

## THE TRANSMISSION OF FLORUS AND THE *PERIOCHAE* AGAIN

to Giuseppe Billanovich

In a recent article I tried to disperse the fog in which modern editions envelop the transmission of the Livian *Periochae* and Florus' *Epitoma de Tito Livio*. Working from editions and catalogues, and without looking at more than a few readily accessible manuscripts, I argued that the *Periochae* reached the Middle Ages in the company of Florus and nothing else; that the mainstream of the medieval tradition, which probably issued from the region south-west of Paris, derived first from a manuscript that presented Florus and only 1–7 of the 142 *Periochae*, *A*, and then from one that presented Florus alone, *e*; that after appearing for several centuries only in *N* (s. ix<sup>1</sup>) and *P* (s. xii<sup>2</sup>) the complete text of Florus and the *Periochae* saw a revival in the Italian Renaissance, probably thanks to Petrarch and Boccaccio; and that most Italian manuscripts contaminate the text of *e* with the complete text. Pending visits to libraries, I left open several questions: whether *A* derived from the source of *NP*; whether *e* derived from *A*; whether the Italian manuscripts of the complete text all derive from one source; whether, if so, it was *P*; whether any of them have escaped contamination in Florus; and whether contamination had already begun in France.<sup>1</sup>

I have now seen enough manuscripts of Florus to answer most of these questions. Out of 189 known to me, I have collated in all but eight as much as time allowed of two long chapters, 2.1–2 and 4.2 (in the older and higher numeration).<sup>2</sup> Where the speculations of my previous article have proved false, I shall say so, but otherwise I shall repeat as little as possible of what I have already written. Once again, I shall refer constantly to P. Jal's editions of Florus and the *Periochae* (Budé, Paris, 1967, 1984) and to R. A. Reid's dissertation *The manuscript tradition of the Periochae of Livy* (Ph.D., Cambridge, 1969). My great debt to Reid must not be obscured by our differences, which begin at a point that it would have taken me far longer to reach on my own.

One scholar will not be surprised or much enlightened by my results: Giuseppe Billanovich. Though he has nowhere followed the tradition back beyond s. xiv<sup>1</sup> or told editors how to approach the Italian manuscripts, I have sometimes wondered whether I am merely spelling out things that he has known perfectly well since at least 1974.<sup>3</sup> A discovery that he announced in that year has lost none of its importance, and

<sup>1</sup> 'The Transmission of Florus's *Epitoma de Tito Livio* and the *Periochae*', *CQ* 38 (1988), 477–91.

<sup>2</sup> For travel grants that took me abroad I thank the British Academy, the University of Cambridge, and the Faculty of Classics, Cambridge; for access to microfilms, the Section Latine of the Institut de Recherche et d'Histoire des Textes, Paris, and the Istituto per la Patologia del Libro, Rome (whose holdings have now been moved to the Biblioteca Nazionale). François Avril and Gilbert Ouy have very kindly given me their opinion on the date and origin of several manuscripts in the Bibliothèque Nationale; Albinia de la Mare has helped me once more by not only annotating a draft but answering further inquiries; and László Havas has kept me abreast of his articles (see nn. 66, 81, 96, 104) and sent me comments on a draft. I have followed up the question of medieval quotations, which are rarer than has been supposed, in a separate article, 'Freculf of Lisieux and Florus', *Revue d'Histoire des Textes* 19 (1989), 381–90.

<sup>3</sup> 'Il Petrarca e gli storici latini', in *Tra latino e volgare: per Carlo Dionisotti* (Padua, 1974), i.67–145.

I can now set it in a firmer context; but I must go through the transmission in roughly chronological order, and it comes in the middle of what I have to say.

### *The family of A*

A is my name for the lost source of those manuscripts that present Florus and the *Periochae* as far as 7.16 *restituti sunt*. Reid has now published his views on how most of them are related in the *Periochae*.<sup>4</sup> In Florus 2.1–2 and 4.2 characteristic readings include 2.2.16 *expugnauit*, 4.2.10 *altiores*, 50 *Caesar* [fuit], 60 *debellata(m)*, 78 *postremo*, 79 *suspectans habens*, 80 *Marte pari*, 81 *quod si nondum*, 82 *pensare*, [agmen], 83 *cogitasse*, 89 (s)ponte, hic for bis, [et] quanto, 90 [hic aliquando finis armis fuit reliqua].

The earliest descendant of A is probably G (Paris Nouv. Acq. Lat. 1767, s. x); as a contemporary annotator put alongside 3.10.21 *Carnuntas* the note *Nota de Carnutisa, Carnotensibus* (fo. 22v), G may well have been written at Chartres or at least in the neighbourhood,<sup>5</sup> which tallies with what I said in my previous article about other descendants of A (p. 488). For an editor G suffers from the disadvantage of having lost everything before 1.16.12 *oculos hostis*. I agree with Reid that L (Leiden Voss. Lat. O 14, s. xi) derives from G, because it shares the errors of G, incorporates some of its corrections and marginal notes, and has errors of its own; I have not checked all the passages from which Jal inferred the independence of L (I clii), but 4.12.32–3 *togas ... principum*, which he reports as absent from G but present in L, was restored in the margin of G by an early enough hand. Leiden Voss. Lat. O 5 (s. xi/xii) also derives from G; corrections of much the same date occasionally restore the truth, e.g. 4.2.82 <agmen>, 90 *generos[um]*, but not often enough or strikingly enough to suggest use of a second manuscript. No less accurate than G are its much later relatives Z (Paris Lat. 17566 + Bern C 219.2, membr. xiv/xv,<sup>6</sup> from the Collège de Navarre, Paris) and Vat. Reg. Lat. 1847 (chart. xiv/xv), which for some reason stops at *Per. 3.9 honore gessissent*. The scribe of the latter worked very carefully, rewriting what did not come out clearly enough; alongside the title, *Annei Flori epithoma de Tito Liuio liber primus incipit*, he wrote ‘Titulus alio libro talis est: *Lucii Annei Flori incipit epithoma de Tito Liuio qui historiam Romanam ab urbe condita scripsit*’ (as in K and O), and the biographical note on fo. 12v will have come from the same source.<sup>7</sup> The Hague 70 E 9 (chart. xv<sup>1</sup>) resembles Reg. Lat. 1847 but has nothing of the *Periochae*.<sup>8</sup>

The most accurate descendant of A is U (Bern 249, s. xi), which in the whole of Florus 2.1–2 and 4.2 has only one error that I noticed: the omission of 4.2.71 *laetus* between lines.

<sup>4</sup> ‘The a Class of the Manuscripts of the *Periochae* of Livy’, in *Owls to Athens: Essays on Classical Subjects Presented to Sir Kenneth Dover*, ed. E. M. Craik (Oxford, 1990), pp. 367–79.

<sup>5</sup> François Avril agrees: ‘La datation X<sup>e</sup> siècle que vous retenez me paraît être en effet la bonne. Le manuscrit est indubitablement d’origine française, et peut être, comme vous le suggérez, en vous fondant sur la note du fol. 22v, d’origine chartreuse’. Gilbert Ouy’s opinion, ‘early XIth cent., hand probably French’, is the same as Munk Olsen’s, *Catalogue*, i (Paris, 1982), p. 388.

<sup>6</sup> Gilbert Ouy confirms the date: ‘ca. 1400’.

<sup>7</sup> At 4.2.80 he left a gap for *in medio* and wrote in the margin ‘In illa uacua platea sic habetur in exemplari: *hi<sup>m</sup>me* (*himne* Z). Silvia Rizzo observes that such scrupulousness recalls the work done at the same date by Nicholas of Clamanges on Paris Lat. 14749 of *Pro Cluentio* and other Ciceronian speeches.

<sup>8</sup> Other parts of this miscellany are signed by Iohannes Steenaert and dated 1446 (fo. 131v) or 1448 (fo. 145v); the same or a similar hand wrote the text of Florus (fos. 85r–115v). On fos. 147–86 is a hitherto unreported copy of the *Florilegium Gallicum*.

At 4.2.82 GU read *itaque* for *denique*, but *denique* survives in Harl. (B.L. Harl. 2620) and Voss. (Leiden Voss. Lat. O 77, s. xii). Dates for Harl. have ranged from s. x to s. xiii, and weighty names have supported the early date, but assigning Harl. to s. x and G to s. xi<sup>1</sup> makes no palaeographical sense to me, and my own date would be s. xii<sup>1</sup>; the scribe has a habit, though, of suspending an open *a* from the last stroke of *m* or *n*, a habit that migrated from St Gallen to Freising, and it has been suggested to me that if Harl. comes from Freising the script may suit a date considerably earlier than s. xii<sup>1</sup>.<sup>9</sup> Whatever the date, I should be happy with Freising as the home of Harl., because I agree with Reid that Harl. was the source of M (Munich C.L.M. 6392, s. xi/xii), which certainly comes from Freising; at 4.2.38, where Jal reports *tunc tractis* from Harl. M in place of *contractis*, Harl. correctly reads *contractis*, and what Jal and the scribe of M took for *t-* is a paragraph sign.<sup>10</sup> Of Pelplin Semin. Duchown. 2574 (s. xi/xii), currently missing, I have seen only the first page, which runs to 1.1.1 *et urbis et imperii*;<sup>11</sup> besides the usual readings of *A*, it has *Praef. 3 tabella tantam* for *tabellatam*, *insimul* (with *e* over the second *i*), 6 *hoc erit*, *Collatioque consule in Quintum*, 8 *ducenti anni*.<sup>12</sup> Of these readings Jal reports only *insimul*, from M; but Harl. has *insemel* as a correction and also corrects *-tam* to *tantam*, which appears in the text of M. The manuscript may therefore derive from Harl., a possibility that will be easier to test if it reappears. Its script could easily be German.

The fragments divided between Leiden Voss. Lat. O 70A and West Berlin Lat. Q 306 (s. xi/xii), which preserve parts of 3.6–19, follow at 3.6.6 Jal's 'codd. sine transp.' (those that have remedied the ancestral transposition), which include Voss. and e. With Voss. they share four transpositions: 3.10.17 *a trepidis accepit*, 14.4 *dies rogationis*, 18.10 *cecineret signa*, 19.7 *ipsi proelio*. As Voss. has the same errors and more, e.g. 3.8.8 [ulla], 14.4 *in partibus et tribuni*, the fragments may well come from its exemplar.

Shifting agreements in error among GZ, U, Harl., and Voss., together with readings that occur in many of them as variants, suggest that *A* produced three or more lines of descent and offered corrections that they each took or left, but in any event it can be reconstructed satisfactorily from ZUHarl. with an eye on NP and on the one manuscript that stands entirely apart from the rest of the tradition, B

<sup>9</sup> I owe the suggestion to Albinia de la Mare. Barrie Hall and Francesco Lo Monaco have kindly given me their opinions on the date: s. xii<sup>med.</sup> and s. xi<sup>2</sup>. For the extremes see on the one hand the Harleian catalogue (London, 1808), followed by Lehmann (who may just have been quoting) and by Munk Olsen, op. cit. 387, and on the other hand Roszbach's edition of Florus (p. xviii). Baehrens, who drew attention to the manuscript in *Rhein. Mus.* 30 (1875), 629, assigned it to s. xi/xii. As classical studies at Groningen have recently been celebrating a century of history, let me quote words from Roszbach's edition of Florus that I had not seen when I wrote something similar in *Texts and Transmission*, ed. L. D. Reynolds (Oxford, 1983), p. 424 n. 18: '... Aem. Baehrensus, quem qui ob futiles coniecturas uituperant saepe numero obliuiscuntur protractis codicibus qui diuturno situ sepulti iacuerant de scriptoribus Latinis bene meruisse ...' (p. xv).

<sup>10</sup> I had not noticed this passage when I asked Ruth Taylor to check another in M. After 4.2.95 *terrarum* Harl. omitted *orbem*, for which a hand of s. xiii/xiv substituted *ambitum*, and the omission can easily be blamed on Harl. itself, because the final sentences of 4.2 are crammed into the margin at the end of a quire. Dr Taylor kindly reported that M also omits *orbem* and after correction reads *terras* for *terrarum*.

<sup>11</sup> Jerzy Axer has very kindly searched for the manuscript on my behalf, but so far to no avail. I am much obliged to Birger Munk Olsen for supplying me with a photocopy of the first page. See also B. Munk Olsen and P. Petitmengin in *Histoire des bibliothèques françaises: Les bibliothèques médiévales, du VI<sup>e</sup> siècle à 1530*, ed. A. Vernet (Paris, 1989), p. 425 and n. 158.

<sup>12</sup> It also reads *Praef. 7 [ipsa]*, which Jal reports only from L of the manuscripts so far mentioned; but they all agree with L. Similarly, they all have *qui prope* at 2.1.2.

(Bamberg Class. 31, s. x<sup>1</sup>). Alongside 2.2.21 *onustam* *A* had in the margin this gloss (I reproduce it from G, which at first mistook it for part of the text): *honustus dicitur aliqua re cum honore oneratus, ut trhopheis per uirtutem adquisitis, unde et aspiratur uidelicet pro affinitate nominis quod est honor; oneratus uero minime utpote diriuatum ab onere* (cf. *T.L.L.* on *onustus*, 65.10–13).

French descendants of *A* persist into s. xv. Jal and Reid, and consequently I myself in my previous article (pp. 486, 488), failed to report that J (Paris Lat. 18273, s. xiii<sup>2</sup>), an inferior relative of Z, has *Per.* 1–7 as its text of Florus would have led one to expect. I have just mentioned the date of Z, Vat. Reg. Lat. 1847, and The Hague 70 E 9, x. xiv/xv. Not in Florus but in *Per.* 1–7 Jal uses Paris Lat. 5794 (membr. xv), which probably derives from Voss., and Voss. is certainly the source of Paris Lat. 16536 (chart. xiv<sup>3</sup>) and Paris Lat. 16708 (chart. xiv<sup>ex</sup>), which unintelligently incorporate most of its numerous glosses; 16708, which has lost everything beyond Florus 2.17.7 *capta Carthagine*, may derive from 16536, and both entered the Bibliothèque Nationale from the Sorbonne.<sup>13</sup> The ed. Paris. c. 1471 of Robert Gaguin (*G.W.* 10092) understandably drops *Per.* 1–7 but is related to G: 2.2.2 *muro* (G<sup>2</sup>), 25 [*immo*] (G<sup>1</sup>), 4.2.14 *grauis ne hic*, 16 <*Pompeio*> *eodem*, 56 [*re*] *poscebat*, 94 *Caesaris*. A few readings of e have crept in, perhaps from Paris Lat. 5792 (French, chart. xv), in which Gaguin appended to the text his name and the date 1469.<sup>14</sup> Paris Lat. 5754 appears to be a calligraphic copy of the edition.

### *The relationship of A to NP*

I blamed the difficulty of establishing the relationship between *A* and NP partly on editorial inaccuracy, which I illustrated with the aid of Florus 4.12.4. NP there turn out to read what I predicted (p. 486), *Noricis animos Alpes et in niues bellum non posset ascendere*. The passage therefore provides no evidence that *A* did not derive from the source of NP.

Besides this illusory error of NP, Jal's apparatus reports about 50 others absent from B*A*. Most of them, however, are minute, and they include only one transposition, 3.16.3 *iam cum* for *cum iam*, and no omissions except of *ut*, *et*, *in*, and *ad*. Furthermore, the apparatus also reports several errors of N*A* and P*A*, though again they include only one transposition, 2.6.51 *angulo Italiae* N*A*, and no omissions of greater moment than 1.1.11 *est* P*A*, 18 *in* P*A*, 2.2.18 *deni* P*A* (Jal's apparatus reads '*deinde* A: *denique* de N om. V de cett.', but N at first had *deni de*, which was corrected to *denique*), and 2.6.31 *immo* P*A*; N*A* are said to omit *sine* at 1.17.5, but so does P (I have checked P only here and at 2.6.51). The omission by N*A* of *imparem* after *interim* at 3.17.9 looks more like preservation of only one variant (*imparem* B *interim* *imparem* P); P has preserved another such variant, 1.14.1 *pari imperio*, albeit out of place. An interesting divergence occurs at 1.16.7: *A* read *ar[mis et discolori ueste usque ad ambitum ar]matam*, N restored the missing passage after *armatam* without *ar-* and with *-it* for *et*, and P has it in the right place without *et*.

<sup>13</sup> I quote what Gilbert Ouy tells me about three of these manuscripts: J 'end of the XIIIth cent.; nice French hand', 16536 'ca. 1375; probably written in Avignon (see flyleaves), but the scribe looks Italian (from the north)'; 16708 'last quarter of the XIVth cent.; ugly cursive, German hand'.

