

## THE TRANSMISSION OF FLORUS' *EPITOMA DE TITO LIVIO* AND THE *PERIOCHAE*

When did Livy write his history? How many books had it, and what did the lost ones cover? Such answers as can be given to these questions come almost entirely from the one extant summary, the *Periochae*. The manuscripts of the *Periochae* disagree, however, on a matter of considerable interest: out of a hundred or so, only three, supported by a lost fourth, have been cited as adding to the title *Ex libro CXXI* the subtitle *qui editus post excessum Augusti dicitur*. When the latest editor, P. Jal in the *Collection Budé* (Paris, 1984), declares himself unconvinced of its authenticity (i.cxx–cxxi), he leaves the reader to decide whether authenticity means truth, authorial origin, or presence in the archetype; but whatever it means, seeds of suspicion have been sown.

If truth is at stake, the subtitle need only be transmitted somewhere. It can be true even if an interpolator added it, and its chance of being true will depend not on his stemmatic position but on his access to sources. If authorial origin is at stake, however, presence in the archetype matters. Though Jal includes the subtitle in his text, he does not adjudicate on the authority of the four manuscripts that attest it beyond mentioning that they are among the oldest. Is the textual tradition of the *Periochae* really so opaque? Let us see.<sup>1</sup>

In the Middle Ages and doubtless already in late Antiquity, the *Periochae* were transmitted quite separately from the extant decades of Livy, and no medieval library, with the possible exception of Lorsch, can be shown to have possessed both the *Periochae* and as much as a single decade. The *editio princeps* of Livy included the *Periochae*, however, and the practice so established itself that the first editor to publish them on their own was O. Jahn (Leipzig, 1853). O. Rossbach founded the Teubner edition (Leipzig, 1910) on the two oldest manuscripts and a collation of a lost manuscript but reported here and there, mainly at second hand, 13 other witnesses.<sup>2</sup> While A. H. McDonald was preparing his edition of Livy 31–40, of which he published 31–5 (Oxford, 1965), he supervised R. A. Reid's dissertation *The Manuscript Tradition of the Periochae of Livy* (Ph.D. Cambridge, 1969), which devotes 495 pages to a list of 76 manuscripts and a discussion of their relationships. Though very methodical, it is not easy to use: it has no table of contents or running titles, and the outlines of the argument do not stand out clearly enough, especially when he embarks on the *recentiores*. Jal despaired of profiting from it (i.ix n. 1), but his criticisms leave two things about his own procedure unexplained: why he collated or inspected 36 manuscripts rather than more or fewer,<sup>3</sup> and why he offers not even

<sup>1</sup> It was the problem of the subtitle that made me look at the transmission of the *Periochae*, and what alerted me to the problem was remarks of John Henderson's in a forthcoming article, of which he kindly lent me a draft. I thank John Briscoe and Leighton Reynolds for helpful comments.

<sup>2</sup> His article 'Die handschriftliche Ueberlieferung der *Periochae* des Livius', *Rhein. Mus.* 44 (1889), 65–103, gives a fuller account of the two oldest manuscripts but has almost nothing to say about others.

<sup>3</sup> Eight of them escaped Reid, but Reid correctly knew as Burn. 202 what Jal calls Burn. 204 (i.e. cxxii). Manuscripts unknown to both Reid and Jal are several of *Per.* 33, for which see below, n. 24; some other partial manuscripts, for which see below, n. 43; E. Berlin Ham. 261; Eugene (Oregon) Burgess 17; Ferrara Civ. II 191; Paris B.N. Lat. 6080; Parma Palat. Parm.

the rudiments of a stemma. These shortcomings are the more regrettable because the shrewdness and learning that have gone into his introduction and notes will inevitably make the edition seem to supersede Rossbach's.<sup>4</sup>

Missing above all both from Reid's dissertation and from Jal's section on the tradition (i.xcv–cxxi) is any interest in who wrote the manuscripts and where. Only Giuseppe Billanovich has seen that apart from the two oldest manuscripts, N (Heidelberg Pal. Lat. 894, s. ix<sup>1</sup>, probably written at Lorsch) and P (Paris B.N. Lat. 7701, s. xii, French), all the other complete manuscripts are Italian (or at any rate of Italian origin) and later than Petrarch's discovery of the text, which revealed to him that Livy wrote 142 books (*Fam.* 24.8.2, a. 1351) and enabled him to work out which he had in front of him.<sup>5</sup> Billanovich derives all these Italian manuscripts through an intermediary from P, but understandably enough, with only Rossbach's apparatus at his disposal, he cites no textual evidence. Briscoe too, albeit without deriving them specifically from P, has suggested that they have no independent authority.<sup>6</sup>

Are Billanovich and Briscoe right about the matter on which they agree? I cannot say without seeing N, P, and several Italian manuscripts, which I have no immediate prospect of doing; but I shall try to show that they need not be right for Jal to be wrong. The investigation will take me so far into the tradition that I shall carry on through the rest. First, then, I will set out in 15 sections, (a)–(o), what seem to me the main facts and questions about the textual evidence; after that, I will turn the stemma into a story; and in conclusion I will review the last century of editing.<sup>7</sup> At several points I shall be speculating about what extant manuscripts read or where they were written, which may seem a poor substitute for checking; but besides the practical excuse that I have just given, I have a methodological purpose, to illustrate the value of predictions for testing stemmatic theories.<sup>8</sup>

(a) A lost manuscript has caused more controversy than any that survives, but not

2800; Rome Angel. 1434 and Vallicell. R 33; Vicenza Bertol. G 2 8 15; and four manuscripts listed by L. Rubio, *Catálogo de los manuscritos clásicos latinos existentes en España* (Madrid, 1984), nos. 34, 523, 543, and 691, namely Burgo de Osma Arch. Capit. 19, Poblet Abadía 50–51–52, Salamanca Univ. 246, and Valencia Univ. 482. I have not checked the file on the *Periochae* at the Institut de Recherche et d'Histoire des Textes.

<sup>4</sup> I agree with almost everything that J. Briscoe says in his review, *Gnomon* 57 (1985), 419–24, about the strictly editorial part of the edition. Add that the apparatus cites the witnesses in alphabetical order, which has nothing to do with their authority or indeed with anything.

<sup>5</sup> *IMU* 2 (1959), 159; pp. 87–8 of 'Il Petrarca e gli storici latini', in *Tra latino e volgare: per Carlo Dionisotti* (Padua, 1974), i.67–145. Petrarch's connexion with the *Periochae* had been revealed by C. Iannelli, *Catalogus bibliothecae Latinae ueteris et classicae manuscriptae quae in regio Neapolitano museo Borbonico adseruatur* (Naples, 1827), pp. 95–7, and W. Wislocki, *Catalogus codicum manuscriptorum bibliothecae uniuersitatis Jagellonicae Cracouiensis* (Kraków, 1877–81), p. 137, and discussed by P. de Nolhac, *Pétrarque et l'humanisme* (Paris, 1892), p. 245; Rossbach, ed. of Florus (Leipzig, 1896), xxii; Sabbadini, 'Le "Periochae Livianae" del Petrarca possedute dai Barzizza', in *F. Petrarca e la Lombardia* (Milan, 1904), pp. 195–201; Nolhac ed. 2 (Paris, 1907), ii.36–7.

