

The Edinburgh Companion to

# Sidonius Apollinaris

Edited by Gavin Kelly and Joop van Waarden



SIDONI VS. CONSTANTINO SVO SAUO:

**Q**ui precipis dñe maior summa suadendi au  
contate. sicut et in h. is que delibabuntur

consiliosissimus. ut si que luce paulo politiores  
uaria occasione affuerunt. putat causa. ob

## THE MANUSCRIPT TRADITION OF SIDONIUS

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AT THE CURRENT count, the manuscript tradition of Sidonius Apollinaris comprises just over a hundred witnesses. This makes Sidonius a well-diffused author, but the situation varies greatly from period to period. Setting aside florilegia and manuscripts containing only excerpts of Sidonius, there are seventy-seven manuscripts in total.<sup>1</sup> Just under fifty of them were copied in the twelfth or very early thirteenth centuries; about fifteen are earlier (three are Carolingian, three date from the second half of the eleventh century, and the others can be placed with more or less precision between these periods), and about fifteen later (almost all from the fifteenth century). The main problem is not the fact that the Sidonian tradition is rich, but rather that in the overwhelming majority of cases, it is impossible to use chronological arguments to classify the witnesses.

In addition, the quality of the transmitted text – or, to be more exact, of the copies produced – is, as far as I can judge, exceptionally good: the most evident mistakes are shared by numerous witnesses, something which often prevents the establishment of a precise classification. Is the quality of the transmitted text due to the particularly high regard in which Sidonius was held? I am inclined to think not; more likely, the notorious difficulty of the text required the scribes' constant attention: any word chosen by Sidonius is rarely what one would write 'by default'; it is often a related or similar but rare form which one must be careful not to garble. The consequence of this is that, short of collating all the manuscripts or at least the majority of them, the construction of the stemma is difficult and often precarious. However, this also means that in practice, the stemma can be used to reconstruct the text even if it is not correct in every detail.

## 1 History of Modern Editing and Stemmatic Scholarship

The other major problem faced by anyone studying the manuscript tradition of Sidonius is its historiography. I hope I will be forgiven for speaking of certain predecessors in rather blunt terms, and for reiterating basic elements of methodology. A sizeable bibliography rests on a very small number of works, which too often have been used to draw conclusions that are – to say the least – risky.

Sidonius has been the subject of three 'critical' editions. The death of Christian Lütjohann in 1884, at the age of thirty-seven, came before he had time to complete the edition he was

<sup>1</sup> A census of the known manuscripts of Sidonius Apollinaris with brief notes is provided in the second part of this chapter: item numbers preceded by a hash sign, such as #37, refer to this census. With a few exceptions, I have stopped short of analysing the manuscripts included in the second category, those that are fragmentary or contain only excerpts of Sidonius.

preparing for the *Monumenta Germaniae Historica*; it was eventually published under his name, but had been completed under Theodor Mommsen's supervision. Hence, although the text and critical apparatus are (directly or not) by Lütjohann, he had only completed the first part of the introduction, devoted to the description of the manuscripts, which are classified into four families. The construction of the stemma is the work of Friedrich Leo, and is the weakest part of the edition. It has already been noted that Leo's stemma directly opposed Lütjohann's classification into four families.<sup>2</sup> Yet Leo's arguments have perhaps not been sufficiently analysed: in the majority of cases, he is prompted by questions of spelling or perfectly reversible variants; that is, elements which can almost never constitute evidence. In Leo's defence, he had no knowledge (like generations of scholars after him) of the fragments of the Laudianus (#37. Oxford, Bodl. Libr., Laud. lat. 104, and now also Erlangen, UB, 2112/7) containing the *Carmina*, which were first noted by Bernhard Bischoff in 1976;<sup>3</sup> Leo thus took it for a manuscript of the *Epistulae* only. Though a minor element, it makes a considerable difference.

Returning to Lütjohann, his work is exceptional in many ways, and particularly the important list of witnesses, nearly all of which he consulted himself.<sup>4</sup> Nevertheless, his classification of the manuscripts into four families raises methodological issues: he admits from the first page that it is dependent on the order of the works and on the lacunae. The second factor is irrefutable, and the fourth family, the only one which is really based on this criterion, undeniably exists. However, the first point is not irrefutable in itself: before we can say that the order followed by a given manuscript is wrong or, in other words, is an innovation, we must be able to say what the authentic order is: as far as I am aware, this has not been generally established in the case of Sidonius' works.<sup>5</sup> Besides, within the first three families, Lütjohann tends to deal with manuscripts separately depending on whether or not they contain the poems as well as the letters. This is acceptable for descriptive ends, but not for a classification, because since an incomplete manuscript may very well derive from one which is more complete, a manuscript that only has the letters may well derive from one containing both the letters and the poems. My final criticism is for the most part a question of vocabulary. Lütjohann legitimately notes the resemblances between manuscripts (*gemellus est, affinis est, artissima affinitate conexus est, simillimus est*, etc.); he is generally right, yet he offers no indication as to the degree of such similarities. Thus, as we will see, certain manuscripts marked as 'twins' or 'very close' are indeed so, while others are actually separated by a more or less significant number of intersections in the stemma. Conversely, certain similarities which might have seemed evident remain undetected. For instance, the two manuscripts containing the eleventh-century poem *Deidamia Achilli*, which this fact alone would have sufficed to bring together, are left unsorted in their family (Lütjohann describes them at pp. ix and x–xi).

Since the subsequent editions by Paul Mohr (for Teubner in 1895) and André Loyen (for the Budé series, in 1960 for the poems and 1970 for the letters) reproduce certain errors found in Lütjohann's apparatus – which, it must be said, is sometimes unclear but rarely

<sup>2</sup> See e.g. Burke (1911) 7.

<sup>3</sup> Bischoff (1976) 19–20 = (1981) 184–5.

<sup>4</sup> The manuscripts not listed in Lütjohann's (1887) preface (and of which he was therefore not aware) are indicated in my census in the second part of this chapter by an asterisk. Lütjohann reached a total of 86 manuscripts (of which 64 belong to the first category of the census); for my part, I reach a total of 116 (with 77 in the first category): such a modest increase over a period of more than a century attests to the sheer breadth of Lütjohann's research.

<sup>5</sup> This criterion may indeed be used, but only when it is possible to decide if a given order is inherited or the result of innovation. This applies, for instance, to the manuscripts inverting *Ep.* 5.2–3 (discussed below), because in doing so they differ from the rest of their branch, the order of which is shown to be authentic because it is also that followed by the other branches.

mistaken – one may harbour doubts regarding their independence from their predecessor; admittedly, however, neither of them claimed to be starting again from scratch. From a philological point of view, there is much to be gleaned from Mohr's edition: he adds collations, notably for witnesses *N* and *V*, and reduces Lütjohann's apparatus by eliminating numerous minor variants (minor as far as establishing the text is concerned, though they are useful for assessing scribal practice), resulting in an edition that is sometimes easier to use. Turning to Loyen's edition: it is not my place to judge his text and translation; others will be more competent in this respect. However, his critical introduction displays a profound disdain for philology, and I find it difficult to trust an editor who quotes manuscripts using obsolete shelf marks,<sup>6</sup> gives a nonsensical description of his witnesses,<sup>7</sup> or does not hesitate to claim, in the space of fifteen pages, first that the manuscripts derive from several late antique editions, and then that they all have a single archetype.<sup>8</sup> Finally, the invention of the hyparchetype *m* is but a flimsy construct.<sup>9</sup>

Moving back in time, I should also mention two important contributions. Fridolf Gustafsson, though dealing primarily with issues of textual criticism, did so using important collations:<sup>10</sup> he was, after Lütjohann, the modern scholar with the most wide-ranging knowledge of Sidonius' manuscripts. Finally, the discovery of #56. Reims, BM, 413 is to be credited to Malcolm Burke, who wrote a doctoral thesis on the topic;<sup>11</sup> the readings from the manuscript quoted by Loyen in his edition were taken from Burke's collation.

The following pages do not endeavour to provide an exhaustive overview of the manuscript tradition of Sidonius Apollinaris – which would be unrealistic in the context of the present volume.<sup>12</sup> Rather, they are an attempt to open up potential avenues of research for future editors, and to indicate the principal manuscripts that they should use as well as the relationships between these. Thus I offer only the first steps towards an overview which, as things

<sup>6</sup> Loyen (1960) 1.xxxvi n. 1, quoting *C* as *Matritensis F 150*, and even noting: 'C'est par erreur que Luetjohann donne au Matritensis la cote Ee 102.' In fact, at the time when Lütjohann was writing, Ee. 102 was indeed the established shelf mark; the shelf mark *F 150* is older (it is used to identify the manuscript in Hänel (1830) 968, but does not appear anywhere in the manuscript itself). At the time when Loyen was writing, the current, numerical shelf mark (9448) had been in use for more than half a century.

<sup>7</sup> Loyen (1970) 2.li: '[*T* and *M*] ont, l'un et l'autre, ce point commun (qui les distingue de *L*): c'est de rapprocher, curieusement inséré [*si*] au milieu des *Epistulae*, les *cammina* I à VIII, les *Panegyriques*'; *T* has all the *Cammina*, not only the panegyrics, while *M* does indeed only have the panegyrics, but they are arranged according to the normal order, that is, following the letters. Loyen's description of the two states of the witness *M*, which follows the passage quoted here, is a serious distortion of elements taken from Lütjohann.

<sup>8</sup> The mere observation (correct, but probably underestimating the number of archetypal errors) at Loyen (1960) 1.xl: 'les . . . *codices* renfermant environ une quinzaine d'erreurs communes, il est nécessaire de leur supposer un même ancêtre *X*' undermines everything that Loyen has said from p. xxx onwards; see below on the archetype.

<sup>9</sup> This is the siglum used by Loyen to designate the readings shared by #29. Montpellier, BU Méd., H 4, #41. Paris, BNF, lat. 2171, and #44. Paris, BNF, lat. 2782 (Loyen (1960) 1.xxxix). Regrettably, Loyen did not realise that the collation of the third of these alone made it practically unnecessary to consult the other two, which are situated much lower on the stemma as I reconstruct it.

<sup>10</sup> Gustafsson (1882). Lütjohann did not have the chance to read it, but it is mentioned in Leo's preface.

<sup>11</sup> Burke (1911).

<sup>12</sup> I shall of course focus only on the works which are considered authentic, that is, the traditional corpus of letters and poems. Pithou's attribution to Sidonius of the epigram against Majorian edited in the *Anthologia latina* (391) looks credible enough, but it is not my place to judge (see Pithou (1590) 466, Sirmond (1652) 118; see in this volume van Waarden, ch. 1, sect. 2, point 8, with n. 33). The distich is known through two manuscripts, #101. Paris, BNF, lat. 8071 (the *Florilegium Thuanicum*) and #115. Vienna, ÖNB, 277, where it directly follows the Ovidian *Halieuticon*, which is itself preceded by the poem of Eucherius (*Anth.* 390). The relationship between these two manuscripts seems decisively settled by Russo (2019).



stand, is impossible to reconstruct in its entirety.<sup>13</sup> The research presented below is of varying degrees of precision: I have attempted to provide as precise a stemma as possible for the complete manuscripts (containing the *Epistulae*, the *Panegyrici*, and the *Carmina minora*), which are few in number; I have sought to clarify as much as I could the genealogy of the witnesses containing only the letters and the panegyrics. The manuscripts containing only the letters, which form the largest group, are classified with far less precision, for two main reasons: first, in the case of prose (and especially Sidonius' prose), it is more difficult, without complete collations, to distinguish an authentic text unambiguously from one that is the product of innovation; second, instances of contamination are notoriously more frequent.