<sup>14</sup> H. Nickel, 'Textkritisches zu den Florus-Inkunabeln', *Philologus* 118 (1974), 166–73, makes the early editions sound much alike in the quality of their text. That does justice neither to the ed. Paris., for the reason just given, nor to the ed. Parm., which I shall mention below (p. 474). Admittedly it would have been hard to draw these distinctions without probing beneath the surface of Jal's edition, but Nickel made it even harder for himself by staying with Malcovati's.

Even if inaccuracies lurk here or elsewhere in Jal's reports, the most reasonable conclusion is surely that N, P, and *Δ*, derive independently from a manuscript that had occasional corrections, notes, or obscurities.

### *The relationship of e to Δ*

Besides dropping *Per.* 1–7, *e* made a large number of errors, and it also had nearly all the errors of *Δ*. My prediction that it might turn out to derive from Voss. (p. 487) proves false, because Voss. has errors not reported by Jal, e.g. 2.1.2 *Italiae dare*, 4.2.32 [*a Basilo*], 38 *undique copiis omnibus in unum*, 63 *obtruiit proelio*, 82 [*clamore*], 91 [*an ipso uolente*], 92 *potentia beneficiorum*. Nevertheless, agreements with Voss. remain, e.g. 4.2.27 *obundatio*, 90 *patruielibus*, 93 *patres* (see also Jal's apparatus on e.g. *Praef.* 2 and 5, 2.6.10), and they surely demonstrate that *e* derived from *Δ*.

Where *e* avoids errors of *Δ* in Florus 2.1–2 and 4.2, it does so by agreeing not so much with NP as against NP/*Δ* with B. Theoretically, it could owe these agreements to a second source, whether a relative of B, a manuscript higher on the same branch as NP/*Δ*, or something independent of either. Is there anything to commend any of these possibilities?

I argued in my previous article, and have said again, that the very different text of B, an Italian product rapidly exported to Germany,<sup>15</sup> never came into contact with the usual tradition, and I do not believe that *e* is an exception. Collation of B or a manuscript like it would have been bound to remove far more of the errors that *e* shares with *Δ* or NP/*Δ*. For the same reason, I cannot believe that *e* drew on a witness independent of both B and NP/*Δ*. The third possibility, that it drew on a superior relative of NP/*Δ*, is less open to this objection but still does not appeal to me. On the contrary, accident and conjecture will account for agreements between *e* and B. To accident I should attribute 2.2.5 *illam ipsam* (*ipsam illam* NP/*Δ*), 15 [*eī*] (hab. NP/*Δ*), 4.2.63 *hostem uictum* (*uictum hostem* NP/*Δ*); to conjecture, 4.2.27 *-undatio* (*-undatione* NP/*Δ*), 44 *uel* (*belli* NP/*Δ*), 53 *recaluerunt* (*recalauerunt* NP/*Δ*), 79 *suspectam* (*-ans Δ*), 87 [*in Celti*] (hab. NP/*Δ*), 89 *Ponto* (*ponte uel sponte Δ*), 93 *patres* (*patricii* NP/*Δ* *patri* GL *pati* ZJ); to one or other, 4.2.48 *contenderent* (*-eret* NP/*Δ*), 69 *suffecit* (*sufficit* NP/*Δ*).

As a witness, therefore, *e* in my opinion can safely be ignored, but it would not surprise me if this conclusion encountered more opposition than any of my others.<sup>16</sup> For one thing, my collations have been confined to 2.1–2 and 4.2. There is also room for argument about how *e* should be defined, as I shall next try to show.

### *The family of e*

Its agreements with Voss. (s. xii) suggest that *e* was written not long before its oldest descendants, H (Heidelberg Pal. Lat. 1568, s. xi/xii<sup>17</sup>) and Q (Paris B.N.

<sup>15</sup> Mirella Ferrari kindly tells me that on p. 214 there are notes by German hands of s. x/xi ('possono essere dell'anno Mille circa o poco dopo'); she inclines to think that the manuscript was taken to Germany by Otto III but would not rule out Henry II. For my purposes the difference hardly matters.

<sup>16</sup> László Havas tells me that he finds it hard to accept.

<sup>17</sup> In my previous article I carelessly allotted H to the Palatini in the Vatican (p. 488). Patricia Stirnemann and Françoise Gasparri have kindly given me their opinions on the date, which concur with mine. On the strength of a comparison with Bodl. 689 and 866 Dr Stirnemann suggests that the place of origin may be Limoges; according to Albinia de la Mare, whom Dr Stirnemann supplied with photographs of fo. 1 and fo. 83, 'the initials are in the same style as the initial to the central portion of Bodl. 866 (a composite manuscript, though all from Limoges – contents in hand of Bernard Itier) and the first initial to Bodl. 689; the script of the central

Lat. 5802, s. xii<sup>med.</sup>). Reconstructing e has its problems even if one ignores Italian descendants contaminated with the complete text and starts from H, Q, and Jal's lost hyparchetype k.

Any reconstruction will ascribe to it e.g. 2.2.10 *omnem uitam*, 32 [et sic], 37 *muros arcemque*, 4.2.1 [*Romanum*], 7 [et] *odia partium cum bello finita*, 10 [ecce], 19 *sine sanguine bellum*, 31 *circumuenit utrumque*, 38 [omnibus], 55 *tanti uiri*, 73 [fuit], 78 *diu et*, 81 *retro gradum*, 83 *subsidio*, but HQ add to these errors several others absent from k, e.g. omission of 2.2.9 *quasi*, 4.2.23 *belli metu*, 27 *ipse*, 35 *iam*, 40 *assiduis*, 69 *postero die*. Jal accordingly puts HQ into a small family of their own alongside a manuscript that has only excerpts, Y (Paris Lat. 18104, s. xii<sup>2</sup>).

The attractions of this classification fade when one takes into account Paris Nouv. Acq. Lat. 576 (membr. xiv<sup>1</sup>, English<sup>18</sup>), which shares all the errors of HQ but also some errors of k. Perhaps, therefore, k corrected some errors of e by consulting a descendant of A that did not belong to the family of e. One visible example of such contamination, at what may be the right date but in the opposite direction, is the presence in J (s. xiii<sup>2</sup>) of the biographical note on Florus, *Anno xiii... componere potuit*, added in paler ink by the same or a very similar hand. Though it follows the text, it is called *prologus in the incipit and explicit*, and descendants of e indeed have it at the beginning: H as part of the text, K and Paris Nouv. Acq. Lat. 576 in the margin. Jal must therefore be right (I clvi) that it entered J from a descendant of e. If so, the descendant of e, whether it was k or an ancestor of k, could have taken readings from J at the same time.

Once k has been set aside, e can be defined as the common source of H, Q, Paris Nouv. Acq. Lat. 576, and the excerpts in Paris Lat. 18104.

### *Two unusual manuscripts*

The biographical note on Florus also precedes the text in Cambridge Corpus Christi College 313 (s. xii<sup>2</sup>, English), but otherwise significant agreements with e do not go beyond *Praef. 2 ubique* and 5 *matrem*, and significant agreements with other descendants of A seem equally absent. Evidence is not available from 4.2, however, because 3.20.9 *exsequiis*... 4.12.21 *utinam* has fallen out between p. 64 and p. 65.

A peculiarity of the manuscript is that it opens with a table of contents for the first book (this precedes even the biographical note) and has one for each book. B has a table of contents, but in the family of NP/A I do not recollect seeing any other before s. xiv<sup>2</sup> except one that has no contents to go with it. The manuscript where it appears, Paris Lat. 5767, consists of 25 leaves: two intact *quiniones* and a *ternio* that has been robbed of its fifth leaf. On fo. 24v ends a text of *De Bello Gallico* in an Italian hand of s. xiii, and the recto of the last leaf is blank, but the verso, which one expects to be equally blank or to have nothing more than scribble on it, presents in a formal hand and alternating colours the table of contents for a manuscript of Florus that like NP/A divided the work into four books rather than two.<sup>19</sup> The most interesting thing

portion of Bodl. 866 is also similar, though not identical, and there is the same very pronounced pricking for the ruling'. I therefore repeat with more confidence my suggestion that H was the manuscript collated by Vinetus at the Dominican library in Bordeaux (p. 488 n. 50).

<sup>18</sup> F. Avril and P. Stirnemann, *Manuscrits enluminés de la Bibliothèque Nationale: manuscrits d'origine insulaire VII<sup>e</sup>-XX<sup>e</sup> siècle* (Paris, 1987), p. 155 n. 194, with plate lxxix.

<sup>19</sup> But for the files at the I.R.H.T. I should never have known about this manuscript, and but for Patricia Stirnemann I should still be waiting to see it. She tells me that it may be Genoese and probably belongs to s. xiii<sup>3/4</sup>. Virginia Brown in *Palaeographica diplomatica et archivistica: studi in onore di Giulio Battelli* (Rome, 1979), i.131-2, mentions the table of contents but says nothing about the origin of the manuscript.

about this table of contents is not that it exists but that like the text of Caesar it was written by an Italian hand, not indeed the same one but a hand no later than about 1300.

*Jal's k and the return of Florus to Italy*

It seems to have been about 1300 that the text of Florus reached Italy from France. Descendants of k, which Jal reconstructs from TβOKδ, had probably carried it there before Petrarch acquired Q and certainly long before Salutati acquired K, which was evidently in his possession by 1371.<sup>20</sup>

O (Paris Lat. 5789) has always been assigned to s. xiv, but I should assign it to s. xiii<sup>2</sup>, and it was written, I suspect, at the Sorbonne;<sup>21</sup> but on fo. 45r, after the end of Dictys, it has an inscription copied out by an Italian hand of s. xv<sup>med.</sup>, *Supra Tyberonem in ponte: Quam bene curbati...* (CIL vi.1199b; Walther, *Initia* 15124). I believe that O is the source, or at any rate the chief source, of over 20 manuscripts both French and Italian, manuscripts that share these errors: 2.2.25 *currus* for *crucis*, 32 *perire*, 35 *castra*, 4.2.4 <et> *commune*, 8 [ciues], 27 *ipseque obsessor*, 33 *diem totum*, 50 *medius[que]*, 80 *pugnantium* for *silentium*. Errors of O unanimously adjusted by the rest, e.g. 2.1.2 [fuerat] *O erat* cett., 2.2.2 *in praesenti praefer* O *in praesenti prae se* cett. (in *proximo* NPA *inpendi* K *in peno* δ *in populo* Paris Nouv. Acq. Lat. 576), 4.2.46 *rep[er]tus O tectus* cett., 62 *ru<b>ebat* O *tendebat* cett., suggest an intermediary. The appearance of O seems to have caused two errors in this intermediary: at 2.2.31, where *o tunc* in O looks like *o taus*, the rest have *o taus*, *octauus*, or a gap, and at 4.2.12 a messy correction in O after *exercitus* appears to cancel *et*, which the rest omit.

The descendants of O fall into at least three families. Whereas the second consists entirely of Italian manuscripts, the first is a mixture of Italian and French, but its French members are late enough for their text to have come back over the Alps.

(1) 2.2.5 *monstris* is omitted by Vat. Lat. 1860 (Italian, membr. 1313?<sup>22</sup>), Paris Lat. 5790 (Italian, membr. xiv), Escorial N II 20 (French, membr. c. 1400), Reims 1327 (French, membr. c. 1400,<sup>23</sup> Jal's β), Bern 242 (French, membr. xv<sup>1</sup>), Vat. Reg. Lat. 1564 (French, membr. xiv/xv), Leiden Voss. Lat. O 70 (probably Italian, chart. xv<sup>1</sup>), Paris Lat. 5792 (French, chart. xv), Madrid Nac. 8593 (first copy, Italian, chart. xv), and the closely related pair Laur. Fies. 181 and Modena Est. Lat. 437 (Florentine, membr. xv<sup>3/4</sup>, both written by the same scribe<sup>24</sup>), and 2.2.19 *ciuitas* by all these except

<sup>20</sup> Cf. F. Novati, *Epistolario di Coluccio Salutati*, i (Rome, 1891), 153.20–154.3. Salutati there uses the biographical note on Florus, very rare in Italian manuscripts but found in K.

<sup>21</sup> François Avril confirms my less competent opinion: 'Ce manuscrit date bien, d'après l'écriture, qui me paraît française (Paris ??), de la deuxième moitié du XIII<sup>e</sup> siècle, et non du XIV<sup>e</sup> siècle'. He adds this: 'Il vient de la librairie de Blois et comporte des notes et un poème final en écriture humanistique italienne, et je me demande s'il ne pourrait pas venir de la bibliothèque des ducs de Milan, bien qu'il ne corresponde à aucun des trois Florus mentionnés dans l'inventaire A de Pellegrin.' Simonetta Cerrini, who kindly inspected it at my request, tells me that she found no evidence in favour of this interesting suggestion.

<sup>22</sup> Every text in the manuscript except the *Breuiarium* of Festus occupies quires of its own, and the date occurs only in the rubricated subscription of Vegetius, where as Silvia Rizzo observes it could well have been copied from the exemplar; but it did not seem to me an impossible date for the manuscript itself. The learned annotations, thickest on Suetonius and probably no later than s. xiv<sup>med.</sup>, call for the eye of a Billanovich.

<sup>23</sup> I learnt from the description at the I.R.H.T. that the manuscript has the signature of Guy de Roeye, archbishop of Reims 1390–1409, and a note by the cathedral librarian Gilles d'Aspremont, who died about 1414–15.

<sup>24</sup> A. C. de la Mare in A. Garzelli, *Miniatura fiorentina del Rinascimento 1440–1525: un primo censimento* i (Indici e cataloghi toscani 18, 1985), 504–5 no. 32 (19) and (26). The scribe, Hubertus

Paris Lat. 5792, which has also corrected some errors of O that I listed above. At the end of Paris Lat. 5792 appears the signature of Robert Gaguin together with the date 1469, and its contents (Florus, *De uiris illustribus*, and Justin) recur in two separate manuscripts whose first known owner, Ambroise de Cambrai (c. 1425–96), was on close terms with Gaguin: Leeuwarden 54 (Florus, *De uiris illustribus*) and 53 (Justin), which are very probably in the same hand as each other and have the same format and binding (membr.).<sup>25</sup> I have not seen them, but 54, ‘Fran.’ in Duker’s edition (Leiden <sup>1</sup>1722, <sup>2</sup>1744), shares 2.2.17 *ipso* <*bello*> with Paris Lat. 5792 and has many other readings of k. Laur. 68.29 (chart. xv<sup>med.</sup>) shares errors in 4.2 with Fies. 181 and Est. Lat. 437. A notable innovation of the family is the addition of a letter from Florus to the Romans, *Lucius Anneus Florus senatui populoque Romano ... xviii annum agens*.<sup>26</sup>

(2) 2.2.34 *et* after *classis*, 4.2.1 *ipsum*, 39 *copiis*, 49 *fuit*, are omitted by Fermo Com. 81, Venice Marc. Lat. Z 368, and Paris Lat. 9681 (all Italian, s. xiv). Billanovich has argued that Fermo Com. 81 was put together at Venice by Guido da Bagnolo, who died in 1370, but he regards the text of Florus at the end as an addition of about 1400;<sup>27</sup> I accept the connection with Guido da Bagnolo, but the interval between Florus and the rest does not convince me, and his reasons for assigning the manuscript to Venice rather than Bologna seem inadequate. I have more to say later about Marc. Lat. Z 368. Heavy correction in both makes their relationship hard to determine. Paris Lat. 9681 was written and decorated at Rome in the last 20 years of the century;<sup>28</sup> some of its errors, e.g. 2.2.15 [*Lucio ... prouincia*], 4.2.6 *anni* <*fuertunt*>, recur in Wrocław I F 312b (chart. xv, in a northern-European hand). B.L. Harl. 2657 (Florentine, membr. 1442<sup>29</sup>), Florence Soc. Colomb. 238 (Florentine, membr. xv<sup>3/4</sup>), and East Berlin Hamilt. 262 (Italian, membr. xv<sup>3/4</sup>), which certainly derive in large measure from O through the lost intermediary, are probably contaminated members of this family, and so too in 2.1–2 Parma Palat. 2800 (Florentine, membr. xv<sup>3/4</sup>, written by Iulianus Pratensis; ‘anno domini 1400’ was added much later); their fresh errors include 2.2.4 *uirtute* <*rei*>, 23 *clade*, 28 *captiuatorum*, 31 *quamuis Octauio*, 32 *principes/-ceps* for *Punicae*, 35 *regebatur, uenantium*.

