so does \( k \) for much of the collection; \( p \) is far and away the best representative of \( \Phi \) in both conservatism and accuracy, \( T \) of \( \Sigma \) in conservatism. The least conservative of the four is \( p \); its titles alone, much longer than those in \( M \) and \( k \), show that it derives from a copy that someone had read with attention, and most people in the 14th and 15th centuries who read texts with attention emended them. I have noticed only two passages where \( p \) stays closer to the truth than \( MkT: XVII 35.7 visibis p, visibus MT, iussibus k \), for \(-ius ibis, \) and \( XXVII 4.4 resipirent p, respirent MkT, for resiperent. \) Just as \( r \) or \( a \) would provide a better text than \( T \), however, so \( p \) would be the most useful member of the whole family if only one had survived, because the other two that had as full a text, \( r \) and \( a \), have both lost leaves\(^{32}\).

So far so good; but whether any two or three of MkTp ever agree in significant error is by no means clear. I will give four illustrations of the problem:

\begin{align*}
\text{Epigr. 58.2} & \quad \text{me potius clausa subice Pasiphae} \\
& \quad \text{potius Mp: protinus kT} \\
\text{Ep. 19.1} & \quad \text{condiderat iam solis equos Tartesia Calpe} \\
& \quad \text{equos T: om. MkP} \\
\text{XXVII 9.26} & \quad \text{nota et parvorum cunis muliebre secus strix} \\
& \quad \text{secus Mk: scelus Tp} \\
\text{VIII 14.5} & \quad \text{condecentior kT: decentior Mp}
\end{align*}

In the first passage \( kT \), in the second MkP, unquestionably agree in error, but both errors are obvious and the remedy simple enough. In the third \( secus \) could have been corrupted independently by \( T \) and \( p \). In the fourth, from prose, which is the error?

One road leads nowhere. The descendants of \( Z \) date from the 14th and 15th centuries, but Paris. Lat. 18275, an anthology that includes quotations from Ausonius, proves that the arche-

\begin{footnotes}
\footnote{31) Whether Bern. 211 (Schenkl p. xxviii n. 13) is related to \( M \) or to \( k \) the eight lines of \textit{Epigr. 10 have little chance of revealing (2 silicitare k, 8 vocem k 1 for sonum).}}
\footnote{32) \( r \) has lost \textit{Epigr. 19. 4–24. 10 after f. 5. a begins after a lost leaf with XVII 30.}}
\end{footnotes}
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type of the collection, which I will call z, was at least as old as the 13th century. Not only do the quotations all come from the works contained in Z, but they almost entirely follow the same order. The Parisinus, which I will call q, cannot derive from Z: at XXI 1.40 it preserves orbis amor, which Z corrupts to a morte (a horrible emendation after orbis dropped out). An even earlier witness that shows affinity with Z is C, Cantab. Kk V 34 (s. x), which contains IV 3 and all the verse of XXVII that Z contains. C conflates versions derived from sources akin to Z and V, and readings in which it agrees with V against Z, such as its inclusion of IV 3.8–16, may have come from the source of V and not the source of Z; but in XXVII 9.23, a line omitted by Z, it gives audaces Lycii ... tamen for fallaces Li-gures ... situs, and these variants, perfectly in keeping with the divergences between V and Z, almost certainly came from a source akin to Z, perhaps z, since C and Z both omit § 12. C and Z are therefore independent descendants of z or a source yet more remote, and C, like q, can be used to distinguish tradition from innovation in the family of Z. Unfortunately the utility of both q and C is slight even for their limited contents: at most they confirm the antiquity of Epigr. 9.8 noxia (qMj¹g: noxa k²Tpv), 11.3 tu (q¹k²d²: sum q²k¹d¹mp), 11 quae (q¹kd²: sum q²d¹mp), XIII 1.15 aspera (qk²: aspera Tpvp), V 11.1 et ternos (qMT: aeternos kp), XXVII 8.2 aequoreus (CMkT: -um pV), 9.4 periturium (CMk: -rum TpV), 17 immterita (CM: -as kTpV), 26 secus (CMk: scelus Tp). The manuscripts that come off best from these passages are A (kdjg) and M. Still less can be said about Vat. Reg. Lat. 711 a (s. xi), which contains Epigr. 8 with aurea (Z) for horrida (V) in 3 and Mus. Brit. Reg. 15 B 19 (s. x) and Trevir. 1464 (s. xi), which together with other snippets contain XVII 29 with inquit (Z) for hic quis (V) in 1.