I refer to the manuscripts using existing sigla when they have entered common usage, and failing that, abbreviations. The latter are only used for convenience within the framework of this chapter; it will be up to future editors to assign proper sigla to the surviving or reconstructed manuscripts which they use.<sup>14</sup> The sigla assigned to the archetypes are used right from the first stemmata in order to facilitate cross-referencing, but their logic will only emerge fully at the end of this chapter.

The stemmata offered here should be understood as an illustration of my text rather than its conclusion: on no account should what they present be used independently of the text which validates them. If their filiation is certain or very probable, witnesses are given in text boxes; if their position is less clear, they are left unframed: they are mentioned only to indicate the fact that their archetype is known, and that they do not descend from any clearly identified branches deriving from it. In general, the source of contaminations (indicated by a dotted line, or by underlining a manuscript with a dotted line) is not provided; in most cases, this is difficult to determine with any certainty, but when it can be identified, this is noted in the text. Quotations are from Lütjohann's text and follow his numeration.

## 2 The Archetype

I should begin with a few preliminary remarks on the origins of the manuscripts: although anticipating some of the arguments made below, this will make the chapter as a whole more readily comprehensible. There is no evidence to suggest that Sidonius' works derive from multiple archetypes:<sup>15</sup> the need for correction through conjecture occurs regularly in the editions, always in places which implicitly reveal faults shared by all the manuscripts and therefore inherited from a single archetype; conversely, there is never any reason to suspect that variants

<sup>13</sup> For reasons of space, I shall not systematically demonstrate the independence of a given manuscript from another. Two manuscripts, say *A* and *B*, can be connected in three ways: either *A* descends from *B*, *B* descends from *A*, or both descend from the same manuscript; I shall not seek to demonstrate the last case, which is by far the most frequent in practice. However, when one of the other two cases occurs or may legitimately be suspected, I shall provide a justification.

<sup>14</sup> When dealing with contaminated manuscripts with identifiable sources, I shall distinguish their initial state (before contamination) from their final state: *M*<sub>1</sub>, *Avr*<sub>1</sub>, or *M*<sub>2</sub>, *Avr*<sub>2</sub>, etc. If no indication is given, I am referring to the final state. Additionally, in the case of #63. Vatican City, Bibl. Vat., Reg. lat. 203, I make a strict distinction between the two sections containing the letters and the poems respectively (which I thus treat as two different manuscripts, *Reg203/1* and *Reg203/2*), because I am not completely certain that they originally formed one manuscript.

<sup>15</sup> I use the term 'archetype' to refer to the first common ancestor (surviving or not) of a given group of manuscripts, and in absolute terms, of all the manuscripts. In the latter sense, when 'archetype' on its own could be ambiguous, I use the German term *Ur-Archetyp*.

in important passages go back to the author himself.<sup>16</sup> The most important consequence of this is that although it is very probable that several editions of parts or all of Sidonius' oeuvre were published within his lifetime, the tradition never bears any direct trace of them; therefore it cannot provide arguments justifying the existence or the nature of these editions, which are irretrievably lost to us.

Certain surviving manuscripts contain both the letters and the poems, and others only the letters; those containing only the poems (which are very few in number) are late and all derive from more complete manuscripts – in other words, they are the result of an editorial choice to omit the letters.<sup>17</sup> Consequently, there is no reason to think that the letters and the poems each have a distinct tradition and that the presence of both corpora in some manuscripts is the result of editorial rather than authorial choice.<sup>18</sup>

It is difficult to provide any precise information on the archetype. Leo thought it was *non vetustissimus*, but without very strong evidence.<sup>19</sup> The oldest surviving manuscripts are witnesses dating to the first half of the ninth century.<sup>20</sup> Since they are – assuming my final stemma

<sup>16</sup> The argument seems certain from an empirical perspective; however, in points of detail it can admittedly be hard to distinguish Sidonius' style from the prose of medieval scribes or scholars. In such cases, the stemma plays a determining role: see, for instance, n. 39 below.

<sup>17</sup> See below on humanist witnesses; they all descend either from #13. Florence, BML, plut. 45.23 (*T*), or from #6. Brussels, BR, 10615–729 (*Br2*). The latter, copied in Trier around 1150, is a genuine exception, but its aim clearly was to compile an anthology of poetry only: it omits Sidonius' prose, but does the same for Paulinus of Nola.

<sup>18</sup> This remark is valid in general; it does not apply to certain special cases, including the English family and the archetype which I call  $\kappa$ .

<sup>19</sup> Leo's *Ur-Archetype* is what he calls  $\alpha$ ; he dates it approximately to the eighth century: see Lütjohann (1887) xxv–xxvii. I cannot understand how Leo (and Mommsen, who is the author of the note on xxv) are able to attribute to the archetype a number of lacunae which do not in fact exist in all the manuscripts: the lost manuscript which they describe is only a hyparchetype.

<sup>20</sup> #37. Oxford, Bodl. Libr., Laud. lat. 104 + Erlangen, UB, 2112/7 (*L*), reign of Louis the Pious; #56. Reims, BM, 413, second quarter of the ninth century; #89. Marburg, Hessisches Staatsarchiv, Hr 4, 15, same dating. To be absolutely exact, the oldest surviving manuscript appears to be #105. St Gallen, Stiftsbibl., 190, dated by Bernhard Bischoff (albeit eighty years ago, at the very beginning of his career) to the early ninth century; see Bischoff (1998–2014) 3.313, no. 5648 (cf. Bischoff (1937) 695). If only because of its date, this manuscript, deserves to be treated as an exception to my rule of not discussing fragmentary or partial witnesses. As it stands, the volume is a very complicated assemblage. The greater part of the manuscript is comprised of the letter collections of Faustus of Riez (and the letters linked to that collection), of Ruricius of Limoges, and of Desiderius of Cahors, but there are also various texts with no obvious relation to this epistolary series: for example, the *Libellus precum* by Faustinus and Marcellinus (= *Collectio Avellana* 2), the *De scripturis divinis* by Evantius (*CPL* 1076), fragments of two letters by Augustine, and two letters which are thought to come from the pen of Bachiarius (*CPL* 570). Furthermore, at the beginning of the manuscript, there is a summary list of (some of) the contents (written in at least two separate stages, and evidently imperfect). Although this list provides much relevant information, there is no obvious way of reconstructing the story of this fascinating manuscript. The two studies by Mathisen (1998, 1999b) have seductive conclusions, but, despite being accepted in subsequent bibliographies, are based on a sequence of hypotheses. To date, moreover, no analysis has taken various highly significant technical elements into account, including composition of gatherings and hints of Luxeuil script, for example on pp. 277 and 300. Two elements connect this manuscript to the transmission of Sidonius: a list of letters selected from Books 3–9 (in the summary of the volume only), and *Ep.* 2.1, which has lost its opening words (until *bella dixerunt*, in §1) and is presented as if it were the conclusion to Faustus' *Ep.* 11 (pp. 130–2). The list, probably incomplete, refers to an earlier stage in the history of the collection represented by the manuscript, when it included an anthology of Sidonius' letters, probably also with letters from Books 1 and 2: for similar florilegia, see #96, 99, 103 in the census of manuscripts below (although they are later in date, such selections are common enough throughout the Middle Ages). The remains of *Ep.* 2.1 may be the only concrete remnant of that earlier stage. Unfortunately, no conclusions can be drawn from an analysis of the text: although it contains various mistakes, errors, and variants, it cannot be linked precisely with

is correct – at least four generations lower than the archetype, it seems highly unlikely that the latter could be dated later than the beginning of the eighth century; it would probably be safer to place it at some point in the seventh century, though this is mere hypothesis.

We can be more confident – even certain – that the archetype contained all of Sidonius' known works:<sup>21</sup> letters, panegyrics, then *Carmina minora*, in that order, which is manifestly original. However, the available stemmatic arguments are insufficient to establish how these works were arranged in detail, especially in Book 7 of the letters; neither do they allow us to confirm whether *Carmen* 24 was indeed at the end of the collection, or if it was followed by *Carmina* 22 and 23.<sup>22</sup> Besides, even if it was possible to reconstruct the archetype in its exact state, it would not necessarily mean that it corresponded to the author's intentions.

There is at least one feature which surely did not form part of the original collection: the inclusion, probably immediately after Sidonius' works, of Ausonius' *Caesares* in a very abridged version, and, moreover, in a pretty bad state.<sup>23</sup> In the Middle Ages, the *Caesares* was probably the most popular work of Ausonius – perhaps his only work known beyond narrow circles. Not only was it transmitted in the principal collections of Ausonius' works, but it was also transmitted independently in anthologies, in the manuscripts of Sidonius, and in those of Suetonius.<sup>24</sup> This is of some significance for our purposes: since the transmission of the *Caesares* in the collections of Ausonius undoubtedly offers the 'authentic' – or at any rate the least damaged – text, it is possible to distinguish systematically between elements derived from the tradition and those derived from innovations in the 'non-Ausonian' tradition. We will see that in certain cases, the *Caesares* provides the most immediately clear arguments, though not the only ones, for classifying the manuscripts of Sidonius.<sup>25</sup>

any known branch of the transmission of Sidonius. This does not mean, however, that the manuscript is 'extra-stemmatic'. Quite apart from the implausibility of an otherwise unknown branch surviving until the beginning of the ninth century merely to end with this sole manuscript, *Ep.* 2.1 may share at least one corruption common to the whole tradition, *quique* for *quippe* on line 4 (but Lütjohann's emendation may be unnecessary); moreover, the order of the letters in the summary clearly indicates a selection made from Sidonius' letter collection in the state and order that we know. Additionally, although it carries less weight as an argument, it should be noted that nowhere in the manuscript is there a variant that might be authentic against all other witnesses. *Citius* for *citius*, §4, might be the truth, but even if that is the case the variant has no value stemmatically. See also Mathisen in this volume ch. 20, sect. 3, with a somewhat different outcome.

Another partial witness worth citing is #111. Vatican City, Bibl. Vat., Vat. lat. 1341 (see Williams (1967), brought to my attention by Joop van Waarden), which contains *Ep.* 4.25 from the beginning up to 4 *augeri* (the ending is regular but nonsensical: the scribe copied just enough of the letter to fill the blank space available). Some variants are noted above the line, all but one absolutely stupid (e.g. *imperitiam* for *impertiam* in l. 1; or are they the text of the model that the scribe corrected?): at 3 *iunctis repente manibus*, the MS has *iunctis* with *L* (Mohr (1895)'s apparatus evokes the possibility that *N* has it too; I checked *N*, and it clearly has *iactis*), and *iactis*, which is the text of all other manuscripts, as a variant. However, I cannot help seeing *iunctis* as a banalisation, which could actually have occurred more than once.