(3) Paris Lat. 5690 (membr. c. 1290–1330,<sup>30</sup> owned by Landolfo Colonna and later by Petrarch) shares no omissions with either family but has others of its own, namely 2.2.5 *ingressus* [*est*], 4.2.8 *recentes*[*que*], 48 [*esse*], 59 [*quidem*], 62 [*suae*]. In 2.1–2 four Italian manuscripts could derive from it: Laur. Edili 168 (chart. xv<sup>med.</sup>); Vat. Reg. Lat. 1749 (membr. xv<sup>med.</sup>); Modena Est. Lat. 383 (chart. xv<sup>med.</sup>), which contains only 1.19.2 *domiti ergo Picentes ...* 3.21.5 *rabiam supergressum est*; and Jal’s T (Pavia Ald. 228, Italian, membr. xv<sup>1</sup>), unprofitably brought into the editing of Florus by

W., wrote two other manuscripts of Florus, B.L. Harl. 3694 = *ibid.* (22), which has an unrelated text, and Verona Capit. CXXXVII (125) = *ibid.* (41), which has yet a third type of text; on Escorial ç. IV 16 (not 66) = *ibid.* (2) cf. no. 91 below.

<sup>25</sup> I am very grateful to Dr Martin Engels of the Provinciale Bibliotheek van Friesland for information about the two manuscripts. On Ambroise de Cambrai and his relations with Gaguin see L. Thuasne, *Revue des Bibliothèques* 11 (1901), 13–15; M. Fournier and L. Dorez, *La faculté de décret de l’Université de Paris au XV<sup>e</sup> siècle* ii (Paris, 1902), p. 208 n. 2; L. Delisle, *Journal des Savants* (1902), 332–8.

<sup>26</sup> See n. 56 of my previous article.

<sup>27</sup> *I.M.U.* 25 (1982), 368–72.

<sup>28</sup> I owe this information to François Avril, who will shortly be discussing the illuminator in an article.

<sup>29</sup> De la Mare, *op. cit.* (n. 24), 500 no. 28 (18).

<sup>30</sup> As I mentioned in my previous article (p. 489 n. 53), the precise date is highly controversial, and I give what all parties accept as the outer limits. See now A. Petrucci in *Renaissance- und Humanistenhandschriften*, ed. J. Autenrieth and U. Eigler (Munich, 1988), p. 4 n. 16.



Malcovati. In 4.2 T derives from Q and teems with errors and interpolations. To judge from Jal's apparatus, the change of exemplar took place somewhere between 3.12.12 and 3.17.1. About the second exemplar I have more to say later.

Manchester Rylands 111 (membr. xv<sup>med.</sup>), written by a scribe from the Low Countries and owned soon afterwards by a church at Herentals in Belgium, shares a number of innovations with e and a few with O, for instance 2.2.17 *Mallio Regulo*, 23 *calamitati*. As its reading *in proximo prae se* at 2.2.2 seems to conflate *in proximo*, the reading of NP1, with *in praesenti prae se*, which arose in the family of O, contamination evidently took place in its ancestry, but I noticed nothing that could not have come by a northern-European route from 1. It has lost leaves and preserves only 1.4.2 *futurum ut totius mundi opes*... 2.18.2 *Celtiberorum quadraginta*.

Jal's K and δ do not derive from O. K (Paris Nouv. Acq. Lat. 3070, s. xiii/xiv) has penwork decoration 'manifestement parisienne',<sup>31</sup> and δ (Arras 902, chart. xv<sup>2</sup>) is not Italian as the catalogue says but French.<sup>32</sup>

The descendants of k span a period in which many streams of classical tradition run dry and texts reach Italy from France either mysteriously or not at all. Like the later of the manuscripts that go back to 1 but not by way of e, they illustrate the growing importance of Paris in the collection and distribution of texts.

#### *Contamination in northern Europe*

Apart from the contamination within the family of 1 that lies behind k, the addition to J, and Rylands 111, I have found no trace of contamination in northern Europe before s. xv<sup>2</sup>.

Paris Lat. 6112 (chart. 1466), written in France, is close to HQ but has 4.2.69 *postero die*, which they omit, and at 2.2.9 reads *uelut* where they omit *quasi*. Apparently, therefore, it derives mainly from e but partly from another descendant of 1, one that may have been Italian. Its omission of *uerni* at 4.2.27, which recurs in some Italian descendants of Q, is not enough in so inaccurate a manuscript to persuade me that it derives from e through Q, and though it could have acquired *postero die* from Q<sup>2</sup>, it has the truth in other places where Q erred and Q<sup>2</sup> did not intervene, e.g. 2.2.2 *uideretur* (*putaretur* Q, the error in 2.1–2 and 4.2 that its descendants least often dislodged).

I take s. xv<sup>2</sup> to be the date of a small family that no more than thinly disguises its descent from O: B.L. Harl. 2500 ('liber canonicorum regularium Sancti Martini de Louanio'<sup>33</sup>), Bodl. Add. C 154 (Low Countries), and Paris Lat. 10400. As I shall explain later, however, the family originated in Italy, where descendants of e and manuscripts of the complete text had already been in contact at least three times before 1400. How the contact happened, and when it began, will be the subject of a later section. First I must introduce the complete text in its purest form.

#### *The emergence of the complete text*

The complete text of Florus and the *Periochae*, unaccompanied by anything else, appears in N (s. ix<sup>1</sup>) and P (s. xii<sup>2</sup>),<sup>34</sup> and the *Periochae* also appear, with or without

<sup>31</sup> I quote the opinion of François Avril.

<sup>32</sup> For confirmation see now C. Jeudy and Y.-F. Riou, *Manuscrits classiques latins des bibliothèques publiques de France*, i (Paris, 1989), p. 154.

<sup>33</sup> Cf. C. E. Wright, *Fontes Harleiani* (London, 1972), p. 228. It includes works of Poggio's.

<sup>34</sup> I wrongly described fos. 103–4 of P as a bifolium (p. 480). Munk Olsen, whom I was citing, rightly says that they are two single leaves, and stubs remain of their other halves. It is therefore possible, though no more than possible, that another work has been cut out after the *Periochae*.

Florus and other texts, in over 80 Italian manuscripts of s. xiv<sup>2</sup>–xv.<sup>35</sup> As many of the earlier ones contain only Florus and the *Periochae*, it seems that a manuscript similar to N and P, or perhaps more than one, came into use during s. xiv.

Several references to the *Periochae* in works of the period antedate the earliest Italian manuscripts, but in my previous article I discussed only the part played by Petrarch, Boccaccio, and Domenico di Bandino, in the diffusion of the complete text, because Petrarch and Boccaccio can readily be connected with extant manuscripts and Domenico professes to have made the *Periochae* widely available (pp. 482–4). I traced Petrarch's knowledge of the complete text no further back than 1351, Boccaccio's to the early 1340s.<sup>36</sup> Here I must add seven other pieces of evidence older than 1351, of which the first three and the last were unknown to me when I wrote.

(1) In his *De originibus rerum*, finished by about 1350, Guglielmo da Pastrengo, a Veronese friend of Petrarch's, quotes from 'Florus ex Liuius' *Per.* 13.7, 14.3, 16.6, 18.4, 68.9, 129.4.<sup>37</sup>

(2) In *Rerum memorandarum libri* 1.18.1, written during 1343–5, Petrarch refers to 142 books of Livy, a figure that must have come from the *Periochae*.<sup>38</sup>

(3) In *De uiris illustribus* 13.2, written between 1341 and 1343, Petrarch mentions that Curius Dentatus celebrated two triumphs in one year. He could have read about the two triumphs in the work of the same name attributed to Aurelius Victor, 33.1–3, but only *Per.* 11.6 says explicitly that they were celebrated *in eodem magistratu*.<sup>39</sup>

(4) At Avignon about 1335 Petrarch's 'Socrates', Ludouicus Sanctus of Beringen, commissioned Ambros. F 138 sup., a historical miscellany that includes Florus but not the *Periochae*. Nevertheless, an introductory note mentions that Florus condensed the 142 books of Livy, and the figure must have come from the *Periochae*. If Ludovicus had only had them included in his miscellany, his copy would have been half a century older than the oldest Italian copies now in existence. I shall return in a moment to his text of Florus. Billanovich's discussion of the manuscript in 1974 is a landmark in modern work on the transmission of Florus and the *Periochae*.<sup>40</sup>

In his editions of both Florus (I cxx) and the *Periochae* (I c) Jal astonishingly contradicts Rossbach's statement that P is written in two columns.

<sup>35</sup> Eleven more (cf. n. 3 of my previous article): Aix-en-Provence 1466 (1323), West Berlin Lat. 4<sup>o</sup> 343 and 436, Bologna Univ. 2675, Cesena S. 15.5, Fermo Com. 45, Lucca Capit. VIII 512 (a. 1467), Modena Est. Lat. 699, Munich C.L.M. 568, Palermo Com. 2 Qq c 73 (which never went beyond 46.10), Parma Palat. 253. I am much obliged to Dr Xavier Lavagne for details of the manuscript at Aix, which has lost its first leaf; strangely, it was missed by Jeudy and Riou, op. cit. (n. 32). On Fermo Com. 45, which contains only Florus and the *Periochae*, see Serafino Prete, *Studia Picena* 24 (1956), 35–7. I exclude from the total manuscripts that have only a selection.

<sup>36</sup> I took on trust Maria Teresa Casella's ascription to Boccaccio of the glosses on an Italian translation of Valerius Maximus (p. 483 n. 35), but I have since learnt by following up the last footnote of her article 'La singolare biblioteca di un chiosatore trecentesco', *Studi Petrarqueschi* n.s. 3 (1986), 117–202, that her arguments have been contested in detail by E. Lippi, *Studi sul Boccaccio*, 14 (1983–4), 357–72, and L. Petrucci, *Riv. Letterat. Ital.* 2 (1984), 369–87. For their part Lippi and Petrucci take it on trust from Billanovich and Casella that Boccaccio used a text of Livy prepared by Petrarch; see, however, *Riv. Fil.* 115 (1987), 424–30.

<sup>37</sup> Sabbadini, *Le scoperte dei codici latini e greci ne' secoli XIV e XV*, i (Florence, 1905), 12 n. 57, as interpreted by V. Fera, *La revisione petrarchesca dell' Africa* (Messina, 1984), pp. 65–6 (I am grateful to Silvia Rizzo for this reference). The quotations occur on fos. 124–30 of the only printed edition (Venice, 1547).

<sup>38</sup> Billanovich, op. cit. (n. 3), pp. 97–8. For the text see Billanovich's edition (Florence, 1943), pp. 17–18; for the date of composition, *ibid.* lxxxii–cxxiv.

<sup>39</sup> U. Bosco, *Giorn. Stor. Lett. Ital.* 120 (1942), 98–9 = *Saggi sul Rinascimento italiano* (Florence, 1970), p. 198, cited by Fera. Cf. G. Martellotti's edition (Florence, 1964), p. 55.

<sup>40</sup> Op. cit. (n. 3), pp. 69, 87–9.

(5) In a famous passage of his *De uiris illustribus*, written at Avignon in the early 1330s, Giovanni Colonna recalls seeing *quartam decadam* of Livy in an old manuscript at Chartres. According to the same passage, Livy wrote 150 books.<sup>41</sup> I do not know of any source that gives a figure so close to the one given in the *Periochae*.

(6) Between 1327 and 1332, at either Avignon or Bologna, Thomas Waleys saw a copy of the *Periochae*. The printed edition of the commentary in which he mentions it speaks of 44 books rather than 142, but the numeral is doubtless corrupt. He may have been introduced to the *Periochae* by Guido de Guisis, bishop of Modena, whom he thanks for putting at his disposal the fourth decade of Livy.<sup>42</sup>

(7) An annotator in Leiden Voss. Lat. F 21 of Livy mentions in the same breath Nicholas Trevet's chronology and the total of 140 books given by 'Florus'.<sup>43</sup> Trevet's chronicle, which I consulted in the first manuscript that came to hand, Paris Lat. 4929, draws copiously, as the preface announces, on the ten books *Ab urbe condita* and the ten *De Punico bello secundo*, but shows no other knowledge of what Livy wrote; and in his commentary on these two decades he adds only the rumour that Livy wrote 300 books.<sup>44</sup> The annotator was therefore using the *Periochae* himself. If Voss. Lat. F 21 really belonged to s. xiv<sup>ex</sup>, that would be of little interest; but I persist in considering it a century earlier.<sup>45</sup> If I am right, the *terminus post quem* for the annotator becomes the date of Trevet's chronicle. It is dedicated to Hugh, archdeacon of Canterbury and papal nuncio, and the period when Hugh of Angoulême held both these offices ran from 1326 to 1329.<sup>46</sup>

Even if there is still evidence that I have missed, Billanovich seems likely to be right that the complete text entered circulation at Avignon about 1330. I can see only one way of determining where it came to light: localization of the manuscript that will turn out to be the closest relative of the Italian manuscripts, P. Written towards the end of s. xii, part II of P, the part that comprises Florus and the *Periochae*, is French like part I, which comprises *De oratore*, and part III, which comprises the opening of Pliny's *Natural history*. Parts II and III show a certain resemblance to each other, but neither bears any mark of origin.<sup>47</sup>

External evidence about the origin of the manuscript that came to light may be exhausted, but external evidence about its date is not. In my previous article (pp. 482–3) I mentioned that three scribes claim to have found the *Periochae* annotated by Petrarch in *codice uetustissime litere*. They are plainly referring to a manuscript older than Petrarch, presumably the one that came to light about 1330. Now despite the

<sup>41</sup> See most conveniently Billanovich, *Tradizione e fortuna di Livio tra Medioevo e Umanesimo* (Padua, 1981), p. 124.

<sup>42</sup> Beryl Smalley, *Arch. Fratr. Praed.* 24 (1954), 95 and *English Friars and Antiquity in the Early Fourteenth Century* (Oxford, 1960), pp. 92–7. She mentions a copy of all three decades written at Modena, namely Paris Lat. 5741 of 1405 (p. 95 n. 5), and there is another, written by a scribe who like Guido de Guisis came from Reggio: Ambros. D 542 inf., dated 1388–9. Their texts differ considerably; see *Riv. Fil.* 114 (1986), 135, 141 with n. 2, and 115 (1987), 141 (on Esc. g I 8), 143, 145.

<sup>43</sup> K. A. de Meyier, *Codices Vossiani Latini I: codices in folio* (Leiden, 1973), p. 45.

<sup>44</sup> Ruth J. Dean, *Med. et Human.* 3 (1945), 89–90. She discusses the chronicle on pp. 335–9 of 'Nicholas Trevet, historian', in *Medieval Learning and Literature: Essays Presented to Richard William Hunt*, ed. J. J. G. Alexander and M. T. Gibson (Oxford, 1976), pp. 328–52; in the list of manuscripts on p. 351 (and in the index to the volume) read 4929 for 4949. I have not noticed any reference in Billanovich's many publications on Livy to the quotations in the chronicle.

<sup>45</sup> *Riv. Fil.* 115 (1987), 145–6.

<sup>46</sup> B. Jones, *John Le Neve, Fasti ecclesiae Anglicanae 1300–1541* iv (London, 1963), p. 7; Dean, *op. cit.* (1976), 335–6.

<sup>47</sup> François Avril confirms that part II is French. Mainly for stemmatic reasons, I had wondered whether it might be Italian.

latitude of such designations, I doubt whether in s. xiv<sup>2</sup> even the script of P would have been considered *uetustissima litera*. It seems likely, therefore, that the manuscript was old enough to be an ancestor of P, a possibility that no-one has yet considered. Whether it could indeed have been an ancestor of P only internal evidence will tell, and so I proceed to it.

*Uncontaminated relatives of P in Florus*

A much purer text of Florus than any that editors have reported from Italian manuscripts occurs in nine manuscripts known to me.

**Mss. 1–2.** When Billanovich discussed Ambros. F 138 sup., just mentioned, he showed that its text of Florus shares errors with P. More to the point, it seems to have undergone almost no contamination from e.<sup>48</sup> Billanovich also observed that its text of Florus was copied into Ambros. G 98 sup. in s. xv<sup>med.</sup> (pp. 139–41).