<sup>6</sup> Op. cit. (n. 4), 421.

<sup>7</sup> The *Periochae* fell between two stools in *Texts and Transmission*, ed. L. D. Reynolds (Oxford, 1983): no mention either under Livy, superficially the most obvious place, or under Florus, where they belong for a reason about to be given. Leighton Reynolds kindly tells me that he would have treated them under Livy if Jal's edition had not been on the point of appearing.

<sup>8</sup> Similarly but more fundamentally, A. Kleinlogel, 'Archetypus und Stemma: zur Problematik prognostisch-retroaktiver Methoden der Textkritik', *Berichte zur Wissenschaftsgeschichte* 2 (1979), 53–64, argues that to postulate a lost witness of a certain character is to predict what may come to light, and he gives examples of predicted witnesses that have indeed come to light.

because it attested *qui editus post excessum Augusti dicitur*. Sigonius' second edition of Livy (Venice, 1566) includes the *Periochae*, and in 1613 the younger P. Pithoeus collated on a copy now in the Bodleian (Auct. 2 R 1 5) a manuscript that resembled N but appended to the usual text the phrase *clades Quinctilii Vari*. Reid points out that someone had already written *clades Quintilii Vari* in the margin of the edition before he made the collation (pp. 134–6); but as he added it himself between the end of the text and the subscription (with *Quinct-* and with *Varii* altered to *Vari*), his manuscript appears to have included it. If the phrase were genuine, the manuscript would be worth all the rest put together; but it shows no other sign of being worth so much, and the phrase would extend the last book of Livy no less than 17 years beyond the death of Drusus. I have no doubt, therefore, that Reid and Jal are right to echo others and protest against Rossbach's espousal of the phrase. When it was added to the manuscript is an insoluble problem but an unimportant one. As Pithoeus' collation shows, the *Periochae* were preceded in the manuscript by Florus, who relates the *clades* immediately after the death of Drusus (2.30.29–39 = 4.12.29–39); and if that is not enough, Suetonius, who twice mentions it (*Aug.* 23, *Tib.* 17), was widely read in the Middle Ages even if Tacitus' discontinuous but fuller account was not (*Ann.* 1). Reid concludes, as Rossbach at first did,<sup>9</sup> that the manuscript was a descendant of N (pp. 123–40),<sup>10</sup> and I see no reason to dispute his verdict.

(b) Not only is N much the oldest of the complete manuscripts, but its colophon ends with an ancient rather than medieval turn of phrase: *LEGE FELICITER BONO TVO*.<sup>11</sup>

(c) All extant manuscripts but N omit the end of *Per.* 48, which cannot be supposed to have fallen out more than once by accident. It would need powerful evidence from elsewhere in the text to overturn the obvious implication, that N, even if it is not the archetype, has as much authority as the rest of the complete manuscripts put together (I reserve till later a group of manuscripts that stop well short of *Per.* 48). In fact evidence elsewhere points the same way as the end of *Per.* 48; at *Per.* 81.3, for instance, only N has *excursiones*, either omitted in other manuscripts or replaced by a stopgap such as *transitus, res* (with *in Macedonia*), or *gesta* (with *in Macedonia* and wrongly placed).<sup>12</sup> Now N has *qui editus post excessum Augusti dicitur*, and so its presence in any other manuscript whatsoever guarantees that the archetype had it, unless contamination can be proved responsible. To go no further, it is present in the next oldest manuscript, P. Whether it could have migrated by contamination remains to be seen, but it already begins to look as though Jal's doubts about its authenticity, if they pertain to its attestation, arose from counting manuscripts instead of weighing them.

(d) Not only in N and Pithoeus' manuscript but in P, in many of the Italian manuscripts, and in a group of incomplete though early manuscripts (about which

<sup>9</sup> Op. cit. (n. 2), 74.

<sup>10</sup> Jal, i.civ n. 2, says that Reid derives it from N through an intermediary, but what Reid derives from N through an intermediary is the collation.

<sup>11</sup> Bischoff, *Lorsch im Spiegel seiner Handschriften* (Munich, 1974), 65–6.

<sup>12</sup> Reid's dissertation is not designed to report what the manuscripts read in any particular passage, but I happened to notice that at *Per.* 81.3 *res* is a correction in Naples Naz. IV C 32, from which it passed to two copies (p. 164); my other information comes from Jal's apparatus. True readings peculiar to N can also be found at e.g. 40.3, 49.13, 68.3, 69.3, 80.3, 112.2, 123.1, 142.5.

more later), the *Periochae* follow Florus' *Epitoma de Tito Liui*.<sup>13</sup> They must have done so in the archetype. Furthermore, N, the incomplete manuscripts, and some of the Italian manuscripts, contain nothing else; about Pithoeus' manuscript there is no evidence. Older descriptions of P mention that it has two sections and in its later section contains another text before, and another after, Florus and the *Periochae*, but they fail to recognize that those other two texts were once separate. As Florus and the *Periochae* occupy eight quaternions and a bifolium, foll. 39–104, it is most unlikely that the scribe wrote anything else after the *Periochae*.<sup>14</sup> The archetype, then, will not have contained anything else.

(e) In Florus, a longer text as well, the manuscripts of the *Periochae* are not the only witnesses, and so their relationships can be clarified with external assistance. In principle, of course, the existence of other witnesses also creates the confusing possibility of contamination, but in this instance the main witness concerned, B (Bamberg Class. 31, s. x<sup>1</sup>; Chatelain II clxxxiii), has so distinctive a text that contamination would hardly escape notice; furthermore, though written in northern Italy, it is probably one of the many Italian manuscripts that Henry II presented to the cathedral library at Bamberg, so that it would have been at Bamberg for some 300 years when the text began to circulate again in Italy.<sup>15</sup> Before they edited the *Periochae*, both Rossbach and Jal had edited Florus (Teubner, Leipzig, 1896; Budé, Paris, 1967), but neither seems to have thought of asking the same questions about both texts and expecting the same answers.

(f) Is P independent of N? Yes, say Rossbach (pp. xiii–xiv) and Reid (pp. 112–22), and Jal in his edition of Florus (i.cxlx–cl) offers even stronger evidence for its independence there. Unless collation has been unusually negligent, the matter may be taken as closed.

(g) Are there errors common to all the Italian manuscripts? No-one has addressed the question directly, but all the manuscripts that Reid discusses in chapters III–VI (pp. 151–413) derive from his hyparchetype [z16] in the stemma (p. 412) shown in Fig. 1.<sup>16</sup>

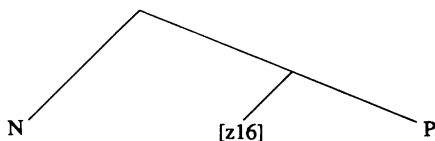


Fig. 1. The place in the stemma of Reid's hyparchetype [z16].