After collating MkTp throughout I am fairly certain that none

33) See Schenkl p. xxvii, where add Epigr. 91 before 93 and two anonymous epigrams (Peiper, ed. pp. 417-8) after the Protrepticus.
34) Schenkl calls it p³, but cf. n. 8.
35) I am grateful to Dr J. L. Butrica for confirming what my collation left unclear.
36) About A.D. 1000 C was at New Minster, Winchester, the precursor of Hyde Abbey; cf. Vollmer, Sitzungsber. der bayer. Akad. 1908 no. 11, P. 34.

23 Rhein. Mus. f. Philol. 121/3-4
of them preserves the truth significantly against the agreement of the other three (if any of them does, it is M) and also that MT, kp, and Tp, do not agree in significant error. Beyond that there is most hope of establishing that Mp, kT, or both pairs, agree in significant error. I begin with three transpositions:

VIII 13.1 illos versus Homericos
   versus Homericos kT: Homericos versus Mp

Ep. 21.1.1 suscepta querimonia mea
   querimonia mea Mp: mea querimonia kT
16.1.12 hi igitur interim, ut Plautus ait, erunt antelogium
   fabularum
   interim ut P. ait Mp: ut P. ait interim kT

In the first passage Homericos versus gives much the better clausula, and in the third I feel as Schenkl did that Mp put interim in a more natural place: about the second, where the clausula might again decide, I am less certain. On balance, however, these passages create a presumption against kT, which in fact share a greater number of certain if not necessarily significant errors than any other pair, e.g. Epigr. 69.4 convenit (corr. T²), 30.2 Aegyptios, Ep. 11.8 habitum, XXV 3.7 ieinum, Ep. 21.2.18 salve revisum, 22.2.13 falsis, 33 damus, 39 ego, 47 -rem, 15.37 (ad) me, 38 scripto k rescripto T, 16.1.9 fuerat, 2.9 profruar, 14 [et] melle fluente, 26 principis, 33 colit, XXI 1.16 securus, 32 (in) campis,
   Epigr. 114.25 [in], VIII 5.12 [enim], 15.16 celebritate, 18.1 sermone (corr. T²); they also omit Epigr. 79.11 longum and Ep. 16.1.58 leni, which Mp would scarcely have supplied without possessing more metrical knowledge than they reveal elsewhere. Unlike kT the other manuscripts, ra included, do not repeat Epigr. 41 after 92 in the version of V, but they may well have cancelled it in view of the earlier occurrence. Passages where Mp may or do agree in error are Epigr. 24.6 primos, Ep. 21.1.14 continuatione, VIII 2.6 participe, 14.5 decentior, 16.6 saeculares, XXVII 11.9 [est], XXVI 2.31 solis, Ep. 14.15 eut-, XXIV 1.5 denique. The most interesting of these is VIII 16.6:

neque vero unum aliquod bonum uno die praestas sed indulgentias
   singulares per singula horarum momenta multiplicas
   singulares kT: saeculares Mp

As the antitheses reside in unum aliquod/ multiplicas and uno die/ per singula horarum momenta, the adjective must be chosen on its own merits. Though I see no obvious sense for saeculares ('quar-
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tas vix quisquam credat uno saeculo praestari posse’ Vinetus),
I cannot help suspecting that singulares, even if right, is an Anticipationsfehler. The errors of Mk are mostly small and easily corrected, none worse than Epigr. 82.1 [misce] aut dissolve M solve aut misce k (d has the truth, misce aut dissolve), XXIV 2.42 [luna]. In short, I incline to postulate a common source for kT but to remain agnostic about the position of p. My advice to the next editor, therefore, is to collate all four and not exclude a reading from the apparatus unless it is wrong and three of them agree against it. The difference that such a procedure would make both to the text and to the apparatus is far from negligible. Schenkl accepts many readings of T against the agreement not just of Mkp but of Mkpra, and Peiper goes even further in devotion to it by printing such absurdities as VIII 1.7 ⟨tui⟩. Unburdened of its aberrations and also of orthographical trivialities, the apparatus would have ample space for what in my view deserves inclusion.

