

## 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.'<sup>1)</sup> 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'<sup>2)</sup>. 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 order<sup>3)</sup>; 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 hand<sup>4)</sup>, and since then the rest of T has basked in reflected glory. The truth about the additions went on record in 1971<sup>5)</sup>, 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 chronicle<sup>6)</sup>. In content, therefore, T is an ordinary descendant of Z augmented

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1) S. Prete in *Studien zur Textgeschichte und Textkritik* (Cologne 1959), pp. 191-2. Cf. A. Pastorino, *Opere di Decimo Magno Ausonio* (Turin 1971), p. 156: 'Vi sono poi altri codici di minore importanza. Alcuni sono copie del *Tilianus* o del *Magliabecchianus*, come...' (and he lists most of the other manuscripts).

2) Schenkl, *Mon. Germ. Hist.* V 2 (Berlin 1883), p. xxviii. Cf. Peiper, *Decimi Magni Ausonii Burdigalensis opuscula* (Leipzig 1886), p. lxxi, and Pastorino, pp. 146-7.

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.

4) 'Die handschriftliche Überlieferung des Ausonius', *Jahrb. für class. Phil.* supp. 11 (1880), p. 198, and ed. p. lxxi.

5) R. Weiss in *Classical influences on European culture A. D. 500-1500*, ed. R. R. Bolgar (Cambridge 1971), p. 69 n. 3.

6) *Prometheus* 3 (1977), pp. 112-3.

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<sup>7</sup>). 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:

*Epigr.* 87.2 *saltavit* T + 2 mss.: *saltabat* V cett.  
XXVI 2.42 *florent* T + 2 mss.: *fulgent* VP cett.

The two manuscripts that agree with T are these:

- r Perus. I 102, chart.
- a Laur. Ashb. 1732, membr.<sup>8</sup>)

Other agreements in error are *Epigr.* 19.3 *iam*, 59.4 *haec*, *Ep.* 22.1.8 *advehi*, 16.1.14 *et*, VIII 1.6 *loco*, 7 *tacens*, 4.6 *bene officii*, 13 [*ab*], 5.11 *debere deus*<sup>9</sup>), 6.15 *subdito*, 18.7 *meminisset* (<*et*>), XXVII 4.4 *haberet*, 10.2 *omne viget*, XXVI 1.5 *amorchotheron*, 21 *nam*, 2.43

7) Cf. *Prometheus* 3 (1977), p. 115 n. 19.

8) Schenkl calls the Perusinus p<sup>2</sup>, 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*<sup>10</sup>), XIII 1.4 *eo*, 2.42 *capias*, 45 b *perlege quodcumque est memorabile ut tibi prosit*, 93 *induerer*, XXV 1.6 *proferre*. I will give the symbol Σ 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:

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.*

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<sup>o</sup>, chart.  
j Guelf. Gud. Lat. 145, chart. a. 1445<sup>11</sup>)  
μ Ven. Marc. App. XII 8, chart.

kjg omit XXIV 2.32–3 (d is absent), kd XXV 1.4 *iisdem* and 4.1 *pro-* (jg are absent); kdjg read XXVIII 1.31 *turturis*, 3.2 *val(l)avi*, 3 *admittitur*, 4.13 *Virgilium*, djg *Epigr.* 9.5 *dividit*, 12.3 *oppressit*. Though I have not investigated μ, I see no reason to doubt Schenkl's assessment. I will give the symbol Λ to the source of the group, but djgμ 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, membr.	v Vat. Lat. 1611, chart.
l Laur. 33.19, chart.	w Vat. Lat. 3152, chart. <sup>12</sup> )
m Magl. VII 315, membr.	s Valentinianus 141, membr. <sup>13</sup> )

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 *haec*).

11) Schenkl calls the Barberinianus b<sup>2</sup> and this manuscript g<sup>2</sup>, but cf. n. 8.

12) Schenkl calls it v<sup>2</sup>, but cf. n. 8.

13) This is its number in M. Gutiérrez del Caño, *Catálogo de los manuscritos existentes en la biblioteca universitaria de Valencia* (Valencia 1914) I p. 49. For an illustration see T. de Marinis, *La biblioteca napoletana dei re d'Aragona* III (Milan 1947) pl. 28, and for further information J. Ruyschaert, 'Minia-

- p Patav. Capit. C64, chart.      x Laur. Conv. Sopp. 6, chart.<sup>14</sup>  
 u Vat. Urb. Lat. 649,  
     membr.                              y Abbey J. A. 3213, membr.<sup>15</sup>)