**Mss. 3–4.** Of the three scribes who claim to have found the *Periochae* annotated by Petrarch in *codice uetustissime litere*, I argued in my previous article that one, the scribe of Escorial S III 21, took the note and the text from different sources (p. 483). The other two manuscripts, Naples Naz. IV C 32 (membr. xiv/xv) and Kraków Jagiell. 416 (chart. xv), closely resemble each other, for instance in omitting 2.2.2 *bello*, 15 *iam*, 29 *consule*, 30 *consule*, 4.2.2 *quidem*, 7 *odia*, and it must have been their common source that was copied from the manuscript referred to in the note; when the scribe of Kraków 416 adds at the end *hoc exemplui ex uetustissimo codice*, that too must have been mechanically transcribed from the exemplar. In their original text they show no more contamination than Ambros. F 138 sup., but Naples Naz. IV C 32 has a number of variants, corrections, and notes, and an ancestral transposition has been corrected by rearrangement of fos. 13–21, 11–12, 22–30, as fos. 11–30 (with consequent rewriting in places but no erasures). These interventions are due to Gasparino Barzizza and his son Guiniforte; ‘Guiniforti Bergigii’ appears on the flyleaf, and the subscription of the *Periochae*, not written by the scribe beyond the first word, reads ‘Explicit epithoma in centum xl libros Titi Liuii mei Gasparini Pergamensis’. I shall return later to some results of the contamination and correction that Gasparino and Guiniforte carried out. The original text was probably written in the Veneto rather than in Lombardy.<sup>49</sup> The marginal notes attracted the attention of Sabbadini, who regarded them as Petrarcan in content; but as they are in hands that all look different from the scribe’s, anything of Petrarcan origin would have had to come from the exemplar in a second phase of copying, and U. Lepore has since argued that the notes suit the interests of Gasparino even better.<sup>50</sup> Some at least must have come from the exemplar, however, because they recur in Kraków 416. The watermarks in this manuscript are Italian of s. xv<sup>2/4</sup>, but it was presented to the library at Kraków by Andreas Grzimala, who died in 1466, and the hands in all three parts must be Polish rather than Italian.<sup>51</sup>

**Mss. 5–8.** Three Florentine manuscripts of Florus in which the *Periochae* follow have undergone almost no contamination from e: Laur. Edili 186 (membr. xiv<sup>2</sup>), S. Croce 20 sin. 11 (membr. c. 1405), and S. Croce 19 sin. 9 (membr. xv<sup>3/4</sup>). In my previous article (pp. 483–4) I surmised that the first of these, which contains only Florus and the *Periochae*, would turn out to be the most important Italian

<sup>48</sup> Besides the agreements with P listed by Billanovich, op. cit. (n. 3), 88 n. 2, some agreements with NP against A can be seen in plate IV (opposite p. 95).

<sup>49</sup> So Albinia de la Mare judges from the illuminated initial.

<sup>50</sup> *Giorn. Ital. di Filol.* 3 (1950), 347–51.

<sup>51</sup> Cf. M. Zwiercan in *Catalogus codicum manuscriptorum medii aevi latinorum qui in bibliotheca Jagellonica Cracouiae asseruantur*, ii (Wrocław, 1982), pp. 240–2.

manuscript, but it is rather less accurate than the second, in which one hand wrote Florus and the *Periochae* and two others the dated text of Justin (a. 1405). The third, which has the same contents as the second, appears to descend from it but is too inaccurate to matter even if it does not. In 2.1–2 and 4.2 I did not notice any errors common to the first and second. Though I have found no evidence to confirm my assumption that these Florentine manuscripts all descend from one used by Boccaccio, a question that I asked about the text of *Per.* 135 has a positive answer that suggests it may be true of Edili 186: one of the two manuscripts recorded in 1451 at S. Spirito, to which Boccaccio's library passed, read *gñs Alpine* for *gens Alpina*, and Edili 186 reads *gens Alpine*.<sup>52</sup> A close relative of Edili 186 in both Florus and the *Periochae* is Ferrara Civ. II 191 (chart. xv<sup>1</sup>), a predominantly historical miscellany written in two columns by someone who has left no trace of his identity except his small and nasty script.<sup>53</sup>

**Ms. 9.** West Berlin Lat. 4<sup>o</sup> 343 (membr. xiv, Italian) consists of five leaves in the same hand. Fo. 1, a badly worn vertical strip, exhibits parts of Florus 1.1.17 *ob asperius ... 7.2 egit quam acquis-*; fos. 2–3, an inner bifolium, *Per.* 41.1 *Ignis ... 48.7 senatus pariturum*; and fos. 4–5, presumably an outer bifolium, *Per.* 57.2 *cogebat. ... 59.20 subegerat* and 103.2 *uxorem Metelli ... 107.3 nulli alii*. None of these passages even overlaps with any of those that I selected for collation, and I had nothing better at hand to collate them against than another manuscript in the same collection, Lat. 4<sup>o</sup> 436; but the readings that I noted turned out to include several not cited by Jal from any of his manuscripts but found in Naples Naz. IV C 32 and Kraków 416, which I have on microfilm: Florus 1.5.1 *accepit petens*, 1–2 [*misicuisset ... tribus*], 6.1 *inuadit urbis*, *Per.* 42.6 [*ad*] *disceptandum*, 43.5 [*in Thracia*], 45.5 *cum [de] eo*, 10 *rex Bithyniae*, 47.1 [*in*] *dicta*, 59.6–10 [*Metellus ... Q.*] (restored in the upper margin by the same hand). I subsequently looked for these errors in Edili 186 and S. Croce 20 sin. 11, which have only the first (Edili 186: S. Croce 20 sin. 11 omits *quamuis ... quippe*). The manuscript is therefore a relative of Naples Naz. IV C 32 and Kraków 416. Perhaps other fragments of it will come to light elsewhere.

Already at *Praef.* 2 these manuscripts reveal their independence of e and A by not interpolating *enim*, but on the other hand the Florentine manuscripts interpolate *nam* with Ae (Ambros. F 138 sup. jumps from *discant* to *laboribus*), and at *Praef.* 5 all nine (except that the last is absent) substitute *matrem* for *cum ipsa* (NP1) with Voss. and Cambridge C.C.C. 313 (*matrem suam e*). Even these nine manuscripts, then, seem not to be wholly free of contamination. In 2.1–2 and 4.2, however, they share no errors with e or A but many with P: 2.2.11 [*ita*], 19 *belli prima*, 21 <et>*iam*, 22 *urbis*, 4.2.59 *aedificiorum proximorum*. At 4.2.87 they mostly preserve the meaningless *in Celti* of NP1 (omitted by e), and some of them preserve such transparent errors of NP as 2.2.15 *infamem*, 16 *nisi* <ut>, 4.2.51 [*ut*] *cum*, 91 *usum*. In my previous article, after reporting Billanovich's view that the Italian manuscripts of the *Periochae* all derive from a copy of P, Briscoe's that they have no independent authority, and Reid's that they derive from two relatives of P, I gave reasons for at least associating them with P (pp. 479, 482, 484). That relationship is now confirmed.

<sup>52</sup> A. Goldmann, *Centralblatt für Bibliothekswesen* 4 (1887), 153. Roszbach, ed. of Florus, p. xxii, resolved *gñs* as *gens*; Antonia Mazza, *I.M.U.* 9 (1966), p. 51, as *gentes*.

<sup>53</sup> Anna Maranini Gobbo very kindly supplied me with film of Florus 2.1–2 and 4.2 before I was able to see the manuscript itself. G. Procacci, *S.I.F.C.* 19 (1912), 40 n. 26, gives a description, misleading inasmuch as it suggests that the text of Loschi's Ciceronian commentary on fos. 238r–265v postdates more than one printed edition. The watermarks that I noticed, one of a dog and the other of a half griffin, are securely Italian but do not sufficiently match any in Briquet.

P itself, however, cannot be the source of the nine manuscripts, because they do not share with it e.g. 2.2.21 *ipsis dis*, 4.2.1 *placato, fortuna inuidens*, 6 *russumque*, 12 *inuasit*, 33 *Vulteo tribuno*, 87 *inter ima* (readings that Jal with one exception does not report). The *Periochae* supply an even stronger argument. I have now checked the reading of N and P in 146 passages, and though Jal was wrong four times about N and no fewer than 18 times about P,<sup>54</sup> on one crucial reading he was right: P omits *Per.* 133.3 *et occisus*, and no corrector ever restored it. The Italian manuscripts all have it, and that by itself establishes the truth of Reid's contention that they do not derive from P.<sup>55</sup>

When I had collated Florus 2.1–2 and 4.2 in the nine purer manuscripts, it seemed that they shared several errors absent from P: 2.2.1 [*Romanus*], 9 [*apud*], 22 *appro<pinqua>batur*, 29 P. for *Appio*, 36 *toto tum*, 4.2.8 <et> *L.*, 21 *tribunis*, 25 *ablata omnia*, 26 *cruentum* (N), 43 *est Thessalia*, 47 *circumuenturum*, 57 *Pompeio aedem*, 68 *et* for *sed*, 76 [*uiri*], 79 *timens* for *habens*. On checking P, however, I found that Jal had simply failed to report these errors in 13 of the 15 passages; only at 2.2.29 and 4.2.79 does P give the correct text. It would be hazardous to conclude from such meagre evidence that the Italian manuscripts of the complete text all derive from one source inferior in some respects to P. The same goes for the *Periochae*. In several places Jal's apparatus ascribes a true reading to NP and an error to 'rell.', but all that survives inspection of P is the omission by 'rell.' of *cum* before *pluribus* at 102.5 (*compluribus* Jahn); at 48.20, where Jal reports *etiamnunc* NP (*multo*) *etiam* rell., P<sup>1</sup> has *etiam uim* and P<sup>2</sup> *etiam*, and though 'rell.' do mostly agree with P<sup>2</sup>, what P<sup>1</sup> reproduced as *uim* survives as *in* in Laur. 89 inf. 25, a Florentine manuscript of the *Periochae* alone (I shall return to it). No more in Reid's dissertation have I been able to find significant errors common to all the Italian manuscripts.

I therefore suspect that Petrarch's manuscript of Florus and the *Periochae* was indeed an ancestor of P. The state of its text on the first leaf may lend support. In Edili 186 a space was originally left between *Praef.* 7 *iam ipsa* and 1.1.9 *qui sub Aenea*, and Ferrara Civ. II 191 passes from one point to the other without warning. The not much later hand that supplemented Edili 186, overrunning the space and trespassing on the margin, borrowed the missing passage from a contaminated descendant of Q like S (about which more when I come to contamination in the Italian manuscripts). S. Croce 20 sin. 11, which shows no disturbance on the surface, betrays underlying disturbance by sharing e.g. the omission of 1.1.2 *in profluentem* with O and its family. Regrettably, I have not yet been able to collate the whole passage in the oldest descendant of Petrarch's manuscript, Ambros. F 138 sup., but it too omits *in profluentem*.<sup>56</sup> Why all this patching and splicing happened is easily explained: in Kraków 416 the passage has frequent gaps,<sup>57</sup> as it also had in Naples Naz. IV C 32 until someone neatly filled them. One of the gaps corresponds to 1.1.2 *in profluentem cum Remo fratre*, so that the words *in profluentem* were unavailable to anyone who had at his disposal for collation only a descendant of O. As the gaps occur in one

<sup>54</sup> At *Per.* 80.6, where Jal gives *rostra* like Rossbach, *rostris* is perfectly clear and unambiguous in N, and at *Per.* 97.1 neither N nor P adds *et* after *caesis* as Jal says and Rossbach implies. In n. 44 of my previous article I mentioned that according to Reid N omits *Per.* 2.22 *debilis*, but it does not.

<sup>55</sup> U. Lepore, *Giorn. Ital. di Filol.* 5 (1952), 254–60, had already put forward the same contention, though for other reasons, about Naples Naz. IV C 32.

<sup>56</sup> See Billanovich's plate (cf. n. 48).

<sup>57</sup> They can be seen in the plate given by I. Lewandowski, *Florus w Polsce* (Wrocław, 1970) opposite p. 32. I am most grateful to Mieczysław Mejor of Warsaw for providing me with a copy of this monograph.

passage and not in two passages separated by a run of unbroken text, the manuscript responsible for them must have suffered damage not to its first leaf but just to the lower part of its first page, presumably through wear or a stain. Though such accidents can befall manuscripts of any age, older manuscripts must be more exposed to them.

If Petrarch's manuscript was an ancestor of P, it will have given rise independently to P in Florus and the *Periochae*, to Ambros. F 138 sup. in Florus, and to at least two Italian families in Florus and the *Periochae*. When I questioned Reid's stemma in my previous article and looked for a way of taking all the Italian manuscripts back to one source, I had in mind a collateral rather than an ancestor of P, but an ancestor will serve equally well. For the sixth time in this article, then, I am postulating a hyparchetype that produced more than two lines of descent: first A, then the source of NPΔ, then e, a lost copy of O, and k, and now the source of all the complete manuscripts except N. As such a stemma rests on the absence of significant common innovations, it can never be established by purely textual evidence,<sup>58</sup> but a bipartite stemma, which can, has no claim to preference when the textual and external evidence for it is weak. I have argued that in both Florus and the *Periochae* the textual evidence for a bipartite stemma on the branch opposite N is weak. It remains to determine how many branches the Italian tradition has. Can more be discerned among contaminated manuscripts of Florus, and are there more in the *Periochae* than in Florus?

*Petrarch's copies of Florus and contamination in the Italian tradition*

Petrarch, whom *Annei Flori florentissima breuitas ad inquirendas Titi Liuii reliquias animauit*,<sup>59</sup> not only annotated the old manuscript that brought the *Periochae* and a new text of Florus into circulation but also owned and annotated two extant manuscripts of Florus: Paris Lat. 5690 (c. 1290–1330), which derives from e through k and O, and Q (Paris Lat. 5802, s. xii), which derives more straightforwardly from e. He even appears to have used a fourth manuscript. A sentence of *De gestis Cesaris*, written after 1366 and therefore one of his last works, reads as follows:

Ita pulsus ex Etruria Libo, Thermus ex Vmbria, Esculo Lentulus, captus Gneus Magius, prefecti ambo.

An autograph of the work, Paris Lat. 5784, breaks off before this point, and the manuscripts of the rest apparently go back to an unfinished draft, which at this point lacked the name of the second *praefectus*.<sup>60</sup> It is *pulsus* . . . *Lentulus*, however, that concerns me. Though Florus was not Petrarch's main source hereabouts, the compression reflects 4.2.19:

tum pulsus Etruria Libo, Vmbria Thermus, Domitius Corfinio.

O of Florus omits *Domitius*, and descendants that restore it mostly put it after *Corfinio*. Paris Lat. 5690 after correction is one of these, and Petrarch's other surviving manuscript, Q, correctly reads *Domitius Corfinio*, as his manuscript of the complete text must have done. He passes over *Domitius* at this point not because any

<sup>58</sup> See e.g. S. Eklund, *Ann. Soc. Litt. Human. Reg. Upsaliensis* (1975–6), 82 n. 16. Had I known this article, 'On Errors and Contamination', I should have cited it in 'Stemmatic Method: "qualcosa che non funziona"?', in *The Role of the Book in Medieval Culture*, ed. P. Ganz (Bibliologia 3–4, Turnhout, 1986), pp. 57–69. I thank the author for bringing it to my attention.

<sup>59</sup> *Fam.* 3.18.5, ed. V. Rossi, vol. i (Rome, 1933), pp. 140.39–40.

<sup>60</sup> G. Martellotti, *Ann. Sc. Norm. Sup. di Pisa* III 9 (1979), 1468–74 = *Scritti petrarcheschi*, ed. M. Feo and S. Rizzo (Padua, 1983), pp. 555–60; on the date of *De gestis Cesaris*, *ibid.* 484. Martellotti's *itaque* seems to be a slip: both Schneider and Razzolini give *ita*.

of his manuscripts omitted the name but in order to treat the episode more fully a few sentences later. The interesting detail is *Esculo Lentulus*, absent from Paris Lat. 5690, Q, and all manuscripts of the complete text, but added after *Vmbria Thermus* by a descendant of O, namely Vat. Lat. 1860, and some later relatives. Whether or not the date 1313 in Vat. Lat. 1860 can be trusted,<sup>61</sup> a date later than 1366 seems out of the question, and with it the possibility of interpolation from *De gestis Cesaris*. Evidently, therefore, someone before Petrarch had supplemented Florus from Lucan 2.468–9 (or less probably from Caes. *B.C.* 1.15.3), and Petrarch saw a manuscript that included the supplement. That he ever owned one, though, I see no reason to believe.

As for the three manuscripts that Petrarch more obviously used, I have found only four Italian manuscripts that seem at all likely to derive from Paris Lat. 5690 (all mentioned above among the descendants of O), but most derive in some measure from the other two. More than anything else, in fact, use of Petrarch's library must account for the diffusion of Florus and the *Periochae*. After his death in 1374, part of it remained in the neighbourhood of Padua and part was carried off to Pavia by the Visconti in 1387. It has long been known that in 1426 the Visconti library included Q, and I pointed out in my previous article (p. 482) that it also included a manuscript of Florus and the *Periochae*, though there is no way of telling whether it was the old manuscript or a recent descendant. External evidence has not yet revealed anything else relevant to transcription from Q or from the old manuscript, and so information must be elicited from the extant tradition.