I have said next to nothing about corrections, and little need be said, because as far as the Latin goes they do not fog the picture. I have already spoken about the corrections in x. Everything in k and v was written by the first hand. The corrections in M, few in number, are mischievous and can be ignored. Of the corrections in r, perhaps made by the same hand at a later date, some have obliterated the original reading. The most heavily corrected manuscript is a, but the correctors have done no damage and are easily distinguished from the scribe. One of them added on ff. 54v–55v the same garbled version of the Ordo Urbium Nobilium (up to 117) as appears in the ed. Mediol. 1490, and it was either this or the ed. Ven. 1494 or 1496 that he had at his elbow40); another cites readings of Erasmus’s from the Adagia41). T and p are rather more problematical. The scribe of p erased a few words in order to introduce corrections of his own, e. g. VIII 3.14 exoret for exero et (illegible, but this is the reading of v) and 4.7 detrabere for deferre; the hand that inserted promisses before me nosti at 9.14 and 10.5 looks to me modern, and it may be the only one that does not belong to the scribe.

40) Epigr. 65, 8 subulo, 97. 2 telum, and much else. The Ordo is followed on ff. 56–7 by Claud. Carm. Min. 32, 17, 51, 13, and preceded on f. 54 by XX 35–41 and Ovidii carmina de venere et vino (Walther no. 11706); Claudian is in the first hand, the other three additions all in different hands.

41) The earliest edition known to me that quotes Ausonius under Graeca fide and Bibel elleborum is the Aldine of 1508.
About T I am uncertain; it is not clear, for instance, who added -que at *Ep.* 16.2.59. kTp all on occasion give variants that they must have found in their exemplars. The Greek is a different matter. In Mk it was written at one stretch with the Latin, though some of it is missing from k. All of it is missing from v. The various descendants of Σ, however, raise difficulties. In a it was added by another hand. In r the scribe wrote it himself, but as parts spill over into the margins and most of *Epp.* 12–13 is held back to the end, he cannot have copied both languages at once. The scribe of T omitted it all, but someone else supplied what he found room for in the *Epigrams,* and still another hand added *Epp.* 12–13 on ff. 62r–63v from the ed. Mediol. 1490. Perhaps Σ had no Greek; at any rate, it cannot be assumed without more ado that Tra all took it from the same source or that any of them took it from Σ. There is also Greek in E. I do not know who wrote the one line of it in x, *Epigr.* 33.2. That leaves w², which added from a source unlike Φ or Σ not only much of the Greek but also supplements and corrections to the Latin. The source was neither M, which e.g. omits *Ep.* 12.8 – δει χω –, nor k, which reads e.g. NYNA for KYNA in *Epigr.* 29.4, and so w² has a right to be cited, if present, wherever the apparatus records variants among MkTp: Greek and titles apart, only in fact for *Epigr.* 2.8 *laudat,* 18.5 *annos,* XVII 31.3 ⟨me⟩ *incisum,* 35.7 cinyre (+ visibilis w), *Epigr.* 62.1 *spe,* 65.6 *secus.* As for the script of the Greek, Mk² use uncial, a²rT²E minuscule, p a mixture (uncial seems to have been used by its exemplar and retained by the scribe where he could not make sense of it). The main authorities for the Greek I take to be Mk²pE, but even without r and a the stemma is not simple:

*Epigr.* 29.4  
KYNA Mk²: κῶνα T: NYNA k: νῦν δέ pE

31.1  μεν Mk²p: om. ET

Φανάσης p: Φανάσην Mk²ET

37.3  γ’ T: om. Mk²pE

5 παρ’ ἀδελφοῦ ’Ακινδόνου Schenkl: παρ’ ἀδελφῶν ἀκινδύνων T: ΠΑΠΑΚΙΝΔΥΜΟΝ ὁΝ Mk²: παρακινδύνων p: παρ’ ἀκινδύνων E

In 37 I suspect T, or rather its source, of emending.