Those that he discusses in chapter VII (pp. 414–88) turn out to be too contaminated to place in the stemma, and he does not compare any of them with [z16]. Closer than

<sup>13</sup> The two branches of the tradition agree on this much of the title, but it cannot be Florus' own, because it misrepresents the work and the use of *de* is late. Many manuscripts give a similar title to the *Periochae*, and some even attribute them to Florus, with the result that catalogues do not always make it clear which of the two works a manuscript contains. One example: 'Florus, Abbreuiatio librorum Titi Liuii' in the description given by S. de Ricci and W. J. Wilson, *Census of Medieval and Renaissance Manuscripts in the United States and Canada* ii (New York, 1937), 1888, of a manuscript owned by William M. Clearwater of Tuxedo Park, New York (I do not know the present whereabouts of his collection).

<sup>14</sup> See B. Munk Olsen, *L'étude des auteurs classiques latins aux XI<sup>e</sup> et XII<sup>e</sup> siècles: Catalogues des manuscrits classiques latins copiés du IX<sup>e</sup> au XII<sup>e</sup> siècle* i (Paris, 1982), 387–8.

<sup>15</sup> Bischoff, pp. 175–7 of 'Italienische Handschriften des neunten bis elften Jahrhunderts in frühmittelalterlichen Bibliotheken ausserhalb Italiens', in *Il libro e il testo*, ed. C. Questa and R. Raffaelli (Urbino, 1984), 169–94; but he does not mention B.

<sup>16</sup> For the main arguments that lead to it see pp. 287–9, 292–4, 410–12.

the rest to P, he says, are five that he discusses in chapter VIII (pp. 489–95), of which he selects his S7 (Vat. Urb. Lat. 462, s. xv<sup>3/4</sup>, Florence) to illustrate their place in the stemma (p. 492) as shown in Fig. 2.<sup>17</sup> If he is right,<sup>18</sup> then Billanovich and Briscoe are

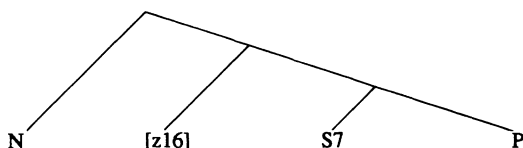


Fig. 2. The place in the stemma of Reid's S7.

wrong: none of the Italian manuscripts derive from P or N, and furthermore they do not all derive from the same source. Now as long as only one source appears in the stemma, the attestation of the subtitle *qui editus post excessum Augusti dicitur* poses no problem: since N and P have it, the archetype had it, and [z16], or whatever lost source represents the Italian manuscripts, merely omitted it. When two sources appear, however, and in such a pattern that their agreement outweighs P, P can only agree with N by accident or contamination; and if P has undergone contamination (accident being out of the question in this instance), the stemma could divide into a branch that presents the subtitle and a branch that does not. It therefore becomes important to know whether [z16] and S7 do actually agree in omitting it.

(h) Jal collated S7, but only 'par sondages' (i.cii). He does report, however, that the subtitle appears in Manchester Rylands 48. As Reid associates with this manuscript two others, Florence Naz. Magl. XXIII 19 (now B.R. 36; Florence, c. 1455–60)<sup>19</sup> and B.L. Harl. 6510 (pp. 414–21), I looked for the subtitle in the latter and duly found it, though as a marginal addition in the ink of the text rather than as part of the text or as a continuation of the rubric *Ex libro CXXI* (it also has an unwanted *est* after *editus*). For various reasons I suspect that in the exemplar it was a continuation of the rubric. How is it written in N and P? Despite this complication, however, Harl. 6510 must clearly be taken seriously. It was written by Sozomeno of Pistoia, probably in the early 1420s.<sup>20</sup> Reid does not place the group in his stemma but describes it as conservative.

(i) Where else should one look for the subtitle? The Italian manuscripts number over 80, and the only short cut in sifting them is to take first the earliest and those that like N and P contain only Florus and the *Periochae*. Neither Reid nor Jal makes much of an attempt to narrow down dates, but so far as I can gather from published descriptions the following manuscripts, and also Harl. 6510, seem likely to be among the earliest: Laur. Edili 186 (s. xiv<sup>2</sup>)<sup>21</sup> and S. Croce 20 sin. 11 (a. 1405), Vat. Pal. Lat. 895 (written by a *cuius Venetianus* in 1396) and Vat. Lat. 1859 (c. 1400), B.L. Harl.

<sup>17</sup> In fact he writes [z2][z9] in place of [z16], but according to his earlier stemma (p. 412) they derive from [z10], which together with [z15] derives from [z16]. Many of his lost hyparchetypes would surely disappear if the manuscripts were viewed with a more historical eye.

<sup>18</sup> His main arguments against descent from P can be found on pp. 166–70, 282–4, 408–9, 490–1.

<sup>19</sup> A. C. de la Mare in *Livy*, ed. T. A. Dorey (London, 1971), pp. 183 + 191 n. 31, and in A. Garzelli, *Miniatura fiorentina del Rinascimento 1440–1525: un primo censimento* i (Indici e cataloghi toscani 18, 1985), p. 530.

<sup>20</sup> De la Mare, *The Handwriting of Italian Humanists* i (part 1) (Oxford, 1973), 100, 103 no. 16.

<sup>21</sup> Ibid. 127 no. 31.

2765 (s. xiv/xv),<sup>22</sup> Venice Marc. Lat. Z 368 (s. xiv) and 367 (a. 1421), Naples Naz. IV C 32 (owned by Gasparino Barzizza, who died in 1431). Encouragingly, four of the nine contain only Florus and the *Periochae*: Edili 186, Naples IV C 32, Pal. Lat. 895, and Marc. Lat. Z 367. The only other manuscripts known to me that do so are Vat. Urb. Lat. 462, Parma Palat. Parm. 2800, and E. Berlin Ham. 261; the last was written at Rome, perhaps in 1463,<sup>23</sup> but the first, on which see (g) above, is Florentine and according to Reid closely resembles Edili 186 and S. Croce 20 sin. 11. Some of the nine occupy fairly high positions in Reid's stemma, but Jal reports only the two Vaticanani.

The Livian *capitula* ascribed to Donato degli Albanzani, which include *Per.* 33 in place of *capitula* for the absent book, may be earlier than any of these manuscripts, but there is no witness older than B.L. Burn. 198 (c. 1400) to the complete set.<sup>24</sup> Billanovich pointed out that much the same *capitula* appear in Paris Lat. 5690, which Petrarch bought at Avignon in 1351 but had had in his possession *diu ante*;<sup>25</sup> in fact they are a later addition, but they seem to have been added before Petrarch made his annotations. Be that as it may, they do not include *Per.* 33.