The survey that follows, which I fear consists largely of shelfmarks and readings, has a double purpose: the one announced at the end of the last section, to look for strains of the complete text unrepresented by any of the manuscripts yet discussed, and a more polemical one, to establish the worthlessness of the contaminated manuscripts used by Malcovati and Jal. I shall try not to lose sight of history.

### *Pure descendants of Q*

The corrections in Q are sporadic rather than systematic, but in 2.1–2 and 4.2 three of them can only have come from the complete text: 4.2.60 *de imbelli ac* for *debellata*, 79 *timens for habens*, and 89 *bis* for *hic*. Contrary to what I predicted in my previous article (p. 489), they do not seem to be in any version of Petrarch's hand,<sup>62</sup> but they had certainly been made by 1396, when a *ciuis Venetianus* wrote Vat. Pal. Lat. 895, and probably by 1387, as will emerge in a moment. The pure or fairly pure descendants of Q fall into two families.

One family goes back to a copy of Q that either antedated the corrections or ignored them. Its oldest member is Naples Naz. IV C 30, carelessly written in a chancery hand of s. xiv<sup>2</sup> and identifiable by means of its *explicit* with the third manuscript of Florus that belonged to the Visconti library in 1426.<sup>63</sup> More accurate but much later is Vat. Lat. 10159 (chart. 1468). Errors of these two manuscripts include 2.1.1 *cum tamen* for *tum*, 2.2.14 *uinxerit/iunxerit* for *scripserit*, 15 *M.* for *L.*, 16 *incendio* for *excidio*, [uictoriae], 23 *uisu*, 25 *sui* for *siue*, 29 *superatus* [est], 4.2.6

<sup>61</sup> Cf. n. 22.

<sup>62</sup> A. Petrucci, *La scrittura di Francesco Petrarca* (Vatican, 1967), pp. 125–6 n. 39, assigns Petrarch's notes in Q to about 1350–5, and other texts in it, for instance Suetonius, certainly have notes for which the ascription is incontestable and the date very plausible; but it would be useful to know what he makes of the notes on Florus, which to an inexperienced eye are in at least two hands both different from Petrarch's of c. 1350–5. Cf. Nollac, *Pétrarque et l'humanisme*<sup>2</sup> (Paris, 1907), i.246–8.

<sup>63</sup> É. Pellegrin, *La bibliothèque des Visconti et des Sforza* (Paris, 1955), 148 A 341. The words *redactus occubuit* conclude an extract from the *Getica* of Jordanes, 27.139–33.171 (fos. 18v–19v).



[*quattuor anni*], 26 <P.> Cn., 33 mille [iuue]num, ipse for in se, 43 increp[ab]ant, 47 abundaret copia, 52 mali, 56 cred- for uid-, 59 paene in insulam, 60 oblitum repertum est limo, 66 maior eoque amplior (amplior eoque amplior Q), 85 ei factus for effectus. Jal's T (Pavia Ald. 228, membr. xv<sup>1</sup>) and Vat. Lat. 1494 (chart. 1470, Pisa or Volterra?) belong to the family in 4.2 but not in 2.1–2.

A copy of Q + Q<sup>2</sup> that seems to have been made in the Veneto, and so presumably before 1387, read e.g. 2.2.28 *praeda magna*, 4.2.27 [uerni], 37 *gubernatorem* <Amiclatem>, 71 *semel*<que>, and produced at least two lines of descent.

(1) One is represented by Florence Naz. Magl. XXIII 14 (membr. xiv<sup>2</sup>)<sup>64</sup> and its close relative Holkham Hall 366 (s. xv<sup>3/4</sup>), by Vat. Pal. Lat. 895 (mentioned above for its date, 1396),<sup>65</sup> and by Vicenza Bertol. G 3 8 3 (c. 1400) and its close relative Wrocław IV F 38a (membr. xv<sup>1/4</sup>).<sup>66</sup> It read e.g. 4.2.2 *detonuit*, 16 *more maiorum*, 33 *duas* [tamen], 91 [dictator].

(2) The other is represented by Geneva Bodmer 121 (chart. xv<sup>1</sup>),<sup>67</sup> Vat. Lat. 1859 (membr. c. 1400, Bologna?), Venice Marc. Lat. Z 367 (membr. 1421, Veneto), and Bologna Univ. 2476 (chart.; Florus is followed in a hand that may be different by texts written at Vicenza in 1409–12). It read e.g. 4.2.4 *ciuile for bellum*, 14 *gloria for dignitas*, *fortuna tanti imperii*, 23 *portas duci*, 47 *equitatum copia*, 61 <et> a *Caesare*, 67 [ef] *fugerent*, 68 *mane for naue*, 69 *se cum, satisfecit*, 72 *medici uirum tentare*, 75 [legati].

Genoa Durazzo 220 (membr. c. 1400) and Laur. S. Croce 20 sin. 8, where Florus is preceded in the same hand by a text of Sallust written at Castiglion Fiorentino in 1404, generally side with the latter group and like Bologna Univ. 2476 pick up *timens* for *habens* from Q<sup>2</sup> at 4.2.79.<sup>68</sup> In 4.2 Manchester Rylands 48, Florentine of about 1450–60, resembles them but reads *habens*; abbreviations often threw the scribe, who resolved some wrongly and left gaps for others.<sup>69</sup> Underneath further contamination Cesena S. 15.5 (membr. xv<sup>med.</sup>) recognizably belongs to this second line of descent.

### *Contaminated descendants of Q*

Errors committed by the first copy of Q recur very clearly in one contaminated family and less clearly in another.

Two errors in 2.1–2 peculiar to Naples Naz. IV C 30, 2.2.2 *bello reuocandam* and 6 *statim*[que], together with others that it shares with Vat. Lat. 10159, e.g. 2.1.1 *cum*

<sup>64</sup> On this manuscript, a palimpsest apparently from Emilia, see V. Fera in *Codici latini del Petrarca nelle biblioteche fiorentine*, ed. M. Feo (Florence, 1991), p. 52 no. 23.

<sup>65</sup> On Pal. Lat. 895 see also n. 98. In 2.1–2 two relatives are Verona Capit. CCXXXIII (198) (a. 1431) and its descendant Ambros. E 73 inf. (membr. 1458 ut uid.). All read 2.2.3 *ut eodem*, 23 [ducem], 25 [suos], 26 *grauior*, 28 *insignis*.

<sup>66</sup> This manuscript, bought by the library in 1826 according to B. Mostowska-Głombiowska, *Eos* 61 (1973), 258, fits the description of one in private ownership that F. Jacob collated at Poznań; see A. Klette, *Catalogi chirographorum in Bibliotheca Academica Bonnensi seruatorum particula I* (Bonn, 1858), pp. 39–40 nos. 166–7. In an article of 1987 published at Debrecen (written in Hungarian and summarized in German), L. Havas associated it with a manuscript that I am about to mention, Vat. Lat. 1859.

<sup>67</sup> I have not seen this manuscript and am much obliged to Harry Jocelyn for collating Florus 2.1–2 and 4.2.1–25.

<sup>68</sup> The first page of Genoa Durazzo 220 (B VI 9) is illustrated by D. Puncuh, *I manoscritti della raccolta Durazzo* (Genoa, 1979), fig. 111. I am extremely grateful both to Dr Puncuh and to Franco Montanari for supplying me with reproductions of 2.1–2 and 4.2.

<sup>69</sup> Albinia de la Mare kindly gave me her opinion on the date and origin of the manuscript. A change of hand on fo. 43v in the middle of 3.18.6 *Telesinus* may have brought with it a change of exemplar, which certainly took place somewhere between 2.2 and 4.2. I return below to the other exemplar; cf. n. 75.

*tamen* for *tum*, 2.2.15 *M.* for *L.*, 16 *incendio* for *excidio*, 29 *superatus* [est], recur in a Lombard family attested from s. xv<sup>1/4</sup>: B.L. Add. 25450, Ambros. A 85 inf., C 109 inf., and S 63 sup., and Bodl. Auct. F 5 9. They have a large number of distinctive errors, especially transpositions, e.g. 2.2.2 *atque* for *et quasi*, 3 [uiam], 5 *non* [est], 11 *interemptus*, 21 *premebat* for *urgebat*, 25 *quid* <autem> *aliud*, 27 *altius spirantibus*, 29 [ille], 4.2.5 [inde], 10 [uellet *tamen* auctiores opes], *eleuabatur*, 20 *naugio*, 22 *adit* for *habet*, 25 *bellum erat*, 33 *haesit in uadis*, 38 *copiis omnibus*, 40 *irrita expugnatione Dyrrachii*, 45 *theatri sui nocturna imagine*, 46 *Caesaris exercitus*, 49 *comitata est ruina*, 58 *Pompei percussoribus*, 59 *paene in insulam*, 74 *dabat partibus*, 75 *mari* <pugna>, 80 *inter utrosque ingens silentium conuenisset quasi*, 83 *fugae faciem*, 84 *in occasionem attribuit*, *suos credunt fugere fugere* <etiam>, 91 *regni uestigia*. Most of these errors also occur in B.L. Egerton 938 (membr. 1454), written somewhere in northern Italy. An obvious source for the numerous readings that the family owes to the complete text is Barzizza's manuscript, Naples Naz. IV C 32, but I should not like to rule out Petrarch's manuscript itself, which could have been at Pavia since 1387.

A sprinkling of readings from the complete text can be seen incorporated from an early date in manuscripts connected with Florence. Jal's S (Paris Lat. 9680, membr. xiv<sup>2</sup>), written in Italy, has twice on a flyleaf (fo. iiv) the initials *A.C.* and at the end of the text (fo. 27v) the note *Iste liber est Antonii Thommasi de Corbinellis, cuius uita sit longeva et sibi ac suis saluberrima, in gratia domini nostri Yesu Christi, qui uiuit atque regnat in secula seculorum amen, anno domini 1398*; this scholar and collector, important in the early history of Florentine humanism, left his manuscripts to the Badia there in 1425.<sup>70</sup> S reads e.g. 2.1.2 [*Asiam*], 2.2.17 *Punico*, 21 *belli caput*, 4.2.8 [*quae*], 32 <*ex*> *Antonio*, 40 *quadraginta* for *uiginti*, 47 *abundaret copia*, 94 *datus Caesari*. The errors at 4.2.8, 32, and 47, occurred in the first copy of Q, and though 8 [*quae*] and 32 <*ex*> *Antonio* could be conjectures of a kind apt to be made more than once, the fundamental text must have come from a manuscript that like HQ omitted 2.2.9 *quasi*, and I know of no reason why it should not have been the first copy of Q, even though nothing else connects it with Florence.

S has interesting relatives. The oldest is West Berlin Lat. 8<sup>o</sup> 4 (membr.), written in a small and neat humanistic script of a kind well attested at Florence between 1405 and 1420. Did someone take it to the Council of Constance? At any rate, it may be the source of Greifswald Univ. 2<sup>o</sup> 680 (membr. 1417), written at Constance for Johann Abezyer, bishop of Ermland, by a northern scribe whose name could well emerge under ultra-violet from the erasure on fo. 147r.<sup>71</sup> Until I inspected these two manuscripts, I had no idea why the ed. Colon. c. 1474 (*G.W.* 10094) resembles S; but Constance provides a possible link. So does a manuscript now lost, Bonn Univ. 169 (chart.), which was at Duisburg when Duker collated it for his edition (Leiden <sup>1</sup>1722, <sup>2</sup>1744) and H. A. Grimm published a collation against Graevius's edition.<sup>72</sup> Duker

<sup>70</sup> R. Blum, *La biblioteca della Badia fiorentina e i codici di Antonio Corbinelli* (Studi e Testi 155, Vatican, 1951); A. C. de la Mare in *Das Verhältnis der Humanisten zum Buch*, ed. F. Krafft and D. Wuttke (Boppard, 1977), pp. 96–8. This is presumably the manuscript of Florus described by Montfaucon, *Diarium Italicum* (Paris, 1702), p. 375 ('*Lucii Flori* codex membr. In fine legitur: *Iste liber est mei Antonii Tomasi de Corbinellis conscriptus anno 1398*'), and spoken of as untraced by Blum, 48 n. 3, and de la Mare, 98 n. 31.

<sup>71</sup> Cf. P. L. Schmidt, 'Eine Cicero-Handschrift des ermländischen Bischofs Johannes Abeziey', *Rhein. Mus.* 109 (1966), 170–84.

<sup>72</sup> Klette, op. cit. (n. 66), 40–1. The manuscript was reported as missing by V. Burr, *Universitätsbibliothek Bonn: Verzeichnis der nach dem 2. Weltkrieg als fehlend festgestellten Handschriften* (Bonn, 1968), p. 18, and Dr Thomas Klein kindly reports that it has not resurfaced. For Grimm's description and collation see his *Beschreibung und Vergleichung einiger*

says that it resembled what Gruter believed to be the *editio princeps*, namely the ed. Colon., and the readings that he and Grimm cite bear him out. The preceding text, Justin, was written at Cologne in 1450 for Moritz von Spiegelberg, who himself wrote the following text, the *Georgics*, at Florence in 1441. Grimm regarded the copy of Florus as somewhat earlier than either, though also German because the scribe sometimes used *gw* for *gu*; but if it was written at Florence and brought back to Cologne, it could lie behind the edition.

A later Florentine relative of S is Laur. 89 inf. 37 (chart. xv<sup>2/4</sup>), one of many Gaddiani that served as exemplars for calligraphic copies.<sup>73</sup> Descendants of this one include Laur. 63.12 and 66.38, B.L. Harl. 2731, Naples Naz. IV C 33, Poblet 50–51–52, Valencia Univ. 482, Vat. Barb. Lat. 175 and Reg. Lat. 759 and 1779, Verona Capit. CXXXVII (125), Wolfenbüttel Gud. Lat. 175, and in 4.2 Florence Naz. B.R. 36.<sup>74</sup> Not all derive separately from Laur. 89 inf. 37: Laur. 63.12, Poblet 50–51–52, the two Reginenses, and Verona Capit. CXXXVII, together with B.R. 36 in 4.2, share e.g. 2.1.2 *fuit*, 2.2.15 [*iam*], 4.2.1 [*erat*], 8 [*potentia*], and have imported from elsewhere at least 4.2.10–11 *eloquentia*... *Caesare*, omitted by Laur. 89 inf. 37 and its other descendants.

The errors of S and its relatives recur in another Florentine manuscript, Manchester Rylands 48, but only in 2.1–2;<sup>75</sup> and many also recur in Budapest Nat. 168 (membr. xv<sup>2/4</sup>), written by a scribe connected with Poggio,<sup>76</sup> and Dresden Dc 176 (membr. xv<sup>3/4</sup>), closely related to it.

Most of the errors committed in 2.1–2 by the first copy of Q appear in Ambros. L 10 sup. (chart. xv<sup>med.</sup>) and in the closely related pair Vat. Reg. Lat. 1730 (membr. xv<sup>med.</sup>) and East Berlin Hamilt. 261 (membr. xv<sup>3/4</sup>, Rome).<sup>77</sup> Many of its errors in both 2.1–2 and 4.2 appear in Siena Com. K VI 47, written at Spoleto by Matteo Petrucciani in 1415 or thereabouts.<sup>78</sup>

Errors committed by the second copy of Q, which as a copy of Q+Q<sup>2</sup> already incorporated some contamination, recur in a family contaminated still further, which includes Jal's F (Rome Vallicell. B 2, membr. c. 1400<sup>79</sup>). The closest relative of F is Laur. 67.18 (membr.), written by a scribe active in the 1420s at Florence or S.

*lateinischen Handschriften in der Duisburgischen Universitäts-Bibliothek*, in Joh. Hildebrand *Withof kritische Anmerkungen über Horaz und andere römische Schriftsteller* (Düsseldorf, 1792–1802), i.97–119, ii.147–73.

<sup>73</sup> See most recently J. Delz's Teubner edition of Silius Italicus (Stuttgart, 1987), pp. xv–xvi.

<sup>74</sup> On Laur. 63.12, Harl. 2731, Naples Naz. IV C 33, Valencia Univ. 482, Barb. Lat. 175, and Verona Capit. CXXXVII, see de la Mare, *op. cit.* (n. 24), 530 no. 62 (9), 537 no. 68 (9), 599 no. 85, 532 no. 62 (53), 504 no. 31 (17), 504–5 no. 32 (41). She kindly tells me that Wolfenbüttel Gud. Lat. 175 is Florentine; like Harl. 2731 and Naples Naz. IV C 33, it contains only the *Periochae* and Florus. Reid missed it but groups together Laur. 63.12 and 66.38, Harl. 2731, Naples Naz. IV C 33, and Barb. Lat. 175 (pp. 386–406); the Reginenses and Verona Capit. CXXXVII do not include the *Periochae*, and Poblet 50–51–52 and Valencia Univ. 482, which Reid also missed, took them from different sources.