<sup>21</sup> With the reservations expressed at n.12 above.

<sup>22</sup> See Franca Ela Consolino's ch. 10 in this volume, n. 7.

<sup>23</sup> The *Caesares* occurs in eighteen manuscripts of Sidonius, covering virtually all the branches.

<sup>24</sup> The *Caesares* appears in the family which includes the famous Vossianus, Leiden, UB, VLF 111, as well as in the *Z* family, which has mostly humanist attestations, and in some manuscripts of the so-called *Excerpta*. The 'anthological', 'Sidonian', and 'Suetonian' *Caesares* all share a single source, which corresponds more or less to the archetype  $\chi$  of Green's edition.

<sup>25</sup> I limit myself here to the bare minimum, but I hope to publish a complete study of the tradition of the *Caesares* in the near future.

### 3 Lütjohann's 'Fourth Family'

The existence of Lütjohann's so-called 'fourth family' cannot be doubted. Its most visible characteristics are a lacuna from *Ep.* 6.12 to 7.5, the omission of *Ep.* 8.2, the reduction of Book 9 to letters 2–6, and the loss of paragraphs 7–8 of *Ep.* 3.3 and of the final words of *Ep.* 7.18. Occasionally, one or another of these features may have been corrected by a given witness using a manuscript other than its direct model; yet the first of them can almost always be easily identified. Here is a list of the twenty-nine relevant witnesses:

- #9. Copenhagen, Kongelige Bibl., GKS 30 2<sup>o</sup> (*Gks*)
- #11. Douai, BM, 291 (*Douai*)
- #13. Florence, BML, plut. 45. 23 (*T*)
- #15. Florence, BML, plut. 90 *sup.* 8 (*Pl90*)
- #16. Florence, BML, S. Marco 554 (*M<sub>i</sub>*)
- #27. Milan, Bibl. Ambr., F 131 *sup.* [second part] (*F131sup/2*)
- #28. Milan, Bibl. Ambr., L 97 *sup.* (*L97sup*)
- #30. Montpellier, BU Méd., H 445 (*Mtp445*)
- #31. Montpellier, BU Méd., H 541 (*Mtp541*)
- #32. Munich, BSB, lat. 70 (*Clm70*)
- #33. Munich, BSB, lat. 24508 (*Clm24508*)
- #37. Oxford, Bodl. Libr., Laud. lat. 104 + Erlangen, UB, 2112/7 (*L*)
- #40. Paris, BNF, lat. 2170 (*Par2170*)
- #45. Paris, BNF, lat. 2783 (*Par2783*)
- #46. Paris, BNF, lat. 2784 (*Par2784*)
- #47. Paris, BNF, lat. 3477 (*Par3477*)
- #48. Paris, BNF, lat. 6360 (*Par6360*)
- #50. Paris, BNF, lat. 14296 (*Par14296*)
- #51. Paris, BNF, lat. 14490 (*Par14490*)
- #52. Paris, BNF, lat. 18584 (*N*)
- #54. Prague, Capit., A. 137 (*Prague1*)
- #55. Prague, Capit., I. 33 (*Prague2*)
- #56. Reims, BM, 413 (*R*)
- #57. Seitenstetten, Stiftsbibl., 51 (*Seit*)
- #62. Vatican City, Bibl. Vat., Reg. lat. 202 (*Reg202*)
- #66. Vatican City, Bibl. Vat., Reg. lat. 412 (*Reg412*)
- #70. Vatican City, Bibl. Vat., Vat. lat. 1783 (*V*)
- #75. Vienna, ÖNB, 3204 (*Wien*)
- #76. Wolfenbüttel, HAB, 486 Helmst. (*Helm486*)

All but four of these manuscripts (*L*, *R*, *V*, and *Par2170*) invert *Ep.* 5.2 and 3, a feature which may be recognised as an innovation since it is limited to only part of an already identified family.<sup>26</sup> Of the four exceptions, *L* is the only one containing the opening of

<sup>26</sup> To be precise, this feature is also found in #60. Vatican City, Bibl. Vat., Ottob. lat. 2013. As this manuscript also displays an irregularity in the case of *Ep.* 8.2 (a half-leaf (f. 53) had to be added in order to copy it), it is fairly likely that its base belongs to the type of manuscripts under analysis here, but that it was completed and massively corrected following another family: see below. Thus for reasons of convenience, I shall neglect the 'first stratum' of *Ott2013*.



*Ep.* 9.7:<sup>27</sup> this must correspond to the state of the family's archetype. The other three recognised that *Ep.* 9.7 was incomplete and chose (either by themselves, or more probably following their model) to omit it entirely.

Of these other manuscripts, *R* and *Par2170* at least are contaminated: for instance, the former knew a manuscript where the ending of *Ep.* 7.18 was complete (the missing words are added in the margin). *R* is also unusual in that it signals, albeit unclearly, the lacuna between Books 7 and 8, which coincides with a change of page (between ff. 59v and 60). *Par2170* was completed (and heavily corrected) using another manuscript, and is related in a somewhat unclear way to #48. Paris, BNF, lat. 6360 (*Par6360*), with which it notably shares three medieval epistles (or rather, if I am right, one epistle in three parts) on the election of Sanctio as bishop of Orléans in 1096. An examination of their respective texts rules out the possibility that one was copied from the other.<sup>28</sup> At first glance, *Par6360* does indeed seem to be independent from this family, since it does not contain the lacuna between Books 6 and 7 of the letters and contains all of Book 9; yet the very order it follows in this book (letters 2–6, 1, 7–16) and the presence at the end of *Ep.* 9.6 of a 'rubric' in black (f. 88), *Explicuit epistole Sidonii*, prove that the first impression it leaves is deceptive. Therefore, I suspect that it is a close relative of *Par2170*, but one which was considerably more heavily contaminated.

Among the descendants of the manuscript that inverted *Ep.* 5.2 and 3, *T*, *Helm486*, *L97sup*, and *Reg412* form a recognisable group, not so much because they contain all or part of the *Carmina*,<sup>29</sup> but because they are alone in omitting *Ep.* 3.5–8. These four witnesses are, moreover, apparently independent from one another (but see below).

<sup>27</sup> Together with a manuscript which was at Fulda, and of which only one leaf remains, #89. Marburg, Hessisches Staatsarchiv, Hr 4, 15; see its description. Something should be said here of the gaps occurring in *L* from the beginning of the corpus until *Ep.* 2.2.15. They vary in length and location, and were studied very systematically by Burke (1911) 18–28 – perhaps too systematically. It is worth making the effort of transferring all the lacunae in *L* onto an edited text – an easy exercise now that a reproduction of the manuscript is available online – to realise that matters are not as systematic as Burke wished. Most of these lacunae do correspond to material defects in the exemplar, but Burke's hypothesis, that this was a manuscript written over two columns and with a damaged corner, does not work. The gaps can be found either approximately six lines apart (like Burke, I use Lütjohann's edition) or approximately eighteen lines apart. Separated by six lines are numbers 1–2, 4–5, 6–7, 13–14, and 15–16, then 18–19, 20–1, etc. down to 34–5, and finally 38–9. Separated by eighteen lines are numbers 2–3, 3–4, 7–8, 8–9, 9–10, etc. In short, though there does indeed appear to be a regular pattern, the alternation between six and eighteen lines is not systematic, so that Burke's hypothesis does not stand up. Besides, these gaps probably do not all correspond to lacunae in *L*'s model (but the chances that this is the case increase the more the space between the gaps is inaccurately calculated, as occurs fairly frequently: these are the places where the scribe was not able to determine the number of missing letters exactly): some of them probably cover words which the scribe was not able to understand even if, technically, he could read them. In any case, editors have used these gaps in a highly problematic way, for if one relies on them to identify lacunae in Sidonius' text – which must, as far as possible, be filled through guesswork – then one must accept that no preserved manuscript is prior to *L*, and this is not possible if my stemma is correct. Yet if one really wishes to rely on these gaps, this must be done consistently, and it would therefore be necessary to reject systematically any text transmitted by the manuscripts which fits in the gaps. If this were the case, Lütjohann's text would be interpolated in every gap except 2, 3, 5, 20, 21, 23, 25, and 29. Conversely, if I am right, it would mean that all the conjectural additions offered in the places cited are unjustified (if not in detail, at least in principle).

<sup>28</sup> These letters (or this letter) are not referenced, and apparently unedited. The two copies are quite bad, though independently so; besides, one may doubt that they were produced in the diocese of Orléans, since in both manuscripts the first occurrence of *Aurelianensis* (*Ecclesia*) is distorted into *aurebavensis* (*Par2170*) and *aut rebavensis* (*Par6360*). The name 'Sanctio' only appears in the 'final letter'.

<sup>29</sup> It is by no means evident at this stage (but it will be shown later on) that these *Carmina* were reinserted in these manuscripts from another branch: since the rediscovery of the fragments preserved in Erlangen, we now know that these *Carmina* were present in *L* from the beginning. If all the manuscripts of this family except *L* do indeed have

Just as easily recognisable is a Germanic family consisting of the two manuscripts from Munich, the two from Prague, together with *Seit* and *Wien*. They all transpose *Ep.* 5.8 after 5.11, and were able somehow or other to restore the ending of 7.18. Another common feature is the addition at the end of a note on the alleged local celebrity of Sidonius (transcribed in the description of *Seit*). It is probably reasonable to assume that *Prague1*, by far the oldest of the group, is also its archetype and was very probably imported from France (whereas all the others are indeed of Germanic origin): it contains the end of *Ep.* 7.18 as an addition written in the margin.

There is a further model that is common to *Douai*, *F131sup/2*,<sup>30</sup> *Par2784*, and *Par3477* (the latter before contamination, through which it gained *Ep.* 7.1–7 and 9.7–16), identifiable, for instance, by its omission of the first word of *Ep.* 7.16 (*facis*); perhaps other witnesses, in which this feature has been hidden through contamination, are also descended from it.

One final, verifiable filiation is that uniting *Mtp445* and *Par14490*; it is recognisable through the addition of the end of Book 9, and after it, of (previously omitted) letters from Book 7, with important lacunae, to reach the following order at the end of the volumes:<sup>31</sup> *Ep.* 9.2–13\*, 14\*, then 7.6\*, 7.2\*, 7.3–5\*. *Par14490* may well be the model for *Mtp445*: the former appears to have copied the letters discussed above at a later stage, whereas in the latter they were clearly part of the original copy. In addition, these two manuscripts seem to have a model in common with *Gks* and *Reg202*, a model in which *Ep.* 7.18 ended with *avocere* and not (as in the rest of the group) with *respiras*. This model was influenced by another manuscript, which contained *Ep.* 7.18 in an incomplete state, yet still extending beyond *respiras*. This provides a further argument for *Mtp445* being copied from *Par14490*: in both, *Ep.* 7.18 does indeed end with *avocere*, yet in *Par14490* this is the result of a marginal addition (as it originally ended with *respiras*).<sup>32</sup>

Of the other descendants of the manuscript inverting *Ep.* 5.2–3, *N*, *Mtp541*, *Par2783*, and *Par14296* cannot be related to any of the groups discussed above. As regards *Par2783*, one must be mindful of the fact that the manuscript was ‘contaminated’ through the systematic replacement of the folia which did not correspond to the state of the source of contamination. Thus numerous characteristic features are hidden; yet the inversion of *Ep.* 5.2 and 3 and the original ending of the collection at *Ep.* 9.6 are definitely present.