(j) Though still alive in 1411, when he made his last will, Donato degli Albanzani brings us within reach of Petrarch, whose library he used as early as 1367.<sup>26</sup> Where exactly does Petrarch fit into the tradition? In particular, what place in the stemma is occupied by the three manuscripts whose scribes claim to have found the epitome *in codice uetustissime litere manibus olim Petrarce lectam et postillatam*, namely Naples Naz. IV C 32, Kraków Jagiell. 416, and Escorial S III 21?<sup>27</sup> Rossbach points out in his edition of Florus (p. xxii n. 1) that Kraków 416 has the same subscription as P apart from an insignificant variation in the spelling of *Periochae*. Naples IV C 32 has a different subscription, but Reid associates it with Kraków 416 and derives their common exemplar, which he calls [z2], through an intermediary from his [z16], on which see (g) above.<sup>28</sup> The fate of Petrarch's manuscript itself is unknown. Much of his collection passed to the Visconti library at Pavia, which in 1426 did include a manuscript of Florus and the *Periochae*,<sup>29</sup> but part remained at Padua.<sup>30</sup> If Naples IV

<sup>22</sup> This manuscript puts after Florus and the *Periochae* three letters of Caesar, namely *Ad Att.* 9.7c, 13a, 16, and four poems, namely *Anth. Lat.* 709, 392–3, 660. Ambros. S 16 sup., described by Sabbadini, *SIFC* 11 (1903), 361–2, puts the same texts between Florus and the *Periochae*, and they recur in a manuscript of more diverse content, Besançon 840. As Sabbadini observes, 'l'uso di estrarre dalle *Epist. ad Att.* le lettere di Cesare rimonta al Petrarca'; he refers to Nohac, op. cit. (n. 5) ed. 1, pp. 249–50 = ed. 2, ii. 42, who also points out, ed. 1 p. 245 n. 5 = ed. 2, ii. 36 n. 5, that Venice Marc. Lat. Z 368 appends the letters of Caesar to the *Periochae*.

<sup>23</sup> H. Boese, *Die lateinischen Handschriften der Sammlung Hamilton zu Berlin* (Wiesbaden, 1966), p. 132.

<sup>24</sup> Other manuscripts that have *Per.* 33 include Burn. 200, Modena Est. Lat. 464 (aX 1 4), and Wrocław Rehd. 96. After Donato's death, Gasparino Barzizza from Padua tried hard to obtain his Livy, which was 'carior mihi... oculis meis'; see L. Bertalot, *Beiträge zur Forschung* N.F. 2 (1929), 81 = *Studien zum italienischen und deutschen Humanismus*, ed. P. O. Kristeller, ii (Rome, 1975), p. 95, no. 48, quoted by Billanovich, *IMU* 5 (1962), 133–4. It is not known whether he succeeded.

<sup>25</sup> *IMU* 2 (1959), 159 n. 1. For more about the manuscript see below, n. 53.

<sup>26</sup> Billanovich, *JWCI* 14 (1951), 174, *GSLI* 130 (1953), 323–4, 332–3; G. Martellotti, *Diz. Biog. degli Italiani* 1 (Rome, 1960), 611–13; A. Sottili, *IMU* 6 (1963), 189–90.

<sup>27</sup> See the scholars cited in n. 5 above, especially Sabbadini, who quotes the Petrarcan *postille*.

<sup>28</sup> Cf. n. 17.

<sup>29</sup> É. Pellegrin, *La bibliothèque des Visconti et des Sforza* (Paris, 1955), p. 103 A 141. She failed to identify the *Periochae* from the words that occurred in *principio ultimi capituli*.

<sup>30</sup> See e.g. Silvia Rizzo, *La tradizione manoscritta della Pro Cluentio di Cicerone* (Genoa, 1979), p. 33.

C 32 is a direct copy, it would be useful to know when and where it came into the possession of Gasparino Barzizza, who worked at Pavia before 1407, at Padua from 1407 to 1421, and then again at Milan and Pavia.<sup>31</sup> Reid's quite different placing of Esc. S III 21 in the tradition (pp. 460–7) suggests that its text and the note about Petrarch's *postille* derive from different sources, but it must still be explained how the note reached the scribe, Stephanus de Apardis de Barolo, who appears to have written the manuscript at Naples in 1456 or shortly before.<sup>32</sup> A similar problem is that Esc. O III 23, which Reid derives from Naples IV C 32 (pp. 177–94), also appears to be Neapolitan but not late enough to have been copied there from Naples IV C 32, which like other manuscripts of Barzizza's reached Naples from Milan in the library of Parrasio. The hand has a very Spanish look, and both the watermarks resemble Briquet 3547 (1467).

Two Florentine figures must also be fitted into the story. Domenico di Bandino claims to have given copies of the *Periochae* to many people; he died in 1418, but he had been busy for over 40 years on the work in which he makes the claim.<sup>33</sup> Boccaccio, who died in 1375, mentions in his life of Livy that the history ran to 142 books and ended with the death of Drusus from a broken leg;<sup>34</sup> moreover, he frequently cites the *Periochae* in glosses on his third translation of Valerius Maximus, which the latest study dates between 1342 and 1346.<sup>35</sup> His manuscripts passed at one remove to the *parva libraria* of S. Spirito, Florence, which in 1451 possessed two relevant manuscripts: one of the first decade whose penultimate leaf ended with *Per. 17.4 ducem feliciter*, and one of Florus and the *Periochae* that read *gentes Alpine* for *gens Alpina* in *Per. 135*.<sup>36</sup> Descendants of the former, presumably, are two manuscripts of the first decade that open with Boccaccio's life of Livy and include *Per. 1–21*: Oxford-New Coll. 277 (s. xv<sup>2/4</sup>, Florence) and Vat. Ottob. Lat. 2044. Jal reports *gentes Alpine* from Vat. Lat. 5258 and 6803, which so far as I know are neither Florentine nor as old as the 14th century; but I wonder whether it appears in Laur. Edili 186 or S. Croce 20 sin. 11. Even if it does not, I cannot help feeling that Edili 186 may be the most important of the Italian manuscripts. Not only is it commended

<sup>31</sup> See G. Martellotti, *Diz. Biogr. degli Italiani* 7 (1965), 34–9. Rome Vallicell. R 33, a composite manuscript in which the *Periochae* were written at Piacenza in 1463, actually attributes them to Barzizza; see P. O. Kristeller, *Iter Italicum* ii (London/Leiden, 1967), p. 133. As Reid missed it, I know nothing about its text.

<sup>32</sup> After the text he wrote topical notes up to 1462, of which the first concerns an earthquake that happened in *regno Sicilie et maxime in ciuitate Neapolis* on December 4th *inditionis quinte* while he was himself at Naples, the second the contingent sent from Buda on July 26th 1456 to join the papal forces against the Turks. The earthquake happened in 1456; see M. Baratta, *I terremoti d'Italia* (Turin, 1901), pp. 66–74 (I thank Charles Melville for this reference). It would be more convenient if he wrote the manuscript in the neighbourhood of Barolo, which is in Piedmont.

<sup>33</sup> Sabbadini, *Le scoperte dei codici latini e greci ne' secoli XIV e XV* ii (Florence, 1914), pp. 180–1; Teresa Hankey, *Diz. Biog. degli Italiani* 5 (1963), 707–9.

<sup>34</sup> Hearne printed the life from Oxford New Coll. 277 in his edition of Livy (Oxford, 1708), i b3v–b4r, and A. F. Massèra from Laur. 63.8 in *Giovanni Boccaccio: opere latine minori* (Bari, 1928), pp. 257–8 (cf. 369–70). See also Billanovich, *GSLI* 130 (1953), 328 n. 1.

<sup>35</sup> M. T. Casella, *Tra Boccaccio e Petrarca: i volgarizzamenti di Tito Livio e di Valerio Massimo* (Padua, 1982), pp. 278–86; oddly, the table on p. xiv gives a somewhat different date, c. 1341–2. She quotes or discusses citations on pp. 55–7, 63–4, 78, and already in *IMU* 6 (1963), 114–17, 124–5. On p. 64 she suggests a connexion between Boccaccio's manuscript and Vallicell. R 33, mentioned above (n. 31); but her three arguments are at best weak.