<sup>75</sup> See n. 69. Similar in many respects, for instance in interpolating *stare(n)t* after 4.2.80 *acies*, is Glasgow Univ. Gen. 212, which Roger Green very kindly collated for me; but it also shares some readings with the next group to be discussed.

<sup>76</sup> As Albinia de la Mare confirms, he wrote Bodl. Canon. Class. Lat. 300 (Livy III) and Philipps 6744 (Livy IV); see *Riv. Fil.* 114 (1986), 166 n. 1, 115 (1987), 159 n. 1. László Havas did me the great favour of sending me a complete reproduction of the manuscript.

<sup>77</sup> For no obvious reason, J. Giacone Deangeli illustrates Hamilt. 261 in her edition of Florus (Turin, 1969), opposite p. 400.

<sup>78</sup> See E. Mecacci, *La biblioteca di Ludovico Petrucciani* (Milan, 1981), pp. 46, 154–7.

<sup>79</sup> Lidia Avitabile, *Studi Medievali* III 11.2 (1970), 1019–20, says 's. xv', and I may well have put it too early.

Gimignano;<sup>80</sup> from this descends Holkham Hall 367, and from that in turn B.L. Harl. 4793 (chart. xv<sup>3/4</sup>, Florence). Other members of the family are Madrid Univ. 141 (membr. xv<sup>1</sup>, Florence), Göttingen Lüneburg 3 (chart. xv<sup>med.</sup>, later 'Iulii Maffei Volaterrani'), Nürnberg Ratsbibl. Cent. IV 88 (chart. xv<sup>3/4</sup>, Vat. Ross. 333 (membr. xv<sup>med.</sup>), the closely related triad B.L. Harl. 2765 (membr. xiv<sup>ex.</sup>, Florence), Ambros. S 16 sup. (membr. xv<sup>1</sup>), and Naples Naz. IV C 31 (chart. xv<sup>3/4</sup>), in 2.1–2 Vat. Lat. 1494, and in 4.2 Laur. Edili 168. Errors include 2.1.1 *robur est*, 2.2.6 [*regem tanta celeritate*], 21 [*onustam*], 4.2.35 [*bello*], 83 [*quod*].<sup>81</sup> In 2.1–2 and 4.2 the only readings of the complete text that have infiltrated this family are 2.2.22 *urbis* (coincidental ?), 4.2.82 *agmen*, 90 *hic aliquando finis armis fuit reliqua*. The last also appears among the rare corrections in Salutati's manuscript, K, but without *reliqua*, and so the family cannot have taken it from K;<sup>82</sup> but the arms in its earliest member, Harl. 2765, belong to Bracciolini of Pistoia, and the manuscript must have been written at about the time when Salutati was taking the young Poggio under his wing.<sup>83</sup>

The influence of the second copy can also be seen, I think, in two more families, though neither derives fundamentally from it.

(1) So far as I am aware, Vat. Ottob. Lat. 1979 (membr. 1437) and Vat. Lat. 1861 (membr. 1446) could have obtained from it all their readings from the complete text, and it is the presence of such readings, rather than of errors introduced by it, that leads me to suspect its influence. Obviously one could derive them from Q by way of a third copy. Their main source, however, was a descendant of O, probably one of those that omitted 2.2.5 *monstris*. Errors peculiar to the pair include 4.2.37 *primum* for *ipsius*, 60 *belli totius*, 68 *se bene*, 85 *inter barbaros etiam*.

(2) The other family includes the manuscripts that I cited above, under 'Contamination in northern Europe', as contaminated descendants of O. They too, so far as I am aware, could have obtained from Q<sup>2</sup> all their readings from the complete text. Contamination from an Italian copy of Q+Q<sup>2</sup> is not the only reason for supposing that the family originated in Italy: characteristic errors, e.g. 4.2.26 *incertum* for *incruentum*, 31 *castra litore*, 57 *dubium*, 71 *primum circa*, recur in two Italian manuscripts, Bologna Univ. 2654 (chart. 1467, 'per me Ianum Albutium Nouariensem') and Vat. Chig. H VII 246 (chart. 1466, Siena?), and in the ed. Rom. c. 1475 (G.W. 10096). As Vat. Lat. 11475 (membr., Italian) leaves gaps where the edition has strange readings that look like misprints, e.g. 2.2.8 *nautur artes*, 35 *patrota* for *prompta*, it must be a copy of the edition; Aberdeen King's 214 is a northern-European copy of the edition.<sup>84</sup> Escorial O III 23 (chart. xv<sup>3/4</sup>, written in Italy but

<sup>80</sup> The same scribe and annotator wrote and annotated Laur. 48.25, on which see *Riv. Fil.* 114 (1986), 142 n. 2. Martin Davies and Albinia de la Mare have since identified the annotator as Mattia Lupi; cf. *Humanistica Louaniensia* 33 (1984), 6–21.

<sup>81</sup> Another member of this group appears to be Budapest Nat. 167, discussed by L. Havas, *Acta Classica Univ. Scient. Debrecen.* 23 (1987), 85–94.

<sup>82</sup> Ullman, *The Humanism of Coluccio Salutati* (Padua, 1963), pp. 196–7 no. 102 ('Salutati notes chiefly on fos. 1–22'; Florus occupies fos. 1r–27v). I have the same difficulty with the notes in K as with those in Q (cf. n. 62). On Salutati's ownership of K see n. 20.

<sup>83</sup> I owe this information to Albinia de la Mare. Guido Billanovich in *Medioevo e rinascimento veneto con altri studi in onore di Lino Lazzarini* (Padua, 1979), i.302–18, with plates V–VIII between pp. 296 and 297 (I thank Mirella Ferrari for this reference), attributes Ambros. S 16 sup. to Sico Polenton; Albinia de la Mare agrees with me that the plates in Ullman's edition of Sico's *Scriptorum illustrium Latinae linguae libri XVIII* (Rome, 1928) do not support the attribution, and in particular she draws attention to Sico's unusual s, which does not occur in Billanovich's plates. Naples Naz. IV C 31 may derive from it.

<sup>84</sup> I am very grateful to Dr C. A. McLaren, Keeper of Manuscripts, for providing me with reproductions of several pages.

probably by a Spaniard) has 4.2.31 *castra litore* and 71 *primam circa* and generally resembles the family. Jal's  $\alpha$  (Rouen 1130, membr. xv, French), Escorial Z III 20 (membr. xv, illuminated in northern Europe), and Munich C.L.M. 11307 (membr. xv<sup>med.</sup>, Italian), fundamentally belong to the family but have a few readings from the complete text absent from Q<sup>2</sup>, e.g. 4.2.90 *hic aliquando finis armis fuit reliqua*.

#### *Descendants of K?*

If Salutati's manuscript, K, has any descendants, it seems unlikely that many of them owe the staple of their text to it. One that probably does, however, is Jal's  $\gamma$  (Besançon 840, membr. xv<sup>2/4</sup>), written by the 'good French scribe' who worked for Poggio at Rome in the later 1420s or early 1430s.<sup>85</sup> It has the same title as K; like K it includes in the margin at the beginning the biographical note on Florus, which being absent from Q, O, and the complete text, seldom appears in Italian manuscripts; and at 4.2.90 like K<sup>2</sup> it restores *hic aliquando finis armis fuit* without *reliqua*. In its stock of other readings from the complete text it most resembles West Berlin Lat. 8<sup>o</sup> 4 and Laur. 89 inf. 37, for instance in giving the rare variant *agitasse* at 4.2.83 (in the text, whereas they give it in the margin). In the *Periochae* too it has a contaminated text, and one of its sources was a Florentine manuscript like Harl. 2765, which must also have supplied the three letters of Caesar and the four epigrams. It therefore combines all the Florentine materials that I have so far mentioned, no surprise for a manuscript written under the direction of Poggio. A close relative, perhaps a descendant, is San Daniele del Friuli 71 (chart. 1442), signed by Guarnerio d'Artegna.<sup>86</sup> Both omit 4.2.14 *iam*, 34 *regis*, 48 *fusus*, 95 *sanguine* before *suo*.

#### *Venice Marc. Lat. Z 368*

The letters of Caesar and the epigrams apparently entered the tradition of Florus and the *Periochae* not at Florence but in the Veneto, and the manuscript where they did so, Venice Marc. Lat. Z 368, occupies an important place in the transmission. Its text of Florus, written in s. xiv<sup>med.</sup>, derives from O, but before the end of the century someone corrected it, someone who had direct or indirect access to Petrarch's manuscript of the complete text. Presumably its text of the *Periochae*, in two later hands (s. xiv<sup>2</sup>), came from the same exemplar as these corrections; and it was the second hand of the *Periochae* that added the letters of Caesar and the epigrams.

The notarial sign at the end, and doubtless the mostly erased *ex libris*, belongs to Luca Cantarelli of Reggio, who took his doctorate in canon law at Padua in March 1399 and died in the 1430s.<sup>87</sup> The hybrid text that resulted from correction reappears in two descendants, Vat. Lat. 7313 (membr. xiv/xv) and Venice Marc. Lat. X 71

<sup>85</sup> I owe this information to Albinia de la Mare. P. L. Schmidt, *Die Überlieferung von Ciceros Schrift 'De legibus' in Mittelalter und Renaissance* (Munich, 1974), pp. 305–6, assigns it to 1450–60 and central Italy, and Jeudy and Riou, op. cit. (n. 32), pp. 270–2, judge its origin to be 'bolonaise d'après la décoration'.

<sup>86</sup> Michael Winterbottom very kindly collated it at my request. For an illustration of the last page see the catalogue of the *Mostra di codici umanistici di biblioteche friulane* (Florence, 1978), plate II.

<sup>87</sup> Albinia de la Mare kindly inspected the manuscript for me. On Cantarelli see her remarks in *The Handwriting of Italian Humanists*, I.i (Oxford, 1973), p. 42 no. V; *Dizionario biografico degli Italiani*, 18 (1975), pp. 239–40; *Manuscripts classiques latins de la Bibliothèque Vaticane*, i (1975), p. 240 on Borg. Lat. 411. She would assign the first part of the manuscript, which contains Festus' *Breuiarium* and Florus, to s. xiv<sup>med.</sup> and probably Bologna, and the second part to s. xiv<sup>2</sup>. 'On the end (paper) flyleaf' she adds 'is a list of contents in a 15th-century hand not dissimilar to that of Francesco Barbaro'.

(3444) (chart. xiv/xv, Florus only). The former appears to be Florentine.<sup>88</sup> If it is a direct copy, as Cantarelli's acquaintance with Salutati suggests it could be, it must be an ancestor of Harl. 2765, which in the *Periochae* shares some of its errors.

That the corrector had independent access to Petrarch's manuscript emerges from 2.2.3, where in the margin after *defuit* he absurdly adds *et nec mole*. These words belong where N and the corrector of P put them, after 2.2.2 *nec mare*, for which *nec mole* was originally a variant (and correct at that). Apart from Vat. Lat. 7313, I have met them in only one other descendant of Petrarch's manuscript, to be mentioned presently. If Petrarch's manuscript was carried off to Pavia, therefore, someone in the Veneto must already have made a copy of it or at least used it for correcting Marc. Lat. Z 368.

### *Contaminated descendants of Petrarch's complete manuscript*

By 1426, when the Visconti library included a manuscript of Florus and the *Periochae*, Gasparino Barzizza had returned to Lombardy from Padua and taken with him an extant descendant of Petrarch's manuscript, Naples Naz. IV C 32. Even after correction from the family of B.L. Add. 25450 by either Gasparino or his son Guiniforte, it retains distinctive errors, e.g. 2 [*nec*] *pontibus*, [*bello*], 10 *tibicinas*, 33 *bello tandem*. These were inherited by five manuscripts from Lombardy, namely Leiden B.P.L. 19 (chart. xv<sup>med.</sup>, 'ductus ab exemplari domini Guiniforti Bergigii'), B.L. Harl. 5292 (membr. xv<sup>2/4</sup>), Ambros. E 122 sup. (chart. xv<sup>med.</sup>), Paris Lat. 5791 (membr. 1458, Milan), and Jal's V (Rome Vallicell. R 33, chart.), and by a sixth manuscript that betrays nothing to me, Paris Lat. 5793 (chart. xv). Jal pointed out that V bears a certain resemblance to P (I cl-cl), and indeed it is the best Italian witness to the complete text that editors have used; but any of its relatives would have served them better, because by 4.2 it has deserted to a different exemplar, one that associates it with the family of B.L. Add. 25450. Not even the date of V is in its favour. Both Malcovati and Jal assign it to s. xiv/xv, and neither comments on its origin; but the *Periochae* and two other texts in it were written at Piacenza and Milan by one hand in 1463–4, and though the hand of Florus looks earlier, the painted initial is uniform with those in the dated parts.<sup>89</sup> As I mentioned in my previous article (n. 31), V discloses its ancestry by attributing the *Periochae* to Gasparino Barzizza. The misapprehension was caused by the subscription of Naples Naz. IV C 32, which consisted of the single word *Explicit* until Gasparino added *epithoma in centum xl libros Titi Liuii mei Gasparini Pergamensis*. Ownership was then mistaken for authorship.

Like the Lombard descendants of Naples Naz. IV C 32, Beroaldus's edition, Parma c. 1476–8 (G.W. 10097), follows the complete text in word order and corrects some errors of the purer witnesses, but I have not noticed any agreements in error with the Lombard group. Commissioned by the printer and dedicated *ad magnificum comitem Petrum Mariam Rubeum Parmensem*, it must have been completed at Parma, but Beroaldus could have acquired his text at Bologna or Milan.<sup>90</sup> Three calligraphic manuscripts were copied from later versions of the edition, printed at Venice not

<sup>88</sup> I thank Albinia de la Mare for inspecting it. A note on fo. 73r, *Iste liber est mei Cardinalis S. Crucis quem emi ab heredibus olim domini Petri de Carbonibus de Rachaneto per manus domini Berardi eorum affinis. Idem d. Cardinalis manu propria*, follows a partly erased notarial record of the same transaction; Albinia de la Mare identifies the cardinal as Niccolò Albergati and reads the date in the erasure as 5 February 1430.

<sup>89</sup> Billanovich, op. cit. (n. 3), p. 100 n. 4, also regards the copies of Florus and the *Periochae* as contemporary.

<sup>90</sup> Cf. M. Gilmore in *Diz. Biog. degli Ital.* 9 (Rome, 1967), p. 382.

before 1487 together with Justin: Vat. Barb. Lat. 7 (written by Clemens Salernitanus) from the earliest of them (*B.M.C.* V 420), Escorial ç IV 16 probably from the same one,<sup>91</sup> and Naples Naz. IV C 52 from one of its successors (*B.M.C.* V 421, 421, VI 781, V 498).

In 2.1–2 two Florentine manuscripts, Laur. 68.29 (chart. xv<sup>med.</sup>) and Florence Naz. B.R. 36 (membr. xv<sup>3/4</sup>), also follow the complete text in word order without sharing all its errors. No doubt they drew ultimately on the same source as Edili 186 or S. Croce 20 sin. 11.

*Vat. Ottob. Lat. 2058*

A cornerstone of the later tradition, unless it has deceived me, is Vat. Ottob. Lat. 2058 (chart.), written at Bologna before 1450 in a clear and spacious hand that lent itself to transcription.<sup>92</sup> Erasures and variants, some of them added only to be cancelled later, are the visible result of what seems to have been a systematic and thoughtful conflation of sources. One source belonged to the family of B.L. Add. 25450, which passed on such errors as 2.2.10 *cuius quidem*, 4.2.38–9 *nunc lacessere*, 66 *signa sponte sua*, 72 *secuta[que]*, 85 *confectus*, 91 *collata*, but perhaps too some readings of the complete text, e.g. 4.2.19 *bellum sine sanguine*, 31 *utrumque circumuenit*, 55 *clarissimi*, 78 *et diu*, 81 *gradum retro*, 83 *praesidio*; I cannot actually prove that they travelled in this direction rather than the reverse, but I should be surprised if the Ottobonianus were earlier than B.L. Add. 25450. More interestingly, another source passed on some impossible and therefore uncommon readings of the complete text that the scribe understandably abandoned on reflection, e.g. 2.2.3 *defuit* <*nec mole*>, 22 *appropinquabatur*, 4.2.6 *annis*, 57 *Pompeio aedem* (or *caedem*), 79 *respectus*. My collations do not record any witness to the complete text as having all these readings, though Marc. Lat. Z 368 has all except 4.2.57 *Pompeio* (*c*)*aedem*.