The titles in Σ pose much the same problem as the Greek. Some of those given by Tra cannot have been in Σ; *Epigr.* 104, for instance, to which like Φ they give the title *De coniunctione*
Salmacis cum Hermaphrodito, was not separated from 103 in Σ, as the agreement of MkT¹ shows. Moreover, only in τ, where they often have a form all their own, were the titles incorporated by the first hand, and in T they first occur after Epigr. 93.6.¹²

I should be inclined to ignore the titles in Tra and also in p, where I have already commented on their length. Z surely had nothing more than brief titles like those in M and k.

Before passing to some remarks on the diffusion of the family I well set out nine passages where the variants may suggest interesting possibilities to scholars better versed in palaeography and stemmaties.

Epigr. 86  si bene quid facias, facias cito: nam cito factum gratum erit, ingratum gratia tarda facit
¹ factum <fact> k
² facit VMp: est q: venit k: redit T

Silvius esse bonus fertur pariterque Britannus
fertur pariterque M: fertur furturque T: simplex furturque k: ... furturque pra

Ep. 16.2.31  quī vincit aëvi iniuriam
vincit kp: vici tā M: viata T

VIII 10.2  divinitatis tuae prope cum piaculo verba transcurro
āpē M: proprie k: prolem Tp
5–6  ut me nosti quid familiarius
ut facere debui quid constantius
ut velle te scivi quid dici blandius potest
sic Ta: ut ... ut ... quid ... quid ...
quid ... p

12.18  nulla enim est equidem contumelia secundi
enim est equidem T perperam: equidem est k:
est equidem Mp

XXVIII 1.20–1  variis de locis sensibusque diversis quaedam carminis structura solidatur, in unum versum ut coeant aut caesi duo aut unus et sequens <medius>
cum medio
sequens Tp: sesque λk: <medius> Mommsen
repugnantibus numeris⁴³)

⁴²) Has this anything to do with the matter touched on in n. 19?
⁴³) In both senses of the word: 1½ and ½ after all make 2, not 1.
L. Müller, De re metrica (St Petersburg ²1894), p. 586, suggested in unum [versum] ut coeant aut caesi <versus> duo aut unus et sequens <medius> cum medio. However in unum ... unus et should be knocked into shape, both sense and
2.72 ... cape Maeonii carchesia Bacchi

maeoneii kTp: sacra manu λ: dona manu T

sscr.: manu Z ex meonu?

Ep. 4.44 flauus Tp: plaus Λκ: plausum p mg.

Finally the history of the family, on which I cannot shed much light. M, copied for Coluccio Salutati in 1385 or soon after and probably from a manuscript formerly owned by Pietro da Moglio, was later given by Cosimo de' Medici to San Marco. k and j were both written at Zara in Dalmatia, k in 1475, j in 1445; clearly someone had taken a manuscript over there. The home of their relative d, written between 1462 and 1477, was wherever Ioannes Petrus Laventinus lectured on Livy. Σ almost certainly belongs to the north. The vεανίσαρος from Perugia who wrote r not only says that he wrote it at Vicenza but also allows a rough date to be put on it: he can be identified with Franciscus Maturantius (c. 1443–1518), who studied at Vicenza with Omnibonus Leonicensus shortly before 1471 at the age of 25. Schenkl identified a with a manuscript in the collection of G. Saibante at Verona in the early 18th century; the text of E shows that a or something very like it was already in existence by 1472, and the script of a would suit the 1460s. The watermarks in T are northern), and as its text of Calpurnius was copied from the ed. Rom. 1471, albeit not at quite the same time as Ausonius, it cannot be much earlier than r and a. p was written by Petrus Barrocius, 'patritius Venetus', and has presumably been in Padua since his translation to the bishopric there in
1487; born in the early 1440s, he was bishop of Belluno from 1471 to 1487, and I dare say he wrote \( \Phi \) before that, though I do not know where\(^{49}\). Nothing straightforward emerges about the other descendants of \( \Phi \); \( v \) looks to me Pomponian of c. 1470, \( x \) is Florentine of about the same date (certainly not earlier than 1453)\(^{50}\), and I cannot place \( l \) or \( w \). \( E \), which I have treated as fundamentally a descendant of \( \Phi \), appeared at Venice in 1472. There is nothing about their descendants, then, that takes \( \Sigma \) or \( \Phi \) back beyond the 1460s\(^{51}\). To the 14th century and Florence belong not only \( M \) but also two lost manuscripts: Boccaccio’s, which passed to Santo Spirito, and the one consulted by Domenico Bandini, who died in 1418\(^{52}\). No readings of Boccaccio’s manuscript are known, but Bandini quotes XI, which contains enough variants for his text to be of interest.