<sup>36</sup> Rossbach, ed. of Florus, p. xxii; Cornelia C. Coulter, *IMU* 3 (1960), 282–3; Antonia Mazza, *ibid.* 9 (1966), 47–8, 51.

by its date, its contents, and its doubtless Florentine origin, but like Kraków 416 it has the same subscription as P.

The involvement of Petrarch and Boccaccio suggests an alternative to Reid's conclusion that the Italian tradition is not a unity. In the tradition of Cicero's speech *Pro Cluentio*, the Casinese manuscript Laur. 51.10 (M, s. xi), which Boccaccio discovered and made available either directly or indirectly to Petrarch, produced two families, one of which consists largely of north-eastern manuscripts easily connected with Petrarch, the other largely of Florentine manuscripts that probably go back to Boccaccio.<sup>37</sup> Two speeches that commonly accompany *Pro Cluentio*, namely *Pro Quintio* and *Pro Flacco*, appear from nowhere but may show the same distribution.<sup>38</sup> Though the manuscripts listed above in (i) have not all been placed, it seems very likely that Naples IV C 32, the two Marciani, and Pal. Lat. 895, are north-eastern, the two Laurentiani Florentine. An early split in the Italian tradition, brought about by Petrarch and Boccaccio, could well have given it the appearance of deriving from two sources, especially if Petrarch turned his hand to emendation.

(k) Petrarch and Boccaccio both believed that Livy wrote 142 books. N and P number the books of the *Periochae* to 142, but the numeration omits 136–7. In the Italian manuscripts confusion reigns; Jal gives some details (i.cvx–cxviii) and says that only six of his manuscripts pass from 135 to 138, but something appears to have dropped out of his account (cxvi n. 1 'ces 6 manuscrits': which, and which pass from 135 to 138?). The numeration correctly runs to 142 in Harl. 2765 and 6510, on which see (h)–(i).<sup>39</sup>

(l) Before I proceed to the rest of the tradition, I will sum up my provisional conclusions about the subtitle *qui editus post excessum Augusti dicitur*. For its attestation to be doubtful, Reid's stemma would need to be right and the Italian manuscripts would need either to omit it in their entirety or to have acquired it through contamination. The Italian manuscripts do not omit it in their entirety; though the necessary work has not yet been done, I very much doubt whether those that have it acquired it through contamination; and I have suggested, speculatively I admit, an alternative to Reid's stemma.

(m) At the other end of the Italian tradition from most of the manuscripts discussed in (g)–(k), Billanovich has pointed out that Bussi's *editio princeps* (Rome, c. 1469) was set up from Vat. Lat. 6803.<sup>40</sup> Both Reid and Jal have failed to spot copies of editions: Vat. Urb. Lat. 392 even includes Bussi's preface; Laur. 63.9 and 33 would be running true to form if they were copied from editions, as Reid's classification leads me to believe they were (pp. 452–60);<sup>41</sup> and B.L. Harl. 3694 was copied from Campano's edition (Rome, c. 1470), which differs far more in the *Periochae* from the *editio princeps* than in the fourth decade of Livy.<sup>42</sup> To judge from these examples, much clarification of relationships within the Italian tradition remains to be carried out.

(n) Besides Italian manuscripts that present only a selection of the *Periochae* in

<sup>37</sup> Rizzo, op. cit. (n. 30), 23–47, especially 44–6.

<sup>38</sup> *RHT* 14–15 (1984–5), 55 and n. 8.

<sup>39</sup> Before I became interested in the *Periochae*, I examined Valencia Univ. 482 (c. 1480, Florence), which contains the fourth decade of Livy, Florus, and the *Periochae*. I happen to have noted that it has a gap after *Per.* 135 and in the margin *Hic deficiunt duo libri nec repperi quenquam habentem*.  
<sup>40</sup> *IMU* 25 (1982), 343.

<sup>41</sup> The scribe of 63.9 wrote 63.8 of the third decade, copied at least in part from the *editio princeps*; see *Rivista di Filologia* 115 (1987), 154 n. 1. On 63.33 see *Rivista di Filologia* 114 (1986), 167.

<sup>42</sup> *Rivista di Filologia* 114 (1986), 167.



company with a decade or more of Livy,<sup>43</sup> there is a group of manuscripts from northern Europe, mainly of s. xi–xii, that attach the *Periochae* to Florus but break off just before the end of *Per.* 7. Reid calls it *a* and Jal A, but I shall need a symbol for its source; as the only member that Rossbach reported was L (Leiden Voss. Lat. O 14, s. x/xi), I choose *A*. Rossbach (pp. xvi–xvii), Reid (pp. 111–12), and Jal (i.cvii–cix, without reasons), all regard *A* as independent of the complete manuscripts (independent, that is, even of their common source), but the evidence that they offer does not amount to much, especially when they admit that it makes drastic changes.

The text of Florus, where variants can be checked against the independent testimony of *B*, may seem to provide stronger evidence in their favour. Rossbach, for instance, cites several errors of *N* absent from *BL* (pp. xv–xvii), and Jal gives a long list of errors common to *NP* (i.cxlx n. 2). Not all of Rossbach's readings, however, recur in *P*, and Jal's list includes many that are errors only in comparison with conjectures or with the readings of *B*. Jal needs what Rossbach looked for, errors absent from *B/A*. His stemma as shown in Fig. 3 (opposite i.clx) should make them easy to find.

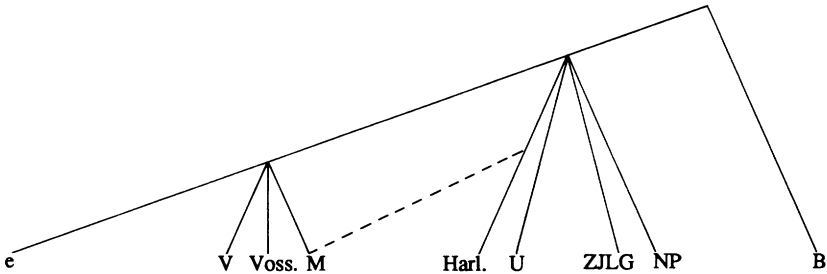


Fig. 3. Jal's stemma.

Voss.MHarl.ULG all derive from *A* inasmuch as they include *Per.* 1–7; *e*, the lost source of a large and corrupt family, did not include *Per.* 1–7. Where is *A* to be placed in the stemma? It can only be the highest manuscript on the left. In other words, Jal derives *NP* from it. That conflicts not only with his view of its relationship to *NP* in *Per.* 1–7 but also with his statement that *BNP* most often have the truth (i.cli) – a statement borne out by the very first page of his apparatus, which displays three true readings of *BNP* where the rest agree in error. The utmost in authority that can be granted to *A*, therefore, is as shown in Fig 4. Jal still needs to show that *A* does not derive from the source of *NP*.

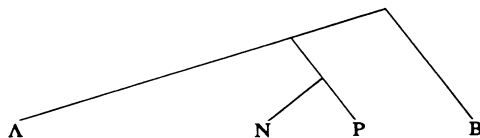


Fig. 4. Authority of *A*?