Finally, there remains *M*, or to be more exact, the original state of *M*, before its contamination and the resulting codicological complexity, both of which are well known.<sup>33</sup> *M* originally reduced Book 9 to 2–6, but does not invert *Ep.* 5.2–3. It must therefore be ‘related’ to *V*, *R*,

a common model – which seems to me to be the case – and if this model was responsible for the position of the *Cammina* right in the middle of Book 1, it would be possible (though admittedly improbable) that its descendants detected the problem, even on more than one occasion, and omitted the *Cammina* – perhaps, in some cases, with the intention of copying them after the letters, even though none actually did.

<sup>30</sup> For the letters only (for the *Cammina*, i.e. *F131sup/1*, see sect. 6 below). It is possible that this manuscript was copied in France, albeit perhaps by an Italian amanuensis, as all other known witnesses are clearly unconnected to Italy.

<sup>31</sup> I use the asterisk (as in the census below) to indicate the lacunose state of a piece, at the beginning (\*18) or at the end (18\*), whatever the cause.

<sup>32</sup> This does not imply, I think, that *Gks* and *Reg202* also descend from *Par14490*: perhaps the latter is faithful in the extreme (regarding this addition at least) to their common archetype. Besides, the addition may have occurred independently in *Par14490* and in an archetype common to *Gks* and *Reg202* only.

<sup>33</sup> On the manuscripts descending from *M*, see n. 38 below.

and *Par2170*; in other words, like them, it descends from the manuscript which eliminated what came after *Ep.* 9.6, without, however, descending from the manuscript which inverted *Ep.* 5.2–3. The overall shape of the family is indicated in Fig. 16.1.

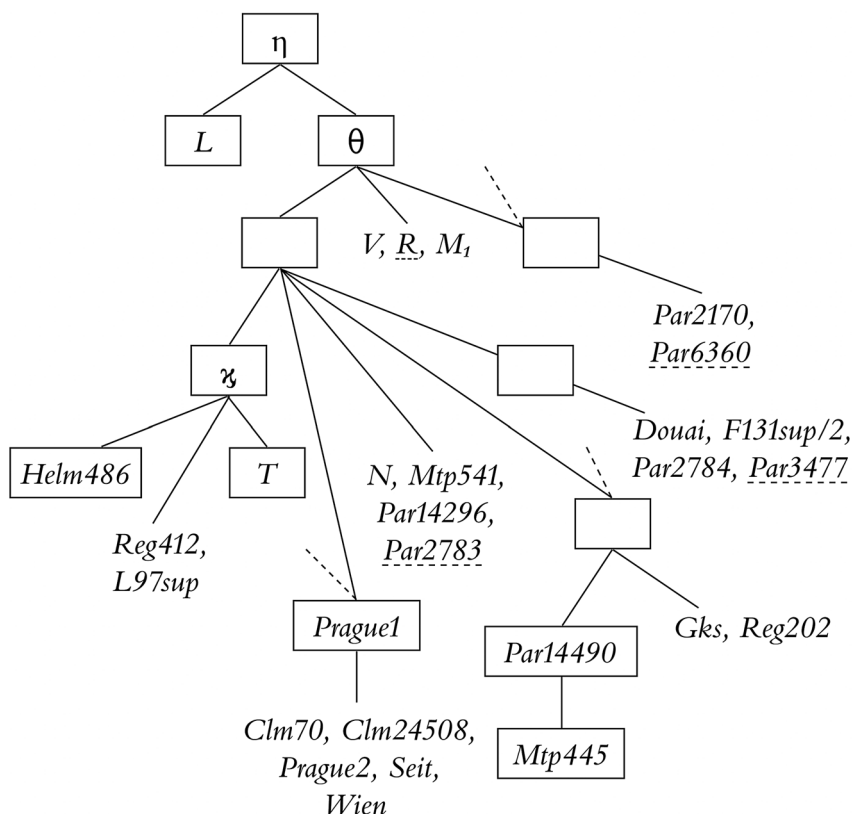


Figure 16.1 Stemma of the so-called ‘fourth family’

#### 4 The English Family

Six manuscripts, all probably copied in England, constitute a homogeneous group and derive from a single archetype, hereafter called  $\gamma$ . They are:

- #19. Hereford, Cath. Libr., O. II. 6 (*Hfd*)
- #23. London, BL, Royal 4 B. iv. (*Roy*)
- #35. Oxford, Bodl. Libr., Auct. F. 5. 25 (*Auct*)
- #36. Oxford, Bodl. Libr., Digby 61 (*Dig*)
- #38. Oxford, Bodl. Libr., Rawl. G. 45 (*Rawl*)
- #49. Paris, BNF, lat. 9551 (*F*)

*F*, *Roy*, and *Rawl* are complete; *Dig* (which lost its first quires through an accident, and thus begins at *Ep.* 3.\*12) contains only the letters and *Carm.* 1–2; *Hfd* contains only the letters; *Auct* contains the letters only until 5.3 (and this already slender selection has suffered multiple material losses).

In three of these manuscripts (*Roy*, *Rawl*, and *Hfd*), letters 5.12 and 18 are copied again after Book 9 – without any immediately visible reason, since the text is broadly the same. Three manuscripts (*Roy*, *Rawl*, and *Dig*) contain two poems by Eugenius of Toledo (copied without the author's name), and two of them (*Roy* and *Rawl*) contain Ausonius' *Ecloques* 20 and 21. Finally, all of them (except *Auct*, which is interrupted before) share an easily identifiable characteristic, the inversion of *Ep.* 5.12 and 13.

The section of the corpus for which all these witnesses are extant is not very large (from the middle of *Ep.* 3.12, since everything preceding is missing in *Dig*, until *Ep.* 5.3, the end of the copy in *Auct*). It contains at least one certain error (besides a few variants, minor omissions, and inversions) which is characteristic of the group:

3.13.4 omnino] *om. Hfd Roy Auct Dig Rawl F*

Otherwise, all these manuscripts share the loss of *-currentibus* at *Ep.* 4.20.3, except *Auct*, which must therefore predate the others (and is, in fact, the oldest of these manuscripts). Similarly, at *Ep.* 4.22.1, all the manuscripts except *Auct* and *Hfd* add *studio* either after *summo* in the case of *F*, *Roy* (which omits it before correction), and *Rawl*, or before *summo* in the case of *Dig*. That is sufficient to establish the relationship between these manuscripts (see Fig. 16.2), but the situation is more complicated when it comes to their relationships with the rest of the tradition. Here we must anticipate what follows. In a nutshell, the corpus of which these manuscripts are a witness is a reconstruction: as far as the letters are concerned, they are connected to an archetype *v* (described in the next section) which is situated relatively low on the stemma, whereas they derive the poems from a much higher branch of the stemma, and therefore contain all of them, not only (as *v* and its other descendants) the panegyrics (with *Carm.* 2 incomplete at the end). This will be set out in the final synthesis (Fig 16.7 below).

*Roy* poses an additional problem, because the text of the *Caesares* that it transmits points to a third branch of the stemma.<sup>34</sup> In both its contents and textual detail (for Sidonius), *Roy* does not particularly differ from its English relatives: it does not seem contaminated. Hence I assume that *Roy* knew a manuscript of Sidonius containing the *Caesares*, deduced that the latter was a work of Sidonius, and therefore copied it (I say *Roy*, but the responsibility probably lies with its antigraph, since the *Caesares* was clearly copied at the same time as Sidonius in *Roy*); the scribe did not collate this last manuscript, either through lack of interest or courage, or deeming it inferior to his principal, English model.<sup>35</sup>

Finally, we should note that *F* is contaminated,<sup>36</sup> but most importantly, it is the only one in its family that is. It seems to me that this could provide support for the hypothesis that the whole group (except *F*, of course) originated in the West Country, which one might suppose was less likely to be in touch with the continent than the more central regions of England.<sup>37</sup>

<sup>34</sup> See sect. 5.2, p. 496, below.

<sup>35</sup> One might suppose that it is from the same manuscript containing the *Caesares* that *Roy* derived those glosses which it did not inherit in the usual way (and which are those also found in *Auct*).

<sup>36</sup> It is the only member of its family to have the addition *si casu dentium* . . . at *Ep.* 1.2.2; see also sect. 5.2, p. 495, below.

<sup>37</sup> See in particular Chronopoulos (2010) 266–7; but the caution that she expresses remains appropriate, and we should also remember that the exemplar of *Roy* at least must have known a 'foreign' manuscript.

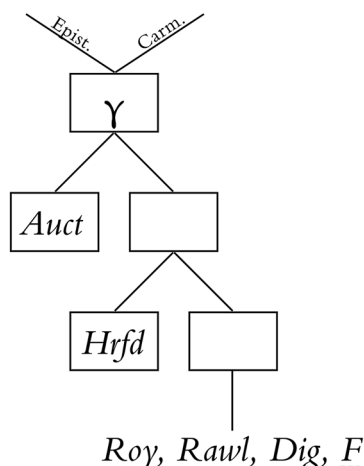


Figure 16.2 Stemma of the English family

### 5 Manuscripts Containing the *Epistulae* and the *Panegyrici*

The group of manuscripts containing only the *Epistulae* and the *Panegyrici* (and thus excluding the *Carmina minora*) probably represents an accidental collection. In other words, these manuscripts are unlikely to derive from an archetype which explicitly selected only this part of Sidonius' works, since the said archetype must also be responsible for the loss of the greater part of *Carm.* 2, from 183 onwards, which is also missing in all its descendants (with two exceptions, through contamination). I would hesitate to class the transposition of *Carm.* 8 before *Carm.* 6 as a characteristic innovation, since the idea of placing the dedication of the Panegyric on Avitus (to Priscus Valerianus) before the poem itself (*Carm.* 7) and its preface (*Carm.* 6) is not fundamentally illogical: such is the order of the texts for the Panegyric on Majorian (*Carm.* 3 = dedication to Petrus; *Carm.* 4 = preface; *Carm.* 5 = panegyric). But, despite my theoretical objection to taking this transposition into consideration, it is worth noting that the results of doing so would fit perfectly with my classification.