Not having determined the home of \( Z \), let alone \( \zeta \), I have little to say about the wider stemma, and it would be nothing at all but for the obfuscations of S. Prete\(^{53}\). In the first place Prete never defines his \( Z \): is it the archetype of MkTp, the archetype of the collection, or the archetype of the readings in which the collection differs from the other families? I have called the

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\(^{49}\) On Barrocius see F. Gaeta in the *Dizionario Biografico degli Italiani* 6 (Rome 1964), pp. 510–2; he studied under Pietro Perleone of Rimini (at Venice?) and then at the University of Padua. p is no. 272 in the inventory of his books reprinted by E. Govi in *Patavinae cathedralis ecclesiae capitularis bibliotheca* (Padua 1958), p. 164.

\(^{50}\) Cf. C.Q. 71 (1977), p. 224.

\(^{51}\) Vat. Barb. Lat. 42, part of which was written in 1466 by Ludovicus Sandeus of Ferrara († 1482), contains V\( \text{f} \) 11, transmitted only by \( Z \) (and also XXI 1 and *Epigr.* 85–6 not from \( Z \)); cf. Schenkl, p. xxviii n. 13, and S. Prete, *Codices Barberiniani Latini* (Vatican 1968), p. 64 (the second poem *De mensibus*, Walther no. 14217, is not printed by Peiper on p. 99 anywhere). The rubric *Ausonii Peonii* suggests a connexion with \( E \), and only \( E a \) of the witnesses known to me have *Decembris* in 6; I doubt, therefore, whether 1466 is the date of this part. Even if it is, the specific *terminus ante quem* that it yields for \( \Phi \) or \( \Sigma \) falls too late to matter.

\(^{52}\) On Boccaccio’s manuscript, thought by Politian to be in his own hand, see most recently A. Mazza, *I.M.U.* 9 (1966), p. 59; on Bandini’s, Sabbadini, *Le scoperte dei codici latini e greci ne’ secoli XIV e XV* II (Florence 1914), p. 185 + n. 35. Before XI Bandini’s manuscript had ‘prius ... post 9 cartas ... largissimas gratiarum actiones per 4 cartas’, and it could well have been \( M \), where the *Gratiarum Actio* occupies four double pages (ff. 136v–140r); on the number of leaves missing before the last see Ullman, *loc. cit.* (n. 44). Bandini, who was on close terms with Salutati, added an index to another manuscript written by the same scribe (*ibid.* p. 201 no. 109).