<sup>43</sup> Oxford New Coll. 277 and Vat. Ottob. Lat. 2044 (both mentioned above) include *Per.* 1–21, Vat. Chig. H VIII 254 *Per.* 11–20, Esc. g I 3 *Per.* 1–10, Modena Est. Lat. 385 *Per.* 22–30 (it has lost *Per.* 21); see also above, n. 24. Vat. Reg. Lat. 1847 (s. xv<sup>1</sup>), which breaks off in the middle of *Per.* 3, is a French manuscript of Florus and may belong to the group about to be discussed.

It is hard to reach a conclusion, because only Jal reports P and he does not include all aberrations in the apparatus. Moreover, his apparatus and Malcovati's (Rome, 1938) conflict in many places with Rossbach's, for instance at 2.22 = 4.12.4:

Noricis animos Alpes dabant, quasi in rupes et niues bellum non posset ascendere

dabant Alpes *L* dabant – rupes *om.* *N* quasi in rupes *om.* *L* quod *post* niues *add.* *L*

So Rossbach, whose negative apparatus implies that B reads what he puts in his text. Here is the text of Malcovati and Jal, followed by the essence of Jal's apparatus (Malcovati's tallies but does not report P):

Noricis animos dabant Alpes, quasi in rupes et niues bellum non posset ascendere

dabant Alpes *Δ* : Alpes dabant *B*: *om.* *NP* quasi in rupes *om.* *NPΔ* <in> niues *NP* <quod> bellum *Δ*

Rossbach, then, failed to report from N the addition of *in*; worse, NP omit not only *dabant*, as he says, but *Alpes* too. Now if NP did indeed omit *Alpes*, that would be a powerful reason for not deriving *Δ* from the source of NP; but the homoeoteleuton *Alpes...rupes* strongly suggests that Rossbach is right and that Malcovati and Jal failed to adjust their apparatus after reversing *Alpes dabant* in their text. If so, then NP have this:

Noricis animos Alpes et in niues bellum non posset ascendere.

Plainly what happened, after the accidental omission had robbed the sentence of a verb and a conjunction, is that *Δ* supplied a verb (the right one but in the wrong place), supplied a conjunction (the wrong one), and deleted *in* (which could have been saved if the conjunction had been substituted for *et*), with the following result:

Noricis animos dabant Alpes et niues quod bellum non posset ascendere.

That *dabant Alpes* gives a favoured clausula, as Malcovati observes, is neither here nor there in the circumstances, especially when nothing can be said against *Alpes dabant*. In this passage, then, a plausible choice can be made between conflicting reports, and ostensible evidence for the independence of *Δ* evaporates; but in general none of the editions is thorough enough or reliable enough to permit safe conclusions about the relationship of *Δ* to NP.<sup>44</sup>

So far *Δ* has been defined only by the inclusion of *Per.* 1–7, and it has yet to be shown that e, V, Z, and J, derive from it in spite of not including *Per.* 1–7. Z (Paris B.N. Lat. 17566, s. xiv) breaks off at 2.21.9 = 4.11.9 (i.cxxii–cxxiii), but unbeknown to Jal the end, followed by *Per.* 1–7, had already been found: Bern C 219.2.<sup>45</sup> Neither his stemma nor his apparatus provides the least reason for doubting that J derives from *Δ*. Before there can be any hope of deciding whether e and V derive from *Δ*, however, e itself must be satisfactorily defined. Jal delineates its family as shown in Fig. 5.

In various permutations FV $\alpha$  $\gamma$ S step out of line and join BNP, but Jal's wider stemma acknowledges contamination in FV, and his apparatus betrays it in  $\alpha$  $\gamma$ S.

<sup>44</sup> I have encountered the same problem in the *Periochae*. At 2.22, for instance, Jal's apparatus implies that P omits *pedibus*, and Reid agrees (pp. 111–12), but Reid also says that N omits *debilis*; Rossbach says nothing either in his article or in his apparatus. Incidentally, the unwanted *et* just before should be changed to *id*, not deleted.

<sup>45</sup> É. Pellegrin, *BEC* 103 (1942), 85–6, 98; it has an erased *ex libris* of the Collège de Navarre, Paris. By some muddle, probably unthinking subtraction, Jal cites Z in 14 places after 2.21.9.

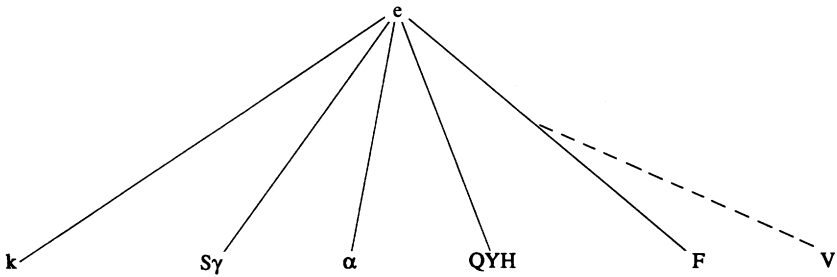


Fig. 5. Jal's stemma below e.

Clearly e must be reconstructed from uncontaminated manuscripts like HQYk. Though Jal's apparatus may permit such reconstruction, it would be so laborious that I shall not pursue the relationship of e to *Δ* any further here. My prediction is that it will prove to derive from *Δ* and perhaps even from Voss. (Leiden Voss. Lat. O 77, s. xi/xii).

Pending a proper analysis of *Δ*e and a full report of the manuscripts that turn out to matter, their relationship to NP must be left undecided.<sup>46</sup>

(o) There is an early fragment of the *Periochae* that does not derive from *Δ*, namely B (Bern A 92.10), a single leaf that presents a passage of *Per.* 6–8 (not to be confused with B of Florus). Rossbach regarded it as contemporary with N, which in his edition of Florus he dated to the middle or the latter half of the ninth century (p. xiii); Bischoff places both in the first half of the century.<sup>47</sup> Because B does not commit some small errors committed by N, Rossbach associated it with P (pp. xii–xiii) – a classic instance of a notorious fallacy. So far as the textual evidence goes, it could have been the source, when complete, of all the other manuscripts; Reid mentions five errors (p. 145), all trivial. Rossbach pointed out that a verb omitted by all the manuscripts in *Per.* 7 was probably omitted where a line ends in B, and though he did not infer that the omission first occurred in B, that assumption would be compatible with everything that anyone has so far revealed about B. The tradition of Censorinus furnishes an analogy. The view now generally accepted that all the other manuscripts derive from D (Cologne Dombibl. 166, s. viii) rests on nothing stronger than L. Urlichs' observation that some words missing from all the manuscripts could have been omitted at the ends of lines in D.<sup>48</sup>

It appears, then, that as the text of Florus and the *Periochae* spread across northern Europe from the ninth century to the twelfth it shed the *Periochae* in two stages: first everything from the end of *Per.* 7, then the rest. The first loss, the one seen in *Δ*, may seem unlikely to have come about through physical damage to a manuscript, because

<sup>46</sup> Jal omits Leiden Voss. Lat. O 5 from his stemma but says elsewhere that it is a twin of L (i.cli n. 5); Reid takes the same view in the *Periochae* but derives them both from G (pp. 73–97). Reid also derives M from Harl. (pp. 63–71). In his edition of the *Periochae*, i.xcvii, Jal describes yet another descendant of *Δ*, Paris B.N. Lat. 5794 (s. xv). Three more northern-European manuscripts that need to be brought into the picture, all of s. xii, are Cambridge Corpus Christi Coll. 313 (not a descendant of e), Leiden Voss. Lnt O 70A + W. Berlin Lat. Q 306 (fragments), and Pelplin Semin. Duchownego 2574; see Munk Olsen, op. cit. (n. 14), pp. 383–8.