This family includes the following manuscripts:

- #1. Avranches, BM, 242 (*Avr.*)
- #2. Berlin, Staatsbibl., lat. fol. 591 (*Ber1*)
- #3. Berlin, Staatsbibl., Phillipps 1685 (*Ber2*)
- #4. Bern, Bürgerbibl., 285 (*Bern*)
- #10. Copenhagen, Kongelige Bibl., Thott 50 2° (*Thott*)
- #14. Florence, BML, plut. 45. 26 (*Pl4526*)
- #15. Florence, BML, plut. 90 *sup.* 8 (*Pl90*)
- #16. Florence, BML, S. Marco 554 (*M*)
- #21. Leipzig, UB, Rep. I 48 (*Leip*)
- #29. Montpellier, BU Méd., H 4 (*Mtp4*)
- #41. Paris, BNF, lat. 2171 (*Par2171*)

- #44. Paris, BNF, lat. 2782 (*Par2782*)
- #58. Stockholm, KB, Va 26a (*Stk*)
- #60. Vatican City, Bibl. Vat., Ottob. lat. 2013 (*Ott2013*)
- #63. Vatican City, Bibl. Vat., Reg. lat. 203 [first part] (*Reg203/1*)
- #65. Vatican City, Bibl. Vat., Reg. lat. 216 (*Reg216*)
- #68. Vatican City, Bibl. Vat., Urb. lat. 1515 (*Urb1515*)

They are all ‘complete’, that is, they all contain *Carm.* 1–8 (apart from the end of *Carm.* 2, which is lost), except *Leip*, which only contains *Carm.* 1–5 (with the end of *Carm.* 2 and *Carm.* 21 added at a later stage), and *Avr*, which contains all the (complete) *Carmina* after contamination. *Reg203/1* is a special case, since the part of it which is relevant here is the one containing the *Epistulae*.

We can eliminate *Pl90* and *Urb1515* straightaway: they are both copies of *M*, and can thus be disregarded.<sup>38</sup>

A subgroup of these manuscripts is easily recognisable in that it ends the *Epistulae* with an incomplete and garbled version of *Ep.* 9.16: I shall deal with this group first, before returning to the classification of the remaining manuscripts.

### 5.1 Manuscripts with an Incomplete and Garbled Version of *Ep.* 9.16

Now the following group of manuscripts is immediately recognisable through a number of innovating characteristics, some of which are clearly identifiable as mistakes.

- #2. Berlin, Staatsbibl., lat. fol. 591 (*Ber1*)
- #3. Berlin, Staatsbibl., Phillipps 1685 (*Ber2*)
- #4. Bern, Bürgerbibl., 285 (*Bern*)
- #10. Copenhagen, Kongelige Bibl., Thott 50 2° (*Thott*)
- #29. Montpellier, BU Méd., H 4 (*Mtp4*)
- #41. Paris, BNF, lat. 2171 (*Par2171*)
- #63. Vatican City, Bibl. Vat., Reg. lat. 203 [first part] (*Reg203/1*)
- #65. Vatican City, Bibl. Vat., Reg. lat. 216 (*Reg216*)

Most evidently, the poem included in *Ep.* 9.16 is given in a garbled version (in the following order: lines 1–9, 27–35, 10–26, 54–77, 36–53; lines 78–84 and the prose conclusion are missing), which must have originated in a faulty reading of an archetype where the poem

<sup>38</sup> This has already been demonstrated for *Pl90*: see Marchiaro (2009) 283, and also sect. 6, p. 502, below. As for *Urb1515*, one need only compare the order of the corpus: the reason why the scribe of *Urb1515* wrongly copied *Ep.* 3.3 after *Ep.* 1.2 evidently has to do with the material state of *M*; he only understood the cross-reference in *M* afterwards. This filiation is fully confirmed by a selective survey of the *Carmina*. I believe that #18. Florence, Bibl. Riccard., Ricc. 247 is also a descendant of *M*, because *M* is the only MS known to me which would explain why the copyist of the Riccardian MS omits vv. 79–80 in the poem inserted at *Ep.* 9.16.3; but oddities in the order of the letters still leave room for doubts. The same can be said of #34. Naples, BN, IV. B. 39, and for the same reasons (*Ep.* 9.16.3 vv. 79–80 are by another hand); but it should be noted that this manuscript was corrected (throughout) and completed (for *Carm.* 2), very likely from a printed edition.



was written at least in part over two columns.<sup>39</sup> With the exception of *Reg203/1*, these manuscripts contain the letters as well as the panegyrics. *Berl1*, *Berl2*, *Bern*, *Thott*, *Mtp4*, and *Par2171* also contain Ausonius' *Caesares*; it is probable that *Reg216* also had the *Caesares*: the manuscript in its present state is interrupted by a lacuna in the course of *Carm.* 7.

*Berl1*, *Berl2*, and *Mtp4* are all witnesses of a collection which must originally have contained Cassiodorus' *Variae*, Sidonius, and a compilation of poetry centred on Hildebert. *Berl2* is the only complete witness of this collection: *Berl1* contains only Sidonius and Hildebert, and *Mtp4* only has Cassiodorus and Sidonius (with works taken from elsewhere inserted between them). Their common origin may be deduced in particular from the erroneous placing of lines 39–41 of the *Caesares* after 17; however, *Berl2* is also characterised by a certain number of distinctive mistakes, a fact which, together with the differing content of each volume, leads to the conclusion that none of them is the source for the other two. *Berl2* cannot come from Pontigny (as has sometimes been claimed) for the simple reason that it does not contain – and never can have contained, given the numbering of the quires – the initial Cassiodorus which is mentioned by the various descriptions of the manuscript attested in this abbey. In addition, the last catalogues of Pontigny very clearly state that the text of Sidonius was lacunose because the end of the manuscript was lost – which does not correspond to any volume known today.<sup>40</sup> However, it is possible – though impossible to probe as things stand – that the Pontigny manuscript was an ancestor of these three surviving manuscripts (presumably indirectly, since *Berl1* and *Berl2* contain exactly the same Hildebertian collection, which is slightly different from that of the Pontigny MS).

*Thott* and *Par2171* are immediately identifiable as twins, both because they have exactly the same layout (as is demonstrated by the indication of their foliation in their descriptions in the census), and because they share a certain number of common mistakes:

*Ep.* 1.1.1 uno] novo

*Carm.* 5.161 arte] om.

*Carm.* 7.178 ac torque] auctor circoque

Nevertheless, they are a priori independent from one another: *Thott* was quite regularly corrected using an exemplar from outside the family (see the next example for such a correction; it is probably also from the exemplar used for these corrections that *Thott* recovered line 28

<sup>39</sup> Another innovation that is characteristic of a number of these manuscripts, and of some interest, is the additional text at *Ep.* 2.2.5, placed either after *personas* (*Berl1*, *Berl2*, *Mtp4*) or after *imbricarentur* (*Thott*, *Par2171*, *Reg216*). Whatever the editors (except Lütjohann) might think, this cannot be anything other than a gloss: *ipsa vero convenientibus mensuris exactissima spatiositate quadratur*. It is omitted in *Reg203/1*: so it was probably still laid out as a gloss (which *Reg203/1* does not copy) in its model. In *Bern* it is an addition by the second hand, the one that completed the MS by adding ff. 56–97 (hereafter I speak of *Bern* only in its definitive state: the original one is of no particular interest, whatever its origin). See also in this volume Furbetta, ch. 17, sects. 2.3 and 2.4, on Woweren's and Savaron's editions, which contain this and other additional pieces of text.

<sup>40</sup> See Peyrafort-Huin (2001) items A 102, C 141, E 40, F 5, G 293. From this last notice onwards the situation is complicated, because under G 289 one finds a closely related Hildebertian collection, a fact which could be taken to suggest that the original manuscript had been split up; yet if this were the case, G 293 would be partially false, since it would wrongly mention the Hildebertian collection, which could not be there any longer. The simplest hypothesis, in my view, is that G 289 and G 293 were two entirely different codices; G 293 was still complete, but had lost its ending (leading to the loss of the end of Sidonius and all Hildebert) when H 161 and I 59 were written. G 289, which had apparently never been catalogued before, would thus correspond to H 62, and to the manuscript known today as Montpellier, BU Méd., H 35.

of the *Caesares*, omitted nearly everywhere else, and the addition *si casu dentium* at *Ep.* 1.2.1, inserted by mistake after *nervis*), whereas the text of *Par2171* is free from any such contamination. In theory, it is possible that *Par2171* was the model for *Thott*, but this is very unlikely: its text is mediocre compared with that of *Thott*, which is far superior even before the contamination.

The archetype of these two manuscripts is related to *Reg216*:

*Ep.* 1.2.5 *si venatione]* est *si venatio* *Thott*<sup>41</sup> *Par2171* *Reg216*

*Ep.* 3.12.5 (v. 14) *purgans]* *purgatus* *Thott* *Par2171* *Reg216*

*Carm.* 5.176 *sparso]* *spargo* *Thott* *Par2171* *Reg216*

These three manuscripts as well as *Berl1*, *Berl2*, and *Mtp4* probably derive from a single archetype, and all the available evidence points to #58. Stockholm, KB, Va 26a (*Stk*). According to Gustafsson,<sup>41</sup> this manuscript, of which I only know ff. 118–118v at first hand,<sup>42</sup> copied the *Carmina* in columns that were too narrow to contain all the verses on one line. This caused a number of transpositions, some of which are very poorly indicated; this is the case notably at *Carm.* 7.224–5, where *amicum* and *-pulsam* are moved back to the end of line 220. Gustafsson notes that there is an exact correspondence between the transpositions in *Stk* and the omissions in *Bern*: in fact, these omissions are characteristic of the entire group. The layout of *Stk* is not isolated: *Par2782* (which, textually, is probably very close to *Stk*) has roughly the same, and presents comparable problems of transposition at the end of verses.<sup>43</sup> However, I do not think that any manuscript other than *Stk* transposes both the *amicum* of *Carm.* 7.224 and the last two syllables of *repulsam* at *Carm.* 7.225 in an equally incomprehensible way (*Par2782* in any case does not). Therefore, it seems a reasonable working hypothesis that all the manuscripts with *Ep.* 9.16\* are descendants of *Stk* (albeit indirectly: a hyparchetype is needed, particularly to explain the problems related to this final letter). This assumes that *Stk* does indeed correspond to Gustafsson's description, which will need to be verified by consulting the original.

Two of *Stk*'s descendants have distinctive features. *Bern* does not systematically bear the characteristic features of the family (see, for instance, *Ep.* 1.2.9 [line 21 in Lütjohann], where *illis* is replaced by *vel* except in *Bern*). This is due to the fact that *Bern* is in fact a two-step manuscript, that is, a manuscript originally produced with a smaller corpus (whose archetype I have not investigated), extended a few years later with the help of a superior model.<sup>44</sup> On the other hand, *Reg203/1*, though it is necessarily a member of the same family (it also shares the loss of the rubric at *Ep.* 9.16), was heavily contaminated, at least at the beginning; it is difficult to determine the origin of its contamination with any precision.

<sup>41</sup> Gustafsson (1882) vii n. All the variants quoted for *Bern* are shared by the whole group except the last one, *Carm.* 7.309, where *Bern* is the only manuscript to have *est* followed by a blank space instead of *estie*, which has remained uncorrected in the text of its cousins.

<sup>42</sup> I would like to extend my thanks to the National Library of Sweden in Stockholm for graciously providing reproductions of these pages.