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 ... 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*, 11 *quas*<sup>1</sup>, 3.6 *nec*, 10 *maxime*, 5.13 *debeo*, 10.12–13 *quis ... nuncupavi*, 11.12 *me ... praestatur* 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 pvl: 2.7 *testimonium*, 10 *omnes*, 4.6 *viam*, 7.2 *-dem*, 12 *-modi*, 14 *ergo*, 9.10 *sic*<sup>2</sup>, 10.3 *sollemnis*, 12.9 *quam*, 13.15 *nunc*, 15.16–17 *et foris ... possem*, 17.14 *devotos*, 18.19 *quae*;  
 xs(bmuy): 1.10 *-ere nesc-*, 2.17 *et*, 4.7 *ipsi*, 16.10 *olim*;  
 l: 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 were<sup>16</sup>). 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. s<sup>17</sup>). The scribe of s also wrote y; though the illu-

turistes "romains" sous Pie II', in *Enea Silvio Piccolomini – papa Pio II* (Siena 1968), p. 278.

14) Cf. *C.Q.* 71 (1977), p. 204–5.

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.

16) Cf. *C.Q.* 71 (1977), p. 204 n. 11.

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 + x<sup>2</sup>, 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<sup>18</sup>). Unless I have underestimated y, all five of the calligraphic manuscripts in the group, bmsy, 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<sup>2</sup>. 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<sup>19</sup>). 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<sup>20</sup>). Let the source of the group be called  $\Phi$ .

$\Sigma$  and  $\Phi$  produced hybrid offshoots:

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

source of lwx; cf. XIII 2. 60 *deverbia* pv, *de nergia* l, *de verbia* w<sup>2</sup> 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* 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. 1v-12v and ff. 13r-64r.

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

22) Mazzatinti IV pp. 172-3.

23) I owe my knowledge of this manuscript to the unpublished dissertation of J. M. Stachniw, *D.A.* 32 (1971) 401-A, *The text of the Ephe-meris, Bissula, and Technopaegnon of D. Magnus Ausonius* (Loyola Univ. 1970), pp. 103-10. Cf. C. A. Ferreira, *Boletim da biblioteca da Universidade de Coimbra* 18 (1948), p. 303.

All but e certainly derive from E<sup>25</sup>). What I know about e suggests that it does too<sup>26</sup>); Schenkl's main argument to the contrary (p. xxvi) rests on the misconception that E repeats *Epigr.* 41 after 92, and his apparatus records only one difference in the *Gratiarum Actio*, 12.9 *alius* for *alium*. E itself, which reads with  $\Phi$  e. g. VIII 4.9 *gratiam rectius*, 6.4 *aetas et fortuna tua*, shows the influence of a or a manuscript very much like it: 4.20 *humani* (<*generis*>), 13.5 *sortiri*, 18 *intus*. In origin it was probably a descendant of  $\Phi$ , because where  $\Sigma$  and  $\Phi$  disagree over the order of words it only once follows  $\Sigma$  (*Ep.* 16.1.12). Being hybrid it is best ignored, even though it may have been a good representative of  $\Phi$ : 16.15 *seminaria* pE *semina* vlxwxa<sup>27</sup>). Two sets of excerpts derive from it:

Ven. Cig. Corr. 858, chart., ff. 20r-36v

Ven. Cig. Corr. 2546, chart., ff. 658r-661v

Both were unknown to Schenkl, and I have seen only partial microfilms at the Institut de Recherche et d'Histoire des Textes<sup>28</sup>). The latter reads with E *Epigr.* 11.6 *fortunat(a)e*, 41.1 [*est*], and the former, which includes XX and Sulpicia, no doubt derives from Ugoletto's edition<sup>29</sup>).

Two manuscripts remain:

M Magl. Conv. Sopp. J 6 29, membr. s.xiv<sup>30</sup>)

λ Laur. 51.13, membr. a. 1490

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 *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. 1 r-6 r (*Epigr.* 1. 1-19. 5).

27) With these remarks I take my leave of Girardinus, whose book has occupied me on two other occasions: *R. H. T.* 6 (1976), pp. 79-98, and *C. Q.* 72 (1978), p. 224. In *Die handschriftliche Überlieferung des Ausonius* (cf. n. 4), which preceded Schenkl's edition, Peiper gave pride of place to E, but Schenkl or Schenkl's discovery of M evidently persuaded him that its text was 'mixed' (ed. p. lxxii; cf. Schenkl p. xxix).

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.

29) Cf. Mirella Ferrari, *I. M. U.* 16 (1973), p. 19 n. 3. With two exceptions (n. 31, n. 51) I ignore other manuscripts of the 15th or 16th century that present odd poems from the collection; cf. Schenkl, p. xxviii n. 13.

30) Schenkl wrongly says that M omits *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<sup>31</sup>).

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 *respirent* p, *respirent* MkT, for *resiparent*. 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<sup>32</sup>).