<sup>47</sup> For his dating of N see op. cit. (n. 11), 96. He very kindly tells me that in his forthcoming catalogue of 9th-century manuscripts he will attribute B to 'Loire-Gegend (?)', ix. Jh., ca. 2. Viertel'.

<sup>48</sup> *Rhein. Mus.* 22 (1867), 472.

*A* ended at the end of a sentence (*Per.* 7.16 ... *patriae restituti sunt*); but then the scribe of *A* could have trimmed a ragged end. Even so, perhaps he made a deliberate choice. He is unlikely to have copied out *Per.* 1–7 as a guide to a tailless text of Livy 1–10, because he divided *Per.* 1 (or maintained the division) and thereby made seven of his eight *Periochae* correspond to the wrong books; by the same token, he could not have been supplementing a headless text of Livy 1–10, unless it had also lost every *incipit* and *explicit* in books 8–10. No: he may simply have given up because after Florus, who had something to say and an agreeable enough way of saying it, he found the *Periochae* unrewarding. That was plainly why the second loss occurred. It occurred independently in e and J, and physical damage would hardly have left Florus intact. Both losses were accompanied by other innovations, many of which show that the text was being read with attention and adjusted where it did not make sense. The two ancestral transpositions of long passages, for instance, were put more or less right, and someone familiar with Jerome's *Chronicle* concocted a biographical preamble on Florus and the Annaei.<sup>49</sup> Anyone capable of such innovations was quite capable of deciding what he did not want to read.

Not all the witnesses to these losses and innovations have been placed or firmly dated, but it seems likely that most activity occurred in northern France, almost certainly the home at least of e: among the descendants of e, H (Vatican Pal. Lat. 1568, s. xi<sup>2</sup>) has been placed there,<sup>50</sup> and one of the two manuscripts presented to Bec before 1164 by Philip of Harcourt, bishop of Bayeux, has been identified as Q (Paris B.N. Lat. 5802, s. xii) and assigned to Chartres, where Y (Paris B.N. Lat. 18104, s. xii/xiii) may also have been written.<sup>51</sup> U (Bern 249, s. xi) has a roughly contemporary *ex libris* of Bonneval, just south of Chartres, and Voss. (Leiden Voss. Lat. O 77, s. xi/xii) a later *ex libris* of La Chaise-Dieu, south of Clermont-Ferrand. Catalogues of the 11th and 12th century attest Florus at Corbie and Limoges as well as Chartres and Bec.<sup>52</sup>

P, though written in the 12th century (where exactly?), was a throwback to the ninth, and in the middle of the 14th century another throwback similar to P, or two if Reid is right, founded an Italian family that by the early 15th had grown to an impressive size, bringing the complete text of the *Periochae* to readers who knew three decades of Livy and giving it a wide circulation for the first time since Antiquity.

Meanwhile, Q and no doubt other descendants of e had made their way to Italy, and in many Italian manuscripts the two kinds of text have merged. FVγ, descendants of e that incorporate many readings from outside *Ae*, are Italian of s. xiv–xv, and αS will be too; there must be scores of Italian manuscripts like them. Had contamination

<sup>49</sup> For the text see Rossbach, p. xxviii, Malcovati, p. xxii, Jal i.cliv. I agree with Jal, i.clv, that it is not ancient. One descendant of *A*, B.L. Harl. 2620 (s. x/xi), has the last part of it, in the form *nec melius...hoc Anneo quisquam componere potuit*, alongside the passage on northern invaders (1.37.2 = 3.1.2, fol. 16v).

<sup>50</sup> *Les manuscrits classiques latins de la Bibliothèque Vaticane* ii (part 2) (Paris, 1982), 226–7. As H is the earliest witness to the preamble, its date matters more than usual; I have given the date adopted in the catalogue just mentioned, but Bischoff, op. cit. (n. 11), 96–7, had adopted s. x/xi. H contains Solinus, Florus, and a fragment of Orosius, and between about 1544 and 1553 Vinetus collated for editions of Solinus, Florus, and Eutropius, a manuscript at the Dominican library in Bordeaux that had the same subscription as H in Solinus and like H included the preamble to Florus. Could H not be the beginning of Vinetus's manuscript? He had no need to mention Orosius if he did not intend to edit him.

<sup>51</sup> On Y see T. Maslowski and R. H. Rouse, *IMU* 22 (1979), 97–122, and on Q the opinion of F. Avril cited there, 121 n. 2; further on Q, *Texts and Transmission*, ed. L. D. Reynolds (Oxford, 1983), pp. 76, 110–11, 160, 172, 402. Richard Rouse has kindly shown me a copy of a forthcoming article on Philip of Harcourt and his library.

<sup>52</sup> Manitius, pp. 145–6 of 'Handschriften antiker Autoren in mittelalterlichen Biblio-

already taken place in northern Europe, or was it someone in Italy who first set about it? Indeed, was it Petrarch? He used at least three manuscripts: one in which the *Periochae* followed Florus, namely the source of Naples Naz. IV C 32 and Kraków 416, and two in which they do not, namely Q and Paris B.N. Lat. 5690 (c. 1300).<sup>53</sup> Do some contaminated manuscripts, or even all of them, descend from Q? Jal's apparatus records several agreements of Q<sup>2</sup> with BNP, and it would hardly be rash to guess that Q<sup>2</sup> is Petrarch, importing variants from his manuscript of Florus and the *Periochae*. Where he obtained this manuscript, or how long before citing the *Periochae* in 1351 (*Fam.* 24.8.2), seems unlikely to emerge from extant manuscripts of the *Periochae*, but clues may lurk in Italian manuscripts of Florus written during his lifetime. The presence of contamination would show that a manuscript of Florus and the *Periochae* had surfaced, while its absence from a fair number of manuscripts would suggest that it had not.

The five descendants of Jal's k appear to be largely uncontaminated, but I do not know whether they are all Italian. Arras 902 (δ, s. xv) is described as Italian.<sup>54</sup> Paris B.N. Nouv. Acq. Lat. 3070 (K, s. xiv) was at least owned and annotated by Salutati.<sup>55</sup> Pavia Ald. 228 (T, s. xiv) is contaminated in an unusual way: Malcovati records in her apparatus that it rewrites the account of Caesar's assassination (2.13.91–5 = 4.2.91–5), but she does not point out that the new material came from *Per.* 116. Can its date be narrowed down? According to Malcovati (p. xx), it has a close relative in Vat. Lat. 1860 (a. 1313), but I do not know whether this manuscript too rewrites the account of Caesar's assassination or whether it is Italian. I also know nothing about Jal's other two descendants of k except that he assigns one to the 14th century and one to the 15th. Three manuscripts that I have examined, however, derive from k, and while one, Escorial N II 20, is French of about 1400,<sup>56</sup> the other two, B.L. Harl. 2657 (a. 1442) and the first of the two texts in Madrid Nac. 8593 (s. xv<sup>med.</sup>), are Italian. At some date, therefore, the text of k must have reached Italy from France or France from Italy.