<sup>43</sup> However, *Stk* and *Par2782* are independent. Discussing the manuscript transmission of the *Deidamia Adhilli*, Stohlmann (1973) 217–23 reaches the same conclusions. (The third MS of the letter, Oxford, Bodl. Libr., Auct. F. 1. 17, England, s. xiv<sup>1</sup>, is much closer to *Stk* than to *Par2782* but is still not a descendant of the former.)

<sup>44</sup> On the material state of the codex, see its description; the important point had already been grasped by Lütjohann (1887) ix.

One final word on the history of this family (see Fig. 16.3): we saw above that some of its members were linked to Pontigny; it also happens that the manuscript of Sidonius located at Clairvaux contained the same texts (the letters and the panegyrics, with *Carm.* 7 as the volume's final piece), and must have had a very similar layout to *Reg216* (judging by the incipit of f. 2, mentioned in Pierre de Virey's catalogue).<sup>45</sup> One may thus speculate that the entire family was Cistercian.

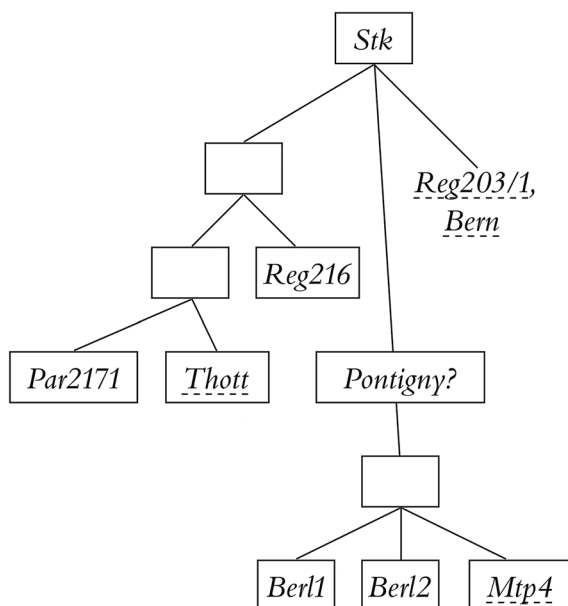


Figure 16.3 Stemma of descendants of Stockholm, KB, Va 26a (*Stk*)

## 5.2 The Remaining Manuscripts

Once we eliminate the *codices descripti* and identify the descendants of *Stk*, we are left with a much more restricted group of manuscripts ‘containing the panegyrics’:

- #1. Avranches, BM, 242 (*Avr<sub>1</sub>*)
- #14. Florence, BML, plut. 45. 26 (*Pl4526*)
- #16. Florence, BML, S. Marco 554 (*M*)
- #21. Leipzig, UB, Rep. I 48 (*Leip<sub>1</sub>*)
- #44. Paris, BNF, lat. 2782 (*Par2782*)
- #58. Stockholm, KB, Va 26a (*Stk*)
- #60. Vatican City, Bibl. Vat., Ottob. lat. 2013 (*Ott2013*)

<sup>45</sup> The shelf mark is I 21 in Pierre de Virey's catalogue, and 1408 in Mathurin de Cangey's; the manuscript no longer appears in the catalogue of 1664. F. 2 began with *[ve]neratur quamquam* (*Ep.* 1.2): no surviving codex corresponds to these characteristics. See Vernet (1979–98) 1.162.

In the poems, the common origin of these manuscripts is attested by numerous mistakes (in the case of *Avr* and *Leip*, before corrections). For instance, if we focus on the opening of *Carm.* 2:

4 fastis] *om.* (*sed Par2782 ante crinem add. p.c. cingere*)

34 *om.*

56 *om.*

162 lesbo sate pittace] lesbos epittace

That said, it is very difficult to work out any detailed picture of the relationship between these witnesses. I am almost certain that they derive from their archetype following two branches, the first including *M* and *Avr*<sub>1</sub>, and the second all the others (*Pl4526*, *Leip*<sub>1</sub>, *Par2782*, *Stk*, *Ott2013*):

*Carm.* 5.101 est nunc praeterea eximius, quem saecula clamant  
praeterea] *M, om. Pl4526 Leip Par2782 <Stk> Ott2013*, praetura *cett.*  
quem] praescia *add. Pl4526 Leip Par2782 <Stk> Ott2013*

In this example, I am merely guessing the reading of *Stk*, but it is almost certain given what we know about it (see above on the close relationship it probably had with *Par2782*). This attests, then, to the existence of a manuscript from which all of these (but not *M* or *Avr*<sub>1</sub>) are descended. This manuscript, which we shall call *v*, has one immediately noticeable feature: it has arranged Book 7 in a particular order, in which 12 comes after 7, and is immediately followed by *Ep.* 6.11, which is extracted from its own book; the latter at least is a definite error.<sup>46</sup> This allows us to relate to *v* both the text of the letters witnessed by  $\gamma$  (the English family) and certain manuscripts which only contain the letters, and therefore obviously do not share the innovations found in the poems:<sup>47</sup>

#8. Copenhagen, Kongelige Bibl. (*olim* UB), Fabr. 91 4° (*Fabr*)

#22. London, BL, Harley 4084

#42. Paris, BNF, lat. 2171<sup>A</sup>

#77. Wolfenbüttel, HAB, 1027 Helmst.

At *Ep.* 1.2.2, *Fabr* has the addition beginning with *Si casu dentium* which is mentioned in Lütjohann's apparatus as occurring in *F*. It does indeed appear there, but through contamination, since no descendant of  $\gamma$  has it; it also appears in *Thott* (thereby enabling us to identify the source of *Thott*'s contamination) and in two further manuscripts, #61. Vatican City, Bibl. Vat., Reg. lat. 166 and #64. Vatican City, Bibl. Vat., Reg. lat. 209, which should thus be considered as relatives of *Fabr*.<sup>48</sup>

Conversely, it is more difficult – but not impossible – to demonstrate the existence of a hyparchetype that is specific to *M* and *Avr*<sub>1</sub>; the variants in *M* indicated by Lütjohann and not

<sup>46</sup> I am grateful to Joop van Waarden for bringing this point to my attention.

<sup>47</sup> However, this argument cannot be used to classify #7. Clermont-Ferrand, BM, 260: contrary to Lütjohann's assumption ((1887) xii – in his defence, he did not have first-hand access to the manuscript), it has *Ep.* 6.11 in the right place. I have no idea where this manuscript should be situated. Conversely, this characteristic allows us to identify *v* as the source of the contamination affecting *Par2783* and *Par6360*, identified above as members of the 'fourth family'.

<sup>48</sup> See Lütjohann (1887) xx–xxi, who was wrong not in his argument but about its significance. He was partly deceived by his attribution of an excessively important role to *F*.

shared by the rest of the group (except *Avr<sub>1</sub>*) are, it has to be said, very rare. Nevertheless, they do exist. The most obvious are in the *Caesares*, and they seem sufficient to me. Following the order of the text, the main variants are: the omission of *duplicem* at line 22;<sup>49</sup> the (metrically impossible) omission of *et* at line 24 (*Galba senex, Otho lascive et famose Vitelli*); and the omission of *sibi* at 27 (*Implet fatalem decadam sibi Vespasianus*). These errors are also found in two other manuscripts of the *Caesares*, #5. Brussels, BR, 10020–21 (*Br1*, which contains only the letters) and #23. London, BL, Royal 4 B. iv. (*Roy*), which both share further errors, such as, for example, the shifting of lines 23–9 after 36. It is impossible to draw conclusions from this in the case of *Roy* (see above); yet there is no reason not to link *Br1* to *M* and *Avr<sub>1</sub>* for the totality of its contents.

The situation is unclear for the other manuscripts, *Pl4526*, *Leip<sub>1</sub>*, *Par2782*, *Stk*, and *Ott2013*.<sup>50</sup> I believe that *Pl4526* and *Ott2013* can be connected without much hesitation on the grounds that they both have appendices containing a small poetic corpus, which includes excerpts from Corippus and Fortunatus as well as anonymous *Versus VII Sapientum* (see the description; *Ott2013* is a rather fine copy in which difficult passages are left blank and copied in the margins when the scribe was not able to correct them; it contains a few valuable conjectures). The same applies for *Stk* and *Par2782*, which both contain the *Deidamia Achilli*.

For editorial purposes, all these manuscripts (whose relationships are indicated in Fig. 16.4) may be ignored except *M*, which is the best by far; however, prudence suggests that *M* should be tested case by case using a manuscript from the other branch, in order to weigh tradition

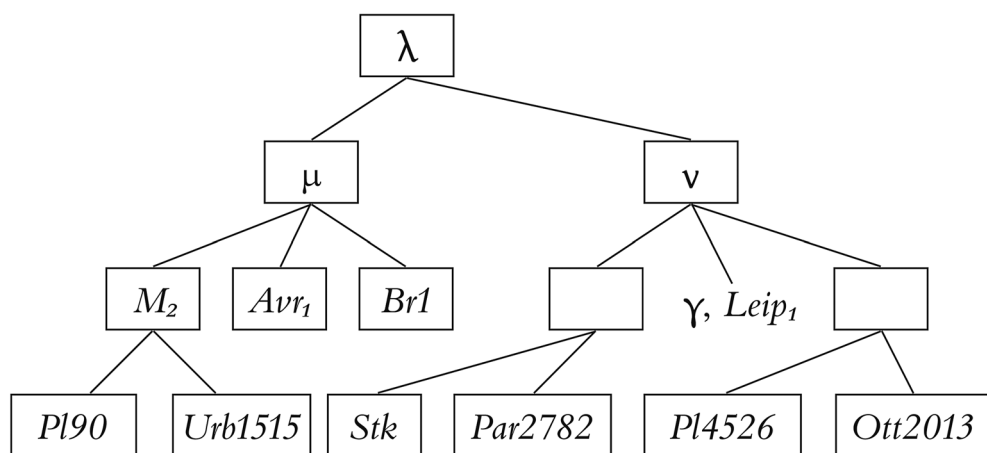


Figure 16.4 Stemma of the manuscripts with *Cam. 2* incomplete

<sup>49</sup> By entirely omitting the word *duplicem*, *M* and its siblings are just faithfully reproducing the text of the *Caesares* as it stood in  $\lambda$  (and this reading may reach further back, as far as  $\beta$ ), whereas  $\nu$  spotted the defect and cleverly, but without authority, corrected it to *binam*. The  $\alpha$  family of Sidonius still has *duplicem*.

<sup>50</sup> I note in passing that of all the manuscripts I have encountered, *Pl4526* is one of only two to provide a portrait of Sidonius, which is reproduced on the cover of this volume. The other is #53. Paris, IRHT, collection privée 347, the former Schøyen MS, on f. 109, in the initial to *Cam. 9. Leip*, which is (coincidentally) from the same family as *Pl4526*, also stands out in terms of illustrations: its margins contain 'figurative glosses' which would probably be worth studying in their own right.

against innovation: any of the above manuscripts would do the job, except *Leip*<sub>1</sub> (to be on the safe side) – which would in any case deserve a collation in its own right, if my discussion below is correct.