\(^{53}\) *Ricerche sulla storia del testo di Ausonio* (Rome 1960) and sundry other places.
first Z and the second ζ, and I aired the possibility that C derives from the third; they may all have been identical, but Prete lumps them together in his Z without so much as drawing the distinction. Secondly, he uses the symbols V and P not just for Voss. Lat. F 111 (s. ix) and Paris. Lat. 8500 (s. xiv) but also for the families they represent. This would be a harmless equivocation if his derivation of both Z and P from V did not rest on it. Certainly in the narrower sense of V and P neither Z nor P derives from V, which has omissions that they are free from: XIII 1.14 ratio est (hab. Z), XXVI 1.5 et ... libellum non (hab. Z), 7–8 Siculis ... nihil (hab. Z), XXVII 9.19–20 quem ... aera (hab. Z), XIII 2.10–12 (hab. ZP), XXVI 1.31–2 postremo ... comparet (hab. ZP). As the first three of these omissions are shared by P (a fact I have not seen mentioned outside the apparatus), Z cannot derive from the exemplar of V either, and anything further removed from V than that is not worth discussing in the present state of knowledge. If Prete means by ‘i rapporti di dipendenza di Z da V e di P da V’ (p. 95) no more than that V preserves the truth in places where Z and P corrupt it, he should say so. Another step backwards is Prete’s suggestion that Z and GBWM derive from complementary parts of a larger collection (p. 83); a simple fact puts paid to it, that the collection would have contained two texts of the Caesares. Peiper more plausibly made the same suggestion about GBWM and P, and the contents of the lost Veronensis lend colour to it, even though Mosella was not among them.

I may as well deal here with a less important obfuscation of Prete’s. Among the sources cited by Mariangelus Accursius in his Diatribae on Ausonius (Rome 1524) are ‘fragmenta quaedam Longobardorum quandoque characteribus ... quorum inspiciendi ... mihi facultatem fecit Hieronymus ... Aleander’ (f. Kiii v). Everything that he transcribed from them also appears in Vindob. 3261 (s. xvi), where it is said to have come from ‘codice vetusto Lugdunensi qui ab Actio Sincero inventus est in Araris insula’, that is, from an old manuscript discovered by Jacopo Sannazaro on the Ile Barbe not later than 1503. Everyone agrees that the old manuscript was V, the only medieval witness to all the texts in question. Was V also the source of Aleander’s

55) Cf. Prometheus 3 (1977), p. 120.
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‘fragmenta’? Peiper and Schenkl thought so, but Prete argues otherwise. His case could scarcely be weaker. Beyond an affronted defence of Accursius’s reputation against a charge no-one has levelled, namely that Aleander’s manuscript never existed, Prete merely observes that it omitted IV 3.9 verbum, XXVII 12.12 Meandrum ... vagor (actually 11 N ... 12 vagor), and XII 2.15 (not 6) est, and read in XII 2 successere for succedere in 5, terra for Lydia (actually for tota) in 14, and non ... nec procul for nec ... nec prorsus in 29. All these readings, however, are manifest errors and no more prove that Aleander’s manuscript was independent of V than Prete’s own errors prove that he had something other than Accursius’s Diatribae in front of him. Two things establish that Aleander’s manuscript derived from V through at least one intermediary: like Vindob. 3261 Accursius ascribes the Oratio (IV 3) to Paulinus; and Vindob. 3261 offers as one elegiac poem a compilation from four (III, II, XV, XVI), which Accursius reproduces without the couplets that had been in print since 1507 (II 9–12).

I should have liked to complete this article without mentioning the problem that has brought Ausonian scholarship not indeed to a halt, though that would have been better, but into a rut from which it shows no sign of extracting itself: the problem of the divergences between Z and V(P) where they overlap. Now that I have pointed out an elementary fact about V and P, however, let me put an equally elementary question about V(P) and Z to both sides in the dispute: have they no corruptions in


58) He takes up more than a page by transcribing a note of Schenkl’s that does not concern Aleander’s manuscript and a sentence from Peiper’s preface that assumes its existence. Logic does not seem to be one of his strong points. ‘Speaking in general’ the article begins ‘we may say that only those works of ancient authors have survived to our own day which chanced to find safe harbour in the Carolingian Renaissance. This however is not true in the case of Ausonius.’ So then, Ausonius bypassed the Carolingian Renaissance. Not at all: ‘Certain manuscripts probably containing works of the poet unknown to us did survive intact up to the Carolingian period, and these works, nevertheless, subsequently disappeared’.

common? The answer implicit even in Pastorino’s very conservative text promises an entertaining discussion when the matter comes into the open.

Exeter College, Oxford       M. D. Reeve