Certainly what the scribe wrote in the margin after 4.1 fits Marc. Lat. Z 368 and its descendants: ‘Hic interponunt nonnulli capita duo quae sunt ex vii<sup>o</sup> Plinii de naturali historia in Caesaris et Pompeii laudem, sed huius auctoris non sunt; ideo ea omisi’.<sup>93</sup> Catalogues have reported this note from at least four manuscripts, all, I believe, wholly or partly descendants of the Ottobonianus: Fermo Com. 45 (chart. xv<sup>med.</sup>), Bodl. Canon. Class. Lat. 194 (membr. xv<sup>3/4</sup>, Ferrara?), Ravenna Class. 245 (membr. 1458?), and Vat. Reg. Lat. 924 (chart. xv<sup>med.</sup>, Bologna?).<sup>94</sup> It also occurs in Madrid Nac. 11245 (membr. 1457), in Modena Est. Lat. 937 (chart. xv<sup>med.</sup>), and in Trento Com. 3225 (chart. xv<sup>3/4</sup>; ‘Iohannes de Hassia in Hungaria et Italia multis annis uersatus scripsit’ for Iohannes Hinderbach), and another manuscript that may well have it (I did not collate 4.2) is Venice Marc. Lat. X 157 (3748) (chart. xv), which closely resembles Hinderbach’s manuscript and was written by a nasty hand that

<sup>91</sup> Whichever it was copied from, it is surely much too late to have been written by Hubertus W. (cf. n. 24). The script apparently imitates print, and like the decoration I should assign it to northern Europe.

<sup>92</sup> *Manuscripts classiques latins de la Bibliothèque Vaticane*, i (Paris, 1975), p. 776. It will become clear why I do not accept their date, ‘xv<sup>e</sup> s. (fin)’.

<sup>93</sup> The interpolation also occurs in Fermo Com. 81, from which the Marcianus may derive (cf. n. 27). It migrated to a descendant of Q, Vat. Lat. 1859, a very handsome manuscript written about 1400 and probably at Bologna.

<sup>94</sup> O. Pächt and J. J. G. Alexander, *Illuminated Manuscripts in the Bodleian Library, Oxford*, ii (Oxford, 1970), p. 41 no. 401, assign Canon. Class. Lat. 194 to s. xv<sup>3/4</sup> and Ferrara. According to *Manuscripts classiques*, ii.1 (1978), pp. 128–9, the watermarks of Vat. Reg. Lat. 924 point to Bologna. The first page of Fermo Com. 45 is illustrated by Prete, loc. cit. (n. 35). Michael Winterbottom very kindly collated Ravenna Class. 245 for me.

struck me as German; Rome Naz. Vitt. Em. 221 (chart. xv<sup>med.</sup>) may also have had it before the loss of 4.1.9–end. Eight of these nine manuscripts agree with the Ottobonianus in omitting 2.2.16 *Poenos*, which was also absent from the manuscript reported in Graevius's edition (Utrecht, 1680) when it belonged to Ryckius and in Duker's (Leiden <sup>1</sup>1722, <sup>2</sup>1744) when it belonged to Maasvicius. The ninth, Madrid Nac. 11245, restores it after *omnes*, and this transposition, together with some other variants of Madrid Nac. 11245, also occurs in Laur. 89 inf. 28 (chart. xv<sup>med.</sup>), B.L. Add. 26068 (chart. xv<sup>3/4</sup>), Lucca Gov. 1402 (chart.; 'in ludo litterario Politico finit per me Fr. Titum', a. 1469 or thereabouts) and its close relative Naples Naz. XIII G 11 (chart. xv<sup>med.</sup>), Bodl. Laud Lat. 58 (membr. 1453), Palermo Com. 2 Qq c 73 (membr. 1466, 'per me Iohannem Ericid. Coloniensis diocesis iuris utriusque scolarem Perusini studii'), Vat. Chig. I V 176 (chart. xv<sup>med.</sup>), and Venice Marc. Lat. Z 370 (1844) (membr. 1453),<sup>95</sup> and the transposition occurs in two manuscripts closely related to each other at least in 2.1–2, Madrid Nac. 8593 (second copy, chart. xv<sup>med.</sup>) and Rome Naz. Vitt. Eman. 11 (chart. xv<sup>med.</sup>). I have not collated all these manuscripts in 4.2, but many of those that I have collated behave so differently both from the Ottobonianus and from one another that changes of exemplar must be involved.<sup>96</sup> Unless, however, the rarities in the Ottobonianus have led me astray amid the complexities of the later tradition, 19 or more manuscripts derive at least in part from it, a fair reward for the work that the scribe put into it.

Who was he? At the end of the text, after the subscription *Anacephaleosis* ..., he wrote this eulogy of Florus:

Nemo uerius	} scripsit.
Nemo breuius	
Nemo ornatius	

It obviously has some connection with the last sentence of the biographical note on Florus, *Nemo melius nec ornatius nec expeditius nec purius nec defecatus nec breuius nec lautius hoc Anneo aliquid componere potuit*; but if it is a quotation, I do not know where it comes from. Not Jerome, *De uiris illustribus*, as a sixteenth-century note in Vat. Ottob. Lat. 1979 alleges; and the scribe himself did not compose it, because it appears in the original hand on fo. 73v of Vat. Pal. Lat. 895 (a. 1396).<sup>97</sup> Could the author be Petrarch, to whom Augusto Campana attributes the eight hexameters that follow in Pal. Lat. 895, *Aspice quam tenui* ...?<sup>98</sup> Be that as it may, Madrid Nac. 11245

<sup>95</sup> Marc. Lat. Z 370, written by Bessarion's doctor, should come from Bologna, where Bessarion spent 1450–5; cf. Lotte Labowsky, *Bessarion's Library and the Biblioteca Marciana* (Rome, 1979), pp. 187 A 237, 208 B 307, 460, and Concetta Bianca in *Scrittura biblioteche e stampa a Roma nel Quattrocento: atti del seminario 1–2 giugno 1979* (Vatican, 1980), pp. 137–8. Pächt and Alexander, op. cit. (n. 94), p. 34 no. 343, tentatively assign Laud Lat. 58 to Rome; cf. A. G. Watson, *Dated and Datable Manuscripts in Oxford Libraries* (Oxford, 1984), i.65–6 no. 583, ii plate 507.

<sup>96</sup> Several innovations in 4.2, e.g. 5 *sed auxilia*, 16 *peteret* <*consulatum*>, 23 *ipse pacem*, 52 *Septimi* <*Photini*>, 56 *odium* <*etiam*>, connect Add. 26068, Lucca Gov. 1402, Naples XIII G 11, Vitt. Eman. 11, and Madrid Nac. 8593, and B.L. Harl. 5438 (membr. xv<sup>3/4</sup>) also has them. Behind their text lies the second copy of Q and in particular a manuscript like Bologna Univ. 2476, to which they owe e.g. 14 *fortuna tanta imperii*, 30 *est fortuna*, 35 *Epiron Pompeius*, 75 *Oceani ostio*. L. Havas discusses Vitt. Eman. 11 and 221 in *Acta Classica Univ. Scient. Debrecen*. 25 (1989), 101–14.

<sup>97</sup> An elegiac couplet based on it, *Floro quid breuius* ...?, occurs in Laur. 89 inf. 28 and Bodl. Laud Lat. 58.

<sup>98</sup> See É. Pellegri, *I.M.U.* 19 (1976), p. 494, cited in *Manuscripts classiques*, ii.2 (Paris, 1982), p. 73, and M. Feo, *Giorn. Stor. Lett. Ital.* 152 (1975), 335–6, who reports that Campana announced the attribution at a conference in 1970 (I thank Michele Feo himself for this



has the same subscription and the same eulogy laid out in the same way, but after *scripsit* the scribe added *Lam* and something later erased. The uncurtailed form of *Lam* appears in the note after 4.1, 'Hic interponunt...', to which he added 'ait Lamola'. Giovanni Lamola came from Bologna and worked there between 1434 and his death in 1449. Did he write the Ottobonianus? Yes: 'it agrees in every way, including catchwords, with Vat. Lat. 3453, Gellius, signed by him in October 1432'.<sup>99</sup> His studies with Guarino at Verona and Ferrara, with Gasparino at Pavia, and with Filelfo at Florence, could have brought him into contact with several forms of the text, and his respect for old manuscripts is on record in the transmission of *De oratore* and Celsus. Those descendants of the Ottobonianus that have a clear enough origin all come from north-eastern Italy.

*The contaminated manuscripts: conclusions for editors*

Some witnesses have yielded to classification in only one of the two passages that I selected for collation, and a few others do not fit neatly into any of the patterns that I have distinguished: West Berlin Lat. 4<sup>o</sup> 436, Florence Ricc. 1206, B.L. Harl. 3694, Vat. Pal. Lat. 893, Urb. Lat. 462, and Vat. Lat. 3333, Wrocław I F 262, and the ed. Ven. c. 1471–2 (*G.W.* 10093).<sup>100</sup> There are also at least eight manuscripts that I have not seen, and though I have classified three of them, namely Budapest Nat. 167, Leeuwarden 54, and Pelplin Semin. Duchown. 2574, I know nothing about Brescia A VI 27, Eugene (Oregon) 16, Stuttgart Hist. 4<sup>o</sup> 152, Tuxedo Park Clearwater 5 (perhaps the *Periochae*), or Vienna 183.<sup>101</sup> So far, however, it appears that at most two people must have had access to Petrarch's manuscript without going through any of its nine pure descendants: the corrector of Marc. Lat. Z 368 and Lamola in Ottob. Lat. 2058. Picking out the collation made by the former and the readings incorporated by the latter would cost editors more trouble than the results would be worth, but I hope they will not omit either from their stemma. It is seldom that one has the luxury of deciding whether a lost manuscript left five, six, or seven lines of descendants.

As for the contaminated manuscripts used by Malcovati and Jal, namely SFayV, I have placed them all, and even though none of them can be shown to derive altogether from extant sources, as T and  $\beta$  can, they should all be banished from the apparatus.

reference). After repeating it at another conference in May 1991 ('Il Petrarca latino e le origini dell'umanesimo', held at Florence), Professor Campana kindly told me that he interprets the hexameters as a note of thanks added by Petrarch to a manuscript of Florus and the *Periochae* on returning it to its owner. If he is right, the manuscript in question can hardly have been any but what I have been calling Petrarch's manuscript of the complete text. Regrettably, I did not transcribe the verses but assumed instead, through a lazy reading of *Manuscripts classiques*, that they came from one of Petrarch's known works. I will return to the matter if the other things that have been competing since 1970 for Professor Campana's time prevent him from ever discussing it in print.

<sup>99</sup> I owe this information to Albinia de la Mare, who kindly inspected the Ottobonianus for me; she recognized the scribe but could not name him until mention of Lamola jogged her memory. On Lamola I have found nothing that supersedes or much augments the works of Sabbadini's cited by Sabbadini himself in *Encicl. ital.* 20 (Rome, 1933), p. 436, especially 'Cronologia documentata di Giovanni Lamola', *Propugnatore* n.s. 3 (1891), 417–36, and *Epistolario di Guarino Veronese*, iii (Venice, 1919), pp. 436–9.

<sup>100</sup> From the ed. Ven. derives Agira Com. 4, a sumptuous manuscript produced at Naples for Ferdinand I of Aragon; the mayor of Agira most kindly presented me with a copy of R. Patanè, *Agira* (Enna, 1989), of which plate 85 shows the frontispiece in colour. See also A. Daneu Lattanzi, *I manoscritti ed incunaboli miniati della Sicilia* (Palermo, 1984), pp. 53–5 nn. 22–3 and plates X–XI.

<sup>101</sup> Cf. also nn. 67, 75, 86, 94.

### *Titles and subscriptions*

At this point it may be useful to survey the medley of titles and subscriptions that appear in the Italian manuscripts of Florus, because no other evidence of relationships is more often recorded in catalogues, more apt to be illustrated in plates, or more easily found in the text. I need hardly say that it must be treated as provisional evidence. Titles and subscriptions were often left in vain for a rubricator, and in the course of time replacements might be borrowed from other manuscripts that came to hand. Moreover, subscriptions were often fashioned from titles and titles from subscriptions, and optional elements such as *incipit*, *libri quattuor*, or *liber primus*, created differences that may or may not be significant. Nevertheless, many titles are distinctive and persistent. I confine myself to descendants of e, whether pure or contaminated.

Q has a very simple title, *Lucii Annei Flori liber primus incipit*. Its descendants were often dissatisfied with it and devised fuller descriptions, of which one that includes *factorum gestorumque populi Romani ab urbe condita in mortem Octavianii Augusti* occurs as a title in Venice Marc. Lat. Z 367, Verona Capit. CCXXXIII (198), and Ambros. E 73 inf., and as a subscription in Vat. Pal. Lat. 895.

K and O have the title *Lucii Annei Flori incipit epithoma de Tito Liuii qui historiam Romanam ab urbe condita scripsit*, which closely resembles the one in H. I used its occurrence in Jal's  $\gamma$  as evidence of descent from K. It did not reach the Italian descendants of O, because the lost intermediary from which they all derive substituted one that included *de tota historia Titi Liuii epithoma*. This was borrowed by Vat. Lat. 1859, largely a descendant of Q, and by the ed. Paris. c. 1471.

The Lombard family represented by B.L. Add. 25450 refers to Florus in its title as *compendiosus historicus*.

The largely Florentine family represented by B.L. Harl. 2765 and Jal's F has a long title, *Lucii Annei (or Agnei) Flori continentie librorum quattuor factorum memorabilium ab urbe condita usque ad tempora Caesaris Augusti secundi imperatoris liber primus incipit*, followed by a summary of the first book that begins *Anacephaleosis septem regum*. Whether in the singular or the plural, the word *continentie* almost looks like a rendering of *periochae*, but so far as I know it is not found among the titles or subscriptions of the *Periochae*. The corrector of Venice Marc. Lat. Z 368 added a subscription that largely corresponds, *Anacephaleosis Lucii Annei Flori librorum quattuor factorum memorabilium ab urbe condita usque ad tempora Caesaris Augusti secundi imperatoris*, and in fact the Marcianus and Harl. 2765, though unrelated in Florus, are closely related in the *Periochae*, as I mentioned above. Did the corrector of the Marcianus even find the subscription in Petrarch's manuscript of the complete text? Be that as it may, its adoption by Vat. Ottob. Lat. 2058, whether from the Marcianus or independently from the same source, greatly increased its circulation.

Many titles refer to Florus as the father of Lucan and the brother of Seneca, but not always in the same words. The delusion was common, and Salutati even took the further step of identifying the author with Seneca.<sup>102</sup> More distinctive is the version that appears in Jal's  $\alpha$ , Escorial Z III 20, and Munich C.L.M. 11307, which includes *patris Lucani fratrisque Senece ac Gallionis*.

The distribution of these titles and subscriptions fits the relationships that I have inferred from variants in the body of the text.

<sup>102</sup> Novati, op. cit. (n. 20), iii (Rome, 1893), 298.17–18 with n. 3. See also G. Martellotti, *I.M.U.* 15 (1972), 164 = *Scritti petrarcheschi*, ed. M. Feo and S. Rizzo (Padua, 1983), pp. 377–8.

*Contamination with a manuscript independent of NP.1?*

So ends for the moment my analysis of the Italian tradition, and I can pass from Florus to the *Periochae* – or could but for one thing. Have we not been told that some Italian manuscripts of Florus drew on a relative of B, which has a very different text from NP.1? I gave my answer briefly in my previous article (pp. 480, 490), but a recent expression of the opposite view necessitates some amplification.

Malcovati and Jal say that V and F are sometimes alone in agreeing with B.<sup>103</sup> While Malcovati does not attempt to explain how this came about, Jal's stemma, in which no dotted line starts from the branch that has B on it, implies that the readings in question came down in direct line from the source of NP.1. My view of the tradition, and for that matter the rest of Jal's stemma, makes this quite impossible.

Now, however, L. Havas has invoked contamination, a harder theory to refute because one can never prove that no other source of readings was available. He has already changed his mind, however, about the other source: at first he supposed that it was B or a relative of B, but he increasingly prefers to suppose that it was a manuscript higher up on the same branch as NP.1.<sup>104</sup> The earlier form of the theory is indeed highly improbable, just as it was for e, because one would expect a collator to have noticed at least some of the far more striking differences between B and NP.1 that occur on almost every page; it is enough to mention the presence in B of 4.7.2–6 *tuebatur... belli*, absent from all the contaminated manuscripts as well as from NP.1. In either form, the theory seems to me once again, just as it was for e, entirely superfluous: allowance need only be made for conjecture and accident. To take Havas's 'sehr prägnantes Beispiel', the agreement of some Italian manuscripts with B at 4.9.4 could easily be accidental, because *disputant* differs from *dispulerant* by no more than the height of the ascender and the angle of the cross-stroke (*dispulant*).<sup>105</sup>

The sorry story of agreements between B and Italian manuscripts of s. xiv–xv goes back to two misjudgements of Malcovati's: her failure to think stemmatically about the tradition, and her assumption, taken over by Jal and Havas, that scribes always copy slavishly. I hope we have heard the last of it.