## 6 Manuscripts containing the *Carmina*

We have already noted the unique origin of certain manuscripts transmitting the *Carmina*, whether or not they are complete: the English family (γ), the family transmitting only the letters and panegyrics (λ), and the group *T Helm486 Reg412 L97sup* (χ). The others are as follows:

- #1. Avranches, BM, 242 (*Avr*<sub>2</sub>)
- #25. Madrid, BNE, 9448 (*C*)
- #43. Paris, BNF, lat. 2781 (*P*)
- #53. Paris, IRHT, collection privée 347 (*olim* Schøyen Collection, 246) (*Sch*)
- #69. Vatican City, Bibl. Vat., Vat. lat. 1661 (*Vat1661*)
- #71. Vatican City, Bibl. Vat., Vat. lat. 3421 (*A*)

To these should be added (though it now only contains fragments of *Carm.* 7 and 9):

- #37. Oxford, Bodl. Libr., Laud. lat. 104 + Erlangen, UB, 2112/7 (*L*)

In addition, a few manuscripts contain only the *Carmina* without the letters (with the exception of *Br2* and *Vat5219*, but these only have insignificant excerpts of the letters); these are:

- #6. Brussels, BR, 10615–729 (*Br2*)
- #12. Florence, BML, conv. soppr. 6 (*Conv*)
- #17. Florence, BNC, Magl. VII 315 (*Magl*)
- #27. Milan, Bibl. Ambr., F 131 *sup.* [first part] (*F131sup/1*)
- #59. Vatican City, Bibl. Vat., Ottob. lat. 126 (*Ott126*)
- #63. Vatican City, Bibl. Vat., Reg. lat. 203 [second part] (*Reg203/2*)
- #67. Vatican City, Bibl. Vat., Urb. lat. 649 (*Urb649*)
- #72. Vatican City, Bibl. Vat., Vat. lat. 5219 (*Vat5219*)

We can deal straight away with the manuscripts containing the panegyrics, which I shall henceforth reduce to their archetype λ, the text of which they all share – disregarding contamination – for the quoted variants: λ is undeniably descended from an archetype that is closer to *L*, as can be seen in the excerpts of *Carm.* 7 which the latter contains. Here are the most notable variants:

- 563 camillus] cimillos *L*, cum illos λ
- 572 forte loco pia] porte loco capta *L*, porte locopta λ
- 579 donantque] namque *L* λ

However, the textual state represented by λ is older than *L*, because it does not share the characteristics of the ‘fourth family’.



For instance, one can, using *Carm.* 9, identify a group including *P*,  $\mathfrak{x}$  (represented by *T* and *Helm486* only, since *Reg412* and *L97sup* do not contain this poem), and *Br2*; I mention *L* explicitly when it is present:

115 curribus occidens] o. c. *contra metrum* *P*  $\mathfrak{x}$  *Br2*  
 148 zmyrnae] zminae *P*  $\mathfrak{x}$  *Br2* (*sed smirnae Helm486*)  
 292 mutato lare Gallias] *L cett.*, muta tolerare gallia *P*, mutatas tolerare gallias  $\mathfrak{x}$  *Br2*  
 297 baetin] *L cett.*, haetin *P*  $\mathfrak{x}$  *Br2*

If we can only cite one example of a certain relationship of these manuscripts to *L*, it is surely due to the brevity of the surviving text in the latter:

313 hoeni] hent *P T Br2 L*,  $\tilde{\text{hnt}}$  (*i.e.* habent) *Helm 486*

This seems to pose a sizeable problem, to which we shall return shortly; let us first deal with the simpler questions. The example quoted at line 292 implies the existence of a common model for  $\mathfrak{x}$  and *Br2* only; this is confirmed in other places, such as:

107 est] *om.*  $\mathfrak{x}$  *Br2*

The characteristic variants of  $\mathfrak{x}$  and *Br2* are also found – though often corrected, rendering some of them invisible – in *Conv*, and therefore in its descendants *Magl* and *Urb649*.<sup>51</sup> *Conv*, which belonged to Giorgio Antonio Vespucci, is a Florentine production from the early 1470s; although I have not looked for definitive proof, it is almost certainly a copy of *T*. As such, it can be ignored together with its descendants. *F131sup/1* and *Ott126* are probably also descendants of *Conv*; they share the latter's innovations, including the omission of *Carm.* 21.

Since *Reg412* and *L97sup* do not transmit *Carm.* 9, I cannot determine the exact relationship between *Br2* and  $\mathfrak{x}$ . Is *Br2* the latter's sibling or offspring? Though I have no proof of this, I would tend towards the former hypothesis, because *T* is considerably closer to *Helm486*, *Reg412*, and *L97sup* than to *Br2*: if they were all siblings, there would be less of a gap between them. Besides, I think that *L97sup* is probably a copy of *Reg412*, but the textual evidence is yet to be found. If I am right, though, it would add a fascinating element to the unknown history of *Reg412* as well as to the far better known history of the library of Archbishop Piccolpasso, who probably ordered the copy of *L97sup*.

Contrary to what one might expect, *Vat5219* is not a descendant of *T* (nor, of course, does it descend from *Conv*, which is probably later and lacks *Carm.* 21); it is thus closer to *Br2*. Since the latter belonged to Nicolaus Cusanus, it seems not unlikely that it was the source used by Bussi (who was a friend of Nicolaus Cusanus) to copy *Vat5219*.<sup>52</sup>

To summarise: at this stage, a manuscript ( $\kappa$ ) generates (1) *Br2*, which itself generates *Vat5219*, and (2) a common model ( $\mathfrak{x}$ ) for *T*, *Helm486*, *Reg412*, and *L97sup*. *T* engenders *Conv*, itself the source of *Magl*, *Urb649*, *F131sup/1*, and *Ott126*. From here onwards I shall reduce the entire group to *T* and *Br2*, which are amply sufficient to represent it. The problem

<sup>51</sup> Reeve (1977) 204 n.11.

<sup>52</sup> I have collated *T*, *Helm486*, *Reg412*, *L97sup*, *Br2*, and *Vat5219* only for the parts of the poems contained by *Reg412* and *L97sup* (the end of *Carm.* 22 and the opening of *Carm.* 23). The collation reveals an undeniable relationship between *Br2* and *Vat5219*, even though the evidence is slender, given the small size of the corpus: *Carm.* 22 ep. 6 (ed. Lütjohann, p. 250, l. 6) istum] illum *Br2 Vat5219*; l. 7 earini] farini *codd. praeter Br2 Vat5219* (sarini) et *Helm486* (farvii); *Carm.* 23.1 cum] vel non *Br2* (*contra metrum*), non *Vat5219*; 28 sollium] solium *Br2*, soltum *ut vid. Vat5219*. In these passages, *T*, *Helm486*, *Reg412*, and *L97sup* contain no errors against the two others.

lies further up: the variants quoted above (for *Carm.* 9.115, 148, 292, 297) give the impression that  $\kappa$  is a sibling of  $P$ , and therefore that  $L$  is older than them. If the stemma offered above for the ‘fourth family’ (see Fig. 16.1 above) is correct – and it definitely is on this point at least – then this is impossible: given the corpus of letters it contains,  $P$  cannot be situated lower than  $L$ . In fact, the solution is provided by a point briefly touched upon above: when the ‘fourth family’ was first classified, it was not possible fully to demonstrate that the *Carmina* in  $\mathfrak{z}$  (and therefore also in  $\kappa$ ) derived from an external source and not from a normal inheritance. We have just seen that the textual analysis of these *Carmina* provides confirmation of this:  $\kappa$  inherited its text of the *Epistulae* from a relative of  $L$ , but its text of the *Carmina* was derived from a relative of  $P$ . This can be represented synthetically by the stemma in Fig. 16.5, where I use continuous lines to represent the tradition of the *Epistulae*, and dotted lines for that of the *Carmina* (thus the dotted lines exceptionally do not indicate contamination).

Incidentally, we might note that this gives us the situation of  $P$  for the *Epistulae*, which is necessarily higher than  $L$ ; this is confirmed by the fact that  $P$ , like  $L$ , omits *Ep.* 7.6–7 but does not have the lacuna which is typical of  $\eta$  (*Ep.* 6.12\*–7.\*5).  $P$  has not been given enough attention; I have gone through three books of the *Epistulae* (2–4) using Mohr’s apparatus (which is more useful for such a purpose, notably because it generally omits variants in spelling, and collates  $V$  and  $N$ ), and found perhaps a dozen mentions of  $P$ , all stemmatically insignificant. There are only two possible explanations for this: either the constitution of Mohr’s text is disastrous, or  $P$  has an excellent text. The second solution is correct: it makes sense that the manuscripts which are closest to the top of a stemma should appear the least frequently in an apparatus.

$C$ ,  $A$ ,  $Sch$ , and  $Vat1661$  form another, radically different branch. It is relatively difficult to find mistakes in them, but not impossible; however, in *Carm.* 4–5 and in a few passages here and there,<sup>53</sup> I have not found anything definitely irrefutable. Still, one should mention *solveret*

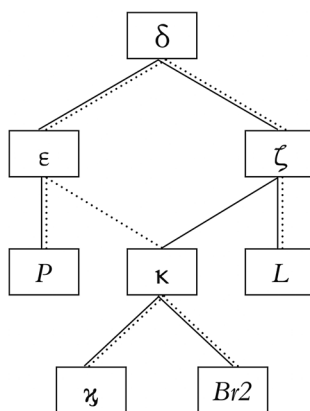


Figure 16.5 Stemma of ancestors and descendants of archetype  $\kappa$

<sup>53</sup> The reason why I chose to use *Carm.* 4–5 at this point is purely practical, for I originally knew *Sch* only through photographs of the folia corresponding to these poems, which Tiziana Brolli was kind enough to share with me: these photographs were taken before the Schøyen Collection parted with the manuscript. The few pictures available on the website of the Schøyen Collection provide a few further fragments of text. I am thankful to Luciana Furbetta, who sent me a collation of the text of the *Caesares* before I gained access (thanks to Francesco Siri, to whom I am deeply grateful) to a complete reproduction of the manuscript, which fully confirms the deductions I made using only *Carm.* 4–5.

for *volveret* at *Carm.* 4.2, and a problem related to line 292: *C* omits the line, *A* copies it in the margin (through contamination? see below), and all (except *C*) invert *nobis potuit* (without, however, affecting the metre). All four manuscripts are independent from one another; yet a group does emerge, uniting *C* and *A* (see, for instance, *Carm.* 5.40 solio] solo). I do not think that it is possible to identify a parallel group uniting *Sch* and *Vat1661*; in fact, it seems to me rather that *Vat1661* has an older text than the other three, that *Sch* is prior to *C* and *A*, and that *A* was fairly regularly (but not always) corrected using an anterior manuscript of a type similar to *Vat1661*; the arguments are somewhat weak, but here are a few:

*Carm.* 1.5 tuba] tubaque *Sch C A<sup>ac</sup>*

*Carm.* 1.15 m(a)enala] melana *Sch C A<sup>ac</sup>*

*Carm.* 5.599 mitis] mittit *Sch C A*, mittis *Vat1661*

*Carm.* 8.9 et] om. *Sch C A<sup>ac</sup>*

The only place where I could find an error common only to *Vat1661* and *Sch* is inconsequential: *contentu* for *contemptu* at *Carm.* 8.14.