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:

*Epigr.* 58.2 *me potius clausa subice Pasiphae*  
*potius* Mp: *protinus* kT

*Ep.* 19.1 *condiderat iam solis equos Tartesia Calpe*  
*equos* T: om. Mkp

XXVII 9.26 *nota et parvorum cunis muliebre secus strix*  
*secus* Mk: *scelus* Tp

VIII 14.5 *condacentior* kT: *decentior* Mp

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-

31) Whether Bern. 211 (Schenkl p. xxviii n. 13) is related to M or to k: the eight lines of *Epigr.* 10 have little chance of revealing (2 *silicitare* k, 8 *voce* k 1 for *sonum*).

32) r has lost *Epigr.* 19. 4-24. 10 after f. 5. a begins after a lost leaf with XVII 30.

type of the collection, which I will call  $\zeta$ , 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<sup>33</sup>). The Parisinus, which I will call q<sup>34</sup>), cannot derive from Z: at XXI 1.40 it preserves *orbis amor*<sup>35</sup>), 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<sup>36</sup>). 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<sup>37</sup>); but in XXVII 9.23, a line omitted by Z, it gives *audaces Lycii ... tamen* for *fallaces Ligures ... situs*, and these variants, perfectly in keeping with the divergences between V and Z, almost certainly came from a source akin to Z<sup>38</sup>), perhaps  $\zeta$ , since C and Z both omit § 12. C and Z are therefore independent descendants of  $\zeta$  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* (qMj<sup>1</sup>g: *noxia* kj<sup>2</sup>TpV), 11.3 *tu* (q<sup>1</sup>k<sup>2</sup>d<sup>2</sup>: *sum* q<sup>2</sup>k<sup>1</sup>d<sup>1</sup>MTp), 11 *quae* (q<sup>1</sup>kd<sup>2</sup>: *sum* q<sup>2</sup>d<sup>1</sup>MTp), XIII 1.15 *aspere* (qk $\lambda$ : *aspera* TpVP), V 11.1 *et ternos* (qMT: *aeternos* kp), XXVII 8.2 *aequoreus* (CMkT: *-um* pV), 9.4 *periurium* (CMk: *-rum* TpV), 17 *immerita* (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. 711a (s. xi), which contains *Epigr.* 8 with *aurea* (Z) for *horrida* (V) in 3<sup>39</sup>), 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 p<sup>3</sup>, 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.

37) Cf. D. Nardo, *Atti dell'Istituto Veneto* 125 (1966–7), pp. 352–4.

38) Cf. Nardo, p. 354.

39) T. Gottlieb, *Wien. Stud.* 12 (1890), p. 130.



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. T<sup>2</sup>), 30.2 *Aegyptios*, *Ep.* 11.8 *habitum*, XXV 3.7 *ieiunus*, *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. T<sup>2</sup>); 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 *continuazione*, VIII 2.6 *participe*, 14.5 *decentior*, 16.6 *saeculares*, XXVII 11.9 [est], XXVI 2.31 *solis*, *Ep.* 14.15 *ent-*, 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* ('quan-

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 elbow<sup>40</sup>); another cites readings of Erasmus's from the *Adagia*<sup>41</sup>). 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 *promississe* 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 *Bibe 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 Mkp 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 w<sup>2</sup>, 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 w<sup>2</sup> 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* (+ *visibis w*), *Epigr.* 62.1 *spe*, 65.6 *secus*. As for the script of the Greek, Mkw<sup>2</sup> use uncial, a<sup>2</sup>rT<sup>2</sup>E 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 Mkw<sup>2</sup>pE, but even without r and a the stemma is not simple:

- Epigr.* 29.4 KYNA Mw<sup>2</sup>: κίνα T: NYNA k: ρὺν δὲ pE  
 31.1 μὲν Mkw<sup>2</sup>p: om. ET  
     Φανάκης p: Φανάκη Mkw<sup>2</sup>ET  
 37.3 γ' T: om. Mkw<sup>2</sup>pE  
     5 παρ' ἀδελφοῦ Ἀκινδύνου Schenkl: παρ' ἀδελφεὸν  
     ἀκίνδυνον T: ΠΑΡΑΚΙΝΔΥΜΟΝΟΝ Mkw<sup>2</sup>:  
     παρακίνδυνον p: παρ' ἀμυδύνον 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  $\Sigma$ , as the agreement of MkT<sup>1</sup> shows. Moreover, only in  $\tau$ , 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<sup>42</sup>). 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 stemmatics.