It must also be asked whether any Italian manuscripts of the *Periochae* present an uncontaminated text of Florus. Neither Malcovati nor Jal reports any of what look

the *theskatalogen*', *Zentralblatt für Bibliothekswesen* Suppl. 67 (1935). In *Neues Archiv* 7 (1882), 533–4 and then in his *Geschichte der lateinischen Literatur des Mittelalters*, for instance i (Munich, 1911), p. 666, iii (1931), pp. 255, 389, 638 n. 1, he attempted to document use of Florus in various authors of the ninth and twelfth century, but he ignored the problem of indirect quotation; to take one example, variants show beyond doubt that the passage about the Gauls quoted from 'Eutropius libro tertio Romanae historiae' by Ralph of Diss, ed. W. Stubbs (London, 1876), i.25, came not straight from Florus 1.20.1–2 = 2.4.1–2, as Stubbs and Manitius say, but through Jordanes, *Rom.* 178 and Paulus Diaconus, *Hist. Rom.* (an expansion of Eutropius) 3.5. I have not seen E. Grunauer, *De fontibus historiae Frechulphi episcopi Lixouiensis* (1864), but Jordanes was certainly one source that Frechulph used.

<sup>53</sup> Nohac, op. cit. (n. 5), ed. 1, pp. 244–5 = ed. 2, ii.34–5. The precise date of Paris Lat. 5690 between c. 1290 and c. 1330 continues to arouse controversy, mainly because it includes three decades of Livy and if late enough may reflect work of Petrarch's on the text; see *Rivista di Filologia* 115 (1987), 420–30, and A. Conti, *Prospettiva* 44 (1986), 76–7.

<sup>54</sup> See the older series of the *Catalogue général des manuscrits des bibliothèques publiques des Départements* iv (Paris, 1872), p. 359.

<sup>55</sup> B. L. Ullman, *The Humanism of Coluccio Salutati* (Padua, 1963), pp. 196–7.

<sup>56</sup> In the lower margin of the second page (fol. 93v) it has the biographical preamble and at the end of the text (fol. 128v) the note *Lucius Anneus Florus senatui populoque Romano ... xviii annum agens*, reported from Laur. Fies. 181 by Bandini, *Bibliotheca Leopoldina Laurentiana* iii (Florence, 1793), p. 121; from Reims 1327 (Jal's β) in the *Catalogue général des manuscrits des bibliothèques publiques de France: Départements* xxxix (Paris, 1904), p. 474; and from Vat. Reg. Lat. 1564 and 1847 and Vat. Lat. 1860 in *Les manuscrits classiques latins de la Bibliothèque Vaticane* ii 1 (1978), pp. 288, 449.

like the most promising candidates, such as the manuscripts that contain only Florus and the *Periochae*. Naples IV C 32, for one, preserves the ancestral transpositions, thinly disguised, and at least in that respect has not undergone the influence of Q or any other descendant of e.<sup>57</sup>

A development common in Italian manuscripts is one that began centuries before in H and Q and at Limoges: what might be termed contamination of contents. Florus and the *Periochae* are put together with other texts, usually historical texts such as Justin or the *Breuiarium* of Festus, and on occasion enormous miscellanies are produced. At the same time, a number of Italian manuscripts contain only the *Periochae*, perhaps because whoever wrote or commissioned them already owned a copy of Florus in a descendant of e.

As for B of Florus, it breaks off in mid page just before the end, and there is no way of telling whether any of its ancestors continued with the *Periochae*; but it is a chip off quite another block, and it played no part in any of these events.

Rosbach's editions suffered from inaccuracy and a shortage of information, but he started from a good principle, that after N the other manuscripts had to prove their value.<sup>58</sup> Malcovati's edition of Florus initiated an unfortunate change. At Pavia, where she prepared it, there happened to be a 14th-century manuscript, and other Italian libraries yielded more manuscripts, none of them ever opened by an editor before. Four of them turned out, we are assured, to be alone in preserving some true readings, to agree occasionally with B against the rest of the tradition, or to give faithful renderings of proper names;<sup>59</sup> these four she collated throughout, and in her *conspectus siglorum* fifteen others of the 14th or 15th century join nine older manuscripts, of which she collated throughout only BNL. In short, a mongrel tail had begun to wag a pedigree dog. No wonder she did not offer a stemma. N. Terzaghi immediately pointed out that the presence of *Per.* 1–7 in three of her older manuscripts could hardly be coincidental, and L. W. Daly questioned the utility of reporting late manuscripts while earlier ones remained unexplored;<sup>60</sup> but I do not know that she ever responded to Daly's objection, and she made light of Terzaghi's.<sup>61</sup> Jal too disinterred some late manuscripts from local libraries, but he collated more of the older manuscripts and embarked on serious analysis, only to cut it short at the crucial point and draw up an impossible stemma and an overloaded apparatus to match it. In her review Malcovati objected to the stemma, but only for the most sweeping of reasons, that it made insufficient allowance for contamination.<sup>62</sup> Whether out of deference to Malcovati or not, Jal in his edition of the *Periochae* dispenses with

<sup>57</sup> Sabbadini, op. cit. (n. 5), p. 197, mentions only the first transposition, but both are reported from the closely related manuscript Kraków 416 by P. Passowicz, p. 387 of 'De Flori codice Cracouiensi', *Rozpr. Akad. Umiej., Wydz. Filol.* ii 12 (Kraków, 1898), 376–99.

<sup>58</sup> Reviewing Jal's edition of Florus, *CR* 83 (1969), 303, F. R. D. Goodyear remarked with his usual trenchancy that Rosbach's apparatus gave pretty well all the information necessary for constituting the text.

<sup>59</sup> *Athenaeum* 25 (1937), 294–6; more summarily in her edition, pp. xvi–xvii.

<sup>60</sup> *Athenaeum* 27 (1939), 152; *AJP* 62 (1941), 371–2.

<sup>61</sup> *Athenaeum* 28 (1940), 264; nothing relevant in her second edition (Rome, 1972) or in 'Floriana', *Athenaeum* 61 (1973), 141–5. I have not seen the edition of J. Giacone Deangeli (Turin, 1969), but from what I can gather it does not render my comments superfluous.

<sup>62</sup> *Gnomon* 42 (1970), 275. In fact Jal draws six dotted lines, and she does not explain why more are needed. The contamination mentioned by Axelsson, *Gnomon* 17 (1941), 272, is not what Jal's reference implies (i.clix n. 2), namely contamination in the extant manuscripts, but contamination of B and the other family in editorial choices, for instance the extraction of *repurgauit* from *purgauit* (B) and *repugnauit* (N).

a stemma, and that is why the subtitle of *Per.* 121 still needed to have its authenticity defended.

The medieval preamble to Florus concludes its genealogical investigation of the author with the judgement that *nemo melius nec ornatius nec expeditius nec purius nec defecatiis nec breuius nec lautius hoc Anneo aliquid componere potuit*. A genealogical investigation of the manuscripts would surely lead not only to a better understanding of how the text travelled but also to an apparatus *defaecatior* and *breuior* than those in the latest editions.

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