This is a good place to deal with the two *editiones principes*, that of Nicolaes Ketelaer and Geraert van Leempt, published in Utrecht in 1474 at the latest, and that of Giovanni Battista Pio, published in Milan in 1498. The 1474 edition is almost certainly derived from a manuscript of the *Vat1661* type, and in my view actually from *Vat1661* itself:<sup>54</sup> certain apparently minor traces of contamination should probably be attributed to the editors rather than to their model (but see also n. 56 below). Pio's edition is probably derived from its predecessor, though he had access to other manuscripts. One example will suffice: at *Carm.* 7.546, the manuscripts with which we are dealing are distributed as follows:

orbem ego sat potui mihi	magistro <i>Sch C<sup>ac</sup> A</i>
orbem ego sat potui mihi subdere teque	magistro <i>C<sup>ac</sup></i>
orbem ego sat potui mihi	magistro <i>Vat1661</i>

The 1474 edition contains exactly the same text as *Vat1661*, with the gap in the same place, but omits *ergo* (which is indeed metrically incorrect); Pio's edition has *Immo orbes potuit si te tibi tota magistro*, which derives more or less faithfully from other manuscripts. One can confirm that the 1474 edition derived from *Vat1661* (or from a manuscript so close that no one would ever feel the need to postulate its existence) by comparing the strange colophon of the final book of letters, where it contains exactly the same garbled text as *Vat1661*:

Explicit epistolarum liber nonus. bb. kk. Tpmmkk. bqpm. kobstkt. Tkepokk. Fqk.  
*Vat1661*  
 Eplicit (*sic*) epistolarum liber nonus. b.b.k.k.T.p.m.m.k.k.b.q.p.m.m.k.obs.T.K.T.T.K.  
 e.p.o.k.k.f.q.k. *ed. 1474*  
 Explicit epistolarum liber viii<sup>us</sup>. h b k T p m m k k . b q p m m k o b s k t . T k e p o  
 k k . f . ċ k. *Sch*

<sup>54</sup> Or, more likely, through a copy of *Vat1661* made for this purpose. But it is not impossible that *Vat1661* itself could have been lent by the Vatican Library, as was confirmed to me by *scriptor* Antonio Manfredi, for whose advice I am most grateful. Unfortunately, there is no record of any such loan, as there are no surviving records earlier than 1475.

What we have here is a perfectly common numbering method according to which each letter is replaced by the next one in the alphabet. If we decipher *Vat1661* and *Sch*, we get the following:

aa. ii. Sollii. apoll. inarsis. Sidonii. ep<iscop>i *Vat1661*  
gaiSollii. apollinaris. Sidonii. eb<iscop>i *Sch*

Both of them contain mistakes, but in different places; neither of them (nor indeed the 1474 edition) understood what they were copying – which once deciphered is, to say the least, unexciting.

Another manuscript, #39. Paris, BNF, lat. 2168, is related to this family. If the order of the letters were not enough to prove it,<sup>55</sup> it can be demonstrated by a few readings which are shared with *C*, *A*, *Sch*, and *Vat1661*.<sup>56</sup>

*Ep.* 1.2.4 ratione] religione  
*Ep.* 1.4.2 perfectionis] huius enim *add.* (*nonnulli codd. add. cuius*)  
*Ep.* 1.6.2 trepidum] tepidum

Finally, *Avr*<sub>2</sub> is related to the same family (although I do not know whether it should be connected to the archetype of *Vat1661* or to that of *Sch*): this is most clearly demonstrated in the *Caesares*, through two irrefutable examples. In the editions (and the manuscripts) of Ausonius, line 35 appears as *Matricida Nero proprii vim pertulit ensis*. Instead of this, most manuscripts of Sidonius (including *Avr*<sub>1</sub>) contain a piece of historical nonsense (deriving from what is basically a reading error): *Matricida Nero propriorum pertulit enses*; the exceptions are precisely *Sch*, *Vat1661*, and *Avr*<sub>2</sub>, which have a secondary reading that is historically correct, but whose existence is due to the impossibility of the reading in the other manuscripts: *Matricida Nero proprio se perculit ense*. In addition, though line 25 is assigned a number in the editions of Ausonius, it is in fact lost;<sup>57</sup> it is generally omitted without any indication in the manuscripts. However, here too *Sch*, *Vat1661*, and *Avr*<sub>2</sub> stand out by offering a new line: *Interitus dignos vita properante probrosa*. The recension they are derived from may be challenged for its authenticity, but certainly not for its intelligence.

We must now investigate the relationships between the families identified above. At least one mistake in *Carm.* 5 is shared by all the manuscripts (including *Avr*) except *C*, *A*, *Sch*, and *Vat1661*, but it is hidden in all the editions:

87 (Romanaque tecta) Hannibal ante meus quam nostra Scipio vidit  
nostra . . . vidit *transp.* *C A Sch Vat1661* (*vadit Vat1661*)

<sup>55</sup> The inversion of 6–7 and 8–9 in Book 7 is a fairly characteristic feature, but it is dangerous to rely upon it insofar as it *could* be inherited from the *Ur-Archetype* (it scarcely matters whether or not it is mistaken; if it is a traditional element, it cannot be used to determine the existence of a family).

<sup>56</sup> All these readings are absent from the *editiones principes*, which may be taken as an indication that that of 1474 was already affected by contamination.

<sup>57</sup> Its presence is deduced from the fact that the piece containing it must have twelve lines, to match the number of ‘Suetonian’ emperors; yet Ausonius does not necessarily attribute one verse to an emperor. Besides, in this particular passage there should have been a group of three verses (24–6) assigned as a group to Galba, Otho, and Vitellius. The third of these (the verse which is lost) must have been fairly superficial, because the two preceding lines make sufficient sense on their own: *Galba senex, Otho lascive et famose Vitelli, / tertia vos Latium regnantes nesciit aestas* (‘old Galba, playboy Otho, and notorious Vitellius, by the third summer your reigns in Latium were forgotten’). I take this opportunity to draw attention to a mistake common to all the Sidonius manuscripts, not because it is important, but because it is amusing: the corruption of *famose* into *formose*. Vitellius, handsome?

I do not understand how one can justify *nostrā*, because Sidonius would never have allowed such a lengthening before *sc-*, especially without a syntactical rest. In particular, he would not have admitted it while there was a simple enough way to avoid it by transposition. To my eyes,<sup>58</sup> it is evident that *C*, *A*, *Sch*, and *Vat1661* are correct,<sup>59</sup> and consequently that all the other witnesses are descended from a single hyparchetype in which the words, for whatever reason, have been inverted.<sup>60</sup>

One manuscript remains from the list of witnesses quoted above: *Reg203/2*. I do not know exactly what its place is; it was very probably copied using two manuscripts at once, resulting in some inconsistent readings which are due either to the original hand or to corrections. Thus for the readings quoted above at *Carm.* 9.115 and 297, *Reg203/2* agrees at first with the received text, but after correction, with  $\delta$  (that is, with *P*, *L*,  $\kappa$ , etc.). At line 313, it originally had *hoeni*, which was then ‘corrected’ to *hent*; in contrast, it remains faithful to the received text at line 292. Since it does not seem to contain any characteristic readings of *C*, *A*, *Sch*, and *Vat1661* or of the English family, I suppose that one of its models is related to  $\delta$ , and that the other is older – and therefore a relative of the archetype shared by  $\delta$  and the English family. Yet since all of this is far from certain, I omit *Reg203/2* – which is probably of no editorial use anyway – from the stemma.

We must finally take into account the manuscripts which only contain a selection of the poems:

- #15. Florence, BML, plut. 90 *sup.* 8 (*Pl90*)
- #20. Leiden, UB, BPL 121 (*Leid*)
- #21. Leipzig, UB, Rep. I 48 (*Leip<sub>2</sub>*)
- #24. London (Kensington), Westminster Diocesan Archives, 83 (*Westm*)
- #26. Milan, Bibl. Ambr., C 52 *sup.* (*C52sup*)
- #73. Venice, Bibl. naz.le Marc., Lat. II, 63 (2077) (*Ven*)

Of these, *Pl90*, though a copy of *M*, includes on f. 169v fragments of *Carm.* 15, 22, and 2 which are taken from another tradition. *Leip*, originally derived from *v*, recovered the endings of *Carm.* 2 and *Carm.* 21 from a complete manuscript. Finally, *Leid*, *Westm*, *C52sup*, and *Ven* are not related to  $\lambda$  since they do not have its characteristic variants; that is why I include them here even if they only contain *Carm.* 1 and a small part of *Carm.* 2 (*Leid* and *Ven*) or the *Carmina* only up to 5.198 (*C52sup*), or even no *Carmina* at all (*Westm*, the end of which is lost): their model was a manuscript containing all the *Carmina*.

<sup>58</sup> As in those of Gustafsson (1882) 119 and Tiziana Brolli (private communication). Incidentally, I am not certain that *simul* at line 386 (in these four manuscripts, when all the others have *postquam*) is a mistake (or even just an innovation).

<sup>59</sup> I do not know of any other example of the first letters of *Scipio* being used to lengthen a preceding short, which – had it existed – could have been used as a parallel to justify Sidonius’ use here. Note that the lengthened shorts at *Carm.* 7.225 (*gerere*. *Stupet*) and 275 (*nuere*, *strictumque*) correspond to a pause in recitation. Note too that the cases gathered by Housman (1927) 3 = (1972) 1117 from *Carm.* 7 are all lengthenings before a mute and liquid: therefore it might be hypothesised (metricians would need to confirm it by further inquiry) that Sidonius allows the lengthening of a short final before two consonants if there is a syntactical rest or, if not, if these consonants are a mute and liquid. I find proof of this in *Carm.* 22.122, a reference I owe to Müller (1894) 388, who also mentions 23.338 – but I am not sure it can be used to judge the point in dactylic verses. A very exciting study of this type of final shorts is Hoenigswald (1949) but unfortunately it is not pertinent to later authors such as Sidonius.

<sup>60</sup> Some manuscripts attempt to correct by writing *nostras* (notably *M*, but there are others as well); this is obviously a poor solution.

The case of *Leip* is highly unusual: in the final part of *Carm.* 2 (which it could not have inherited through the branch ‘containing the panegyrics’), I was not able to find any mistakes corresponding to either of the identified branches. *Leip* is far from having no mistakes of its own, yet all the evidence suggests that it is descended from the *Ur-Archetyp* without relying on one of the hyparchetypes which I have reconstructed.<sup>61</sup>

Judging by their ordering of the letters, *Leid*, *Westm*, *C52sup*, and *Ven* do not derive from the family of *C*, *A*, *Sch*, and *Vat1661*; by the same token, we may deduce that they do not descend from the English archetype, or indeed – very probably – from  $\kappa$ . But they still have a common archetype whose most distinctive feature was to put *Ep.* 5.18 after 5.20, and I think that *Ven* was copied from *Leid*.<sup>62</sup> In all likelihood, the ancestor of these four manuscripts is a descendant of the hyparchetype on which *P* depends, but I do not have any evidence of this, and have omitted it from the stemma.

Finally, *