- 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
- 110.1 *Silvius esse bonus fertur pariterque Britannus  
fertur pariterque* M: *fertur ferturque* T: *simplex  
ferturque* k: ... *ferturque* pra
- Ep.* 16.2.31 *qui vincit aevi iniuriam*  
*vincit* kp: *vici tā* M: *viata* T
- VIII 10.2 *divinitatis tuae prope cum piaculo verba trans-  
curro*  
*ppe* 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 ... 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 car-  
minis structura solidatur, in unum versum ut  
coeant aut caesi duo aut unus et sequens <medius>  
cum medio*  
*sequens* Tp: *sesque* lk: <medius> Mommsen  
*repugnantibus numeris*<sup>43</sup>)

42) Has this anything to do with the matter touched on in n. 19?

43) In both senses of the word:  $1\frac{1}{2}$  and  $\frac{1}{2}$  after all make 2, not 1. L. Müller, *De re metrica* (St Petersburg 1894), 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 ... *capē Maeonii carchesia Bacchi*  
*maeonii* kTp: *sacra manu λ: dona manu T*  
*sscr.: manu Z ex meonu?*

*Ep.* 4.44 *flavus Tp: plaus λk: 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<sup>44</sup>). 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<sup>45</sup>). Σ almost certainly belongs to the north. The *νεανίσκος* 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 Leonicensis shortly before 1471 at the age of 25<sup>46</sup>). 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<sup>47</sup>), and as its text of Calpurnius was copied from the ed. Rom. 1471, albeit not at quite the same time as Ausonius<sup>48</sup>), 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

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rhythm commend *sesque cum medio*, a reading unknown to Schenkl and Peiper.

44) See B.L.Ullman, *The humanism of Coluccio Salutati* (Padua 1963), p. 173 no. 59, pp. 268–9.

45) Cf. S.Prete, *Codices Barberiniani Latini* (Vatican 1968), p. 247, and Eubel, *Hierarchia Catholica Medii Aevi* II (Münster 1901), p. 14 + n. 1.

46) On Maturantius see G.B.Vermiglioli, *Memorie per servire alla vita di Franc. Maturanzio* (Perugia 1807); I have not been able to find G.Zappacosta, *Francesco Maturanzio* (Bergamo 1970). G.Battelli, 'Gli antichi codici di S.Pietro di Perugia', *Bollettino della deputazione di storia patria per l'Umbria* 64 (1967, pub. 1968), p. 256 no. 43, says of r 'fu del Maturanzio, in parte di sua mano'; Dr de la Mare has kindly shown me the typescript of an unpublished article on Perus. C 61 by A.Perosa, who says 'al tipo grafico della prima maniera (cioè della giovinezza) è molto vicina la scrittura del testo del primo fascicolo (ff. 1–81) del codice I 102'.

47) K. de Meyier, *Codices Vossiani Latini* II (Leyden 1975), p. 238.

48) Cf. *C.Q.* 72 (1978), p. 237.

1487; born in the early 1440s, he was bishop of Belluno from 1471 to 1487, and I dare say he wrote  $\rho$  before that, though I do not know where<sup>49</sup>). Nothing straightforward emerges about the other descendants of  $\Phi$ ;  $\nu$  looks to me Pomponian of c. 1470,  $\chi$  is Florentine of about the same date (certainly not earlier than 1453)<sup>50</sup>), and I cannot place  $\iota$  or  $\omega$ . 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<sup>51</sup>). 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<sup>52</sup>). 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<sup>53</sup>). 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

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.  $\rho$  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 ( $\dagger$  1482), contains V 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 or anywhere). The rubric *Ausonii Peonii* suggests a connexion with E, and only Ea 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<sup>2</sup> 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<sup>2</sup> and P<sup>54</sup>), and the contents of the lost Veronensis lend colour to it<sup>55</sup>), 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 inspicendi ... 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<sup>56</sup>). 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

54) *Die handschriftliche Überlieferung* (cf. n. 4), p. 313, and ed. p. xi.

55) Cf. *Prometheus* 3 (1977), p. 120.

56) Peiper, *Überlieferung* (cf. n. 4), p. 349 n. 251.

'fragmenta'? Peiper and Schenkl thought so, but Prete argues otherwise<sup>57</sup>). 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<sup>58</sup>), Prete merely observes that it omitted IV 3.9 *verbum*, XXVII 12.12 *Meandrum ... vago* (actually 11 *N ... 12 vago*), 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)<sup>59</sup>).

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

57) 'Notes on a lost manuscript of Ausonius', *Misc. Crit. Teubner* II (Leipzig 1965), pp. 287-94.

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'.

59) Prete ingeniously calls it 'the text of an autobiography of the poet (inc.: Ausonius genitor nobis - des.: commemoravit patriae, v. 58)' (p. 289).



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

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Addendum. p. 352: For the corruption of *flexit* to *felix* cf. Don. in Ter. I 335. 12 (Wessner) *infelix* B, *inflexit* cett.; for the reverse, Val. Flacc. 8. 136 *inflexit* codd., *infelix* Schenkl.