*The stemma of the Periochae*

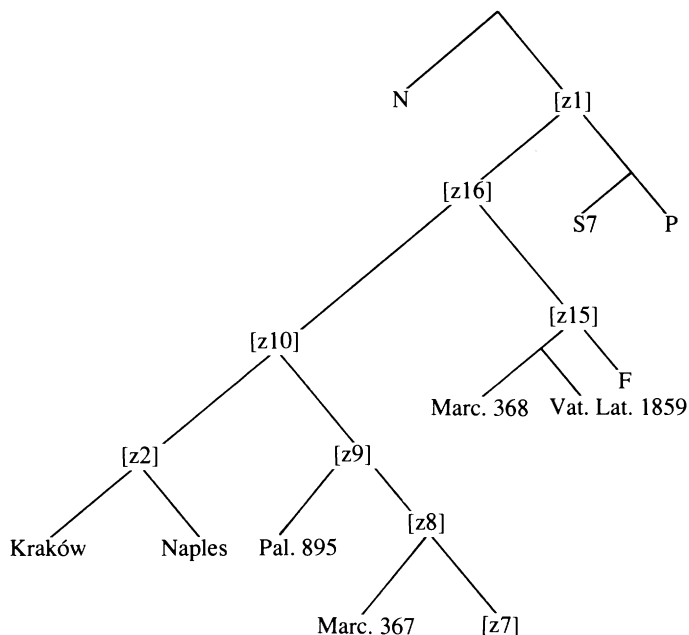
In discussing the Italian manuscripts of Florus related to P I have slipped into the habit of treating Petrarch's manuscript as their source. Reid's stemma for the *Periochae*, however, assigns so low a place to what must have been Petrarch's manuscript that it confronts me with a stern challenge. Time, therefore, to look at his stemma and the evidence for it. I will reduce it to what I hope are essentials (Fig. 1). With the exception of F (Leiden Gron. 107) all the extant manuscripts that I pick out include Florus, and with the further exception of S7 (Vat. Urb. Lat. 462, Florence, s. xv<sup>3/4</sup>) they have all cropped up already. The relentless bifurcation, which stops only at [z7], is Reid's and not the result of my simplification.

According to Reid, all the Italian manuscripts except those too contaminated to place either derive from [z16] or resemble S7; those that resemble S7 are three Laurentiani now familiar from Florus, namely Edili 186 and S. Croce 20 sin. 11 and 19 sin. 9, and also 89 inf. 25 (m. xv<sup>1/4</sup>), which contains only the *Periochae*. Petrarch's

<sup>103</sup> Malcovati, ed. pp. xvi–xvii and more fully in *Athenaeum* 25 (1937), 294–6 (she says the same about T and Ravenna Class. 245); Jal, ed. i. cxvii, cxviii.

<sup>104</sup> Op. cit. (n. 81), 93–4; *Athenaeum* 67 (1989), 34–5; op. cit. (n. 96), 111–14.

<sup>105</sup> The four examples that he gives in op. cit. (n. 96), 112–14 are a particularly unfortunate choice. In the third, 2.17.13 (*sic*, not 3), he misreports the reading of C.

Fig. 1. Reid's stemma for the *Periochae*.

manuscript, if one works upwards from Naples Naz. IV C 32 and Kraków 416, must have been [z10], unless one supposes that the reference to finding the *Periochae* in a *codex uetustissime litere* annotated by Petrarch had been taken over mechanically not just by Naples Naz. IV C 32 and Kraków 416 from [z2] but also by [z2] from [z10] and perhaps even by [z10] from [z16]. Inasmuch as the stemma gives P no weight against the agreement of the Italian manuscripts, it squares with my arguments so far; but besides assigning an uncomfortably low position to [z10], it associates S7 with P when I found no evidence in Florus for associating either Edili 186 or S. Croce 20 sin. 11 more closely than Naples Naz. IV C 32 with P.

Resolving these problems satisfactorily would require more collation than I can face in a text that consists largely of names and numbers; handbooks sometimes warn collators against the very real danger of coming to know their text too well, but a text that makes no impression on the memory can put them off altogether. Accordingly, I have confined myself to comparing Reid's lists of errors and collating *Per.* 22, 49, 55, 61, 84–5, 109–11, 116, 121, and 135, in a spread of manuscripts that allegedly derive from [z16], namely Naples Naz. IV C 32, Vat. Pal. 895, Venice Marc. Lat. Z 367 and 368, Vat. Lat. 1859, and B.L. Harl. 4905 (a relative of F), and in all the Florentine manuscripts that allegedly do not. I can say at once that I accept the existence of [z2], [z9], and [z15]; by diagnosing contamination in F and perhaps even in Vat. Lat. 1859 one could alter the stemma below [z15], but as Reid's stemma gives the rosiest picture of its accuracy and still defines it adequately, nothing would be gained.<sup>106</sup> On the other hand, I have grave doubts about the existence of [z10] and

<sup>106</sup> At 7.7 some descendants of [z15] read *liberauerat* with *Λ* (*uindicauerat* NP cett.). Without knowing them better I must suspend judgement, but I should expect the agreement to be coincidental in default of other evidence that the text of *Λ* reached Italy (except of course through e and k, which did not include anything of the *Periochae*).

[z16], and what he does with S7 collapses on inspection of the other Florentine manuscripts.

First, how tightly has he defined [z16]? As he does not list its errors, I set out to reconstruct them from errors common to [z10] and [z15]. His list for [z10] (pp. 287–94) is hedged about with qualifications, but it begins with 12 unqualified errors absent from P (to ignore a few that concern names). Most of them, however, are trivial, and ten occur in S7 (p. 492). This result augured badly for errors introduced by [z16], which were bound to be fewer. When I reached [z15] (pp. 406–12), I found that he cites only two errors of [z10] that it avoids: 49.18 *eius* for *ipsius*, 55.6 *audiuit* for *accidit*. The latter passage, however, baffled almost every scribe who met it:

consendenti deinde nauem ut in Hispaniam proficisceretur accidit ‘mane, Mancine’.

To *accidit* P<sup>2</sup> adds *uox* and several of the Florentine manuscripts *audiri*; some descendants of [z15] drop it and add *uox audita est* after the quotation, while Harl. 4905 reads *accessit uox* at the right point; and both [z2] and [z9] substitute *audiuit*, which if not conjectured twice could easily have been jotted in [z16] but ignored by [z15] or jotted in [z16] after the copying of [z15]. What errors he takes [z10] and [z15] to share I did not succeed in working out, but clearly the difference between [z10] and [z16], and between [z16] and [z1], is so slight that the stemma can easily be redrawn and shifting allegiances blamed if necessary on correction in [z1]. Certainly my own collations have thrown up no reason for not deriving [z2], [z9], and [z15], independently from [z1]. This section of the stemma, then, can be superimposed very easily on my stemma for Florus: [z1] was Petrarch’s manuscript, [z2] was a copy of it in both Florus and the *Periochae*, [z15] was a copy in the *Periochae* and either a collation or a copy in Florus, and [z9] was another copy in the *Periochae*, absent in Florus because Florus had already been copied from Q.

But does not [z16] announce its identity by omitting the subtitle of *Per.* 121, *qui editus post excessum Augusti dicitur*, which was the starting point of my previous article? I should be happy to think so, but unfortunately anything transmitted in a rubric, as the subtitle is in P and its relatives,<sup>107</sup> has a harder struggle for survival. To that extent I was wrong in my previous article to imply that the preservation of the subtitle or of the archetypal numeration (1–135, 138–42) should be a good pointer to the more valuable manuscripts. Important as the subtitle and the numeration are for the understanding of Livy, the loss of almost any reading from the body of the text would do more to associate manuscripts in a group and to confer value on those that have escaped it. I still need to be convinced, therefore, that [z16] ever existed.

Next, Reid’s argument for a close connection between S7 and P turns almost entirely on one passage, 61.6 (p. 489):

*Iipse, cum ad satisfaciendum senatui Romam profectus esset, Albam custodiendus datus est, quia contra pacem uidebatur ut in Galliam remitteretur.*

*Albam...est hic N[z16]: post remitteretur PS7*

Here he omits too many witnesses. Though S. Croce 20 sin. 11 and Laur. 89 inf. 25 agree with P and S7, Edili 186 agrees with N and [z16]. The same discrepancy recurs in three other manuscripts that he regards as an interestingly conservative group but does not include in his stemma: whereas B.L. Harl. 6510 and Manchester Rylands Lat. 48 agree with N and [z16], Florence Naz. B.R. 36 agrees with P. My own collations bring me to a different classification of these Florentine manuscripts

<sup>107</sup> On Harl. 6510, though, see my previous article (p. 481); N has it as part of the text. I thank John Briscoe for checking Rylands 48 here and at 61.6.

(Florentine they all quite certainly are).<sup>108</sup> Laur. 89 inf. 25 is closely related to S7, which has another relative unknown to Reid, West Berlin Lat. 4<sup>o</sup> 436 (membr. xv<sup>3/4</sup>); of the rest, the closest relatives are Edili 186 and Harl. 6510, which both read 22.1 *uenit in Etruriam*, 110.5 [*auxilia*]res, 135 *gens Alpine*;<sup>109</sup> Rylands 48 and B.R. 36 each separately share distinctive errors with Harl. 6510; and S. Croce 20 sin. 11 stands on its own. I do not pretend to know how many lines of descent led to them from Petrarch's manuscript, but it can hardly have been fewer than three. Again, the stemma can be superimposed very easily on my stemma for Florus: Edili 186 and S. Croce 20 sin. 11 are simply reinforced by at least one other descendant of Petrarch's manuscript.

That Petrarch's manuscript should have left so many lines of descendants at Florence may appear surprising, but it is not out of the question, for instance, that he lent it to Boccaccio and Boccaccio passed it round, or that it was the manuscript from which Domenico di Bandino, as I mentioned in my previous article (p. 483), claims to have supplied many people with copies of the text. It could even have come to rest in a Florentine library, though one might then have to reinstate [z16] so as to account for developments in northern Italy.

To round off this discussion of the *Periochae*, let me single out the conservative features that have struck me in the manuscripts that I have collated. 49.11 *Orfitiano* or *Orficiano* (NP<sup>2</sup>: *Africano* P<sup>1</sup> cett.) survives only in the text of Laur. 89 inf. 25, West Berlin Lat. 4<sup>o</sup> 436, and S7 (corrupted by S7 to *Orficinio*), and in the margin of Harl. 6510 and Rylands 48; the archetypal numeration (1–135, 138–42) only in [z15], in Edili 186 before correction, and in Harl. 6510;<sup>110</sup> the subtitle of *Per.* 121 only in the Florentine manuscripts (all except Laur. 89 inf. 25, West Berlin Lat. 4<sup>o</sup> 436, and S7); and the combination of Florus and the *Periochae*, without anything else, only in [z2], Edili 186, and S. Croce 20 sin. 11. The last point needs some expansion. Both in my previous article and in this one, I have set great store by manuscripts that contain only Florus and the *Periochae*, but many of them have let me down. The text of Florus in S7 and West Berlin Lat. 4<sup>o</sup> 436 derives largely from e; Pal. Lat. 895 owes its text of Florus largely if not entirely to Q; Marc. Lat. Z 367, which again owes its text of Florus largely if not entirely to Q, has the *Periochae* (a. 1421) in a different hand (or the same hand at a later date); and three manuscripts written well into s. xv, East Berlin Hamilt. 261, Fermo Com. 45, and Parma Palat. Parm. 2800, have either a contaminated text of Florus or one that derives from e. Conversely, S. Croce 20 sin. 11 and Kraków 416, which contain more than Florus and the *Periochae*, have the rest

<sup>108</sup> On Harl. 6510 and B.R. 36 see my previous article (p. 481); on S7, written by Francesco Contugi, de la Mare, op. cit. (n. 24), 494 no. 20 (6); and on Rylands 48 n. 69 above.

<sup>109</sup> These errors, compounded at 110.5 (*rostratae nauis* for *res rate*) and 135 (*gentes Alpinae*), recur in another group of manuscripts that does not appear in Reid's stemma, the well-defined group that includes the *editio princeps* (Rome, c. 1469). Its earliest member may be B.L. Burn. 202 (s. xv<sup>1</sup>, Milanese according to Albinia de la Mare), which incidentally, like some descendants of [z15], reads *liberauerat* at *Per.* 7.7 (cf. n. 106). In my previous article (p. 484) I derived another member, B.L. Harl. 3694 (1470s, Florence), from Campano's edition (Rome, c. 1470), which was certainly one of its sources; but from about the end of *Per.* 49 its main source was a manuscript like Laur. 89 inf. 25. On the scribe see above, n. 24.

<sup>110</sup> I simplify. After numbering 135 correctly, Kraków 416 has the numbers 136–7, 140, 141 altered to 151, and 152 (or 142 altered to 152); Edili 186 before correction gave the number 134 to 135 but then skipped the numbers 135–7 and so ended correctly with 138–42; and Marc. Lat. Z 368, which also gives the number 134 to 135, has *hic deficit* after it and again ends correctly with 138–42. The note after 135 that I reported from Valencia Univ. 482 in my previous article (n. 39), namely *Hic deficiunt duo libri nec repperi quenquam habentem*, also occurs in Florence Naz. Magl. XXIII 12 (chart. 1461, written at Florence by Iohannes Stagnensis), and like the Marcianus both derive from Reid's [z15].

in different hands. Billanovich has also pointed out that in 1423 Pileo de Marini, archbishop of Genoa, lent a *chartaceus* of Florus and the *Periochae* to Bartolomeo Capra, archbishop of Milan; but nothing is known about its text except that the opening words of Florus up to *Caesarem* were not written in black ink.<sup>111</sup> It therefore transpires that the combination of Florus and the *Periochae*, without anything else, may be ancestral in no more than four extant manuscripts: S. Croce 20 sin. 11, Edili 186, Naples Naz. IV C 32, and Kraków 416. On this point and the numeration, then, honours are even between northern Italy and Florence, but on *Orfitiano* and the subtitle Florence has no competition.

### *Conclusion*

The information presented here has confirmed the historical sketch that I ventured in my previous article. Such contamination as occurs in northern Europe before s. xiv does nothing to obscure the main lines of the tradition, and the grosser contamination that then takes hold in Italy and eventually in northern Europe conflates recognizable strains of the text: late and poor versions, amply represented already, of Florus alone, and a good version of the complete text, one represented in the *Periochae* with varying fidelity by all the Italian manuscripts but in Florus so far by only nine, two of which are *descripti*.

The upshot for editors of Florus is that the lost manuscript from which the whole tradition except B derives can be reconstructed adequately from N, P, a relative of P such as Ambros. F 138 sup. or S. Croce 20 sin. 11, and two good representatives of A such as U and Harl.; even NPU would probably suffice. In the *Periochae* the absence of B altogether and of A after 7.16 calls for a check on P where it disagrees with N, and to judge from the variants at 61.6 and elsewhere no single manuscript can adequately provide it. Collating eleven Italian manuscripts, namely Naples Naz. IV C 32, Vat. Pal. Lat. 895, Venice Marc. Lat. Z 368, and all the nine Florentine manuscripts that I have discussed except the *descriptus*, might reveal more about the transmission, but probably two of them would turn out to suffice for the apparatus, say Edili 186 and Laur. 89 inf. 25. Available evidence does not suggest that the standard of conjecture in manuscripts unimportant as witnesses is high enough in either Florus or the *Periochae* for collation to be let in again by the back door.

It was not just for the sake of Florus and the *Periochae*, however, that I wrote these two articles. Malcovati's view that there is too much contamination in the tradition of Florus for any stemma to be drawn up matches pronouncements from many quarters either on other traditions or on traditions in general. In some traditions, certainly, I should not like to have the task of producing a stemma; but all too often such pronouncements rest on nothing better than impatience with evidence, not least historical evidence. After devoting much of this second article to explaining how various forms of contamination came about, I hope I shall escape any charge of being blind to its pervasiveness. Unless I have misread the map in other ways, then, the tradition of Florus is one antistemma-tist bastion that has fallen to the stemmatists; and I see no reason why more should not suffer the same fate.

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<sup>111</sup> Op. cit. (n. 3), 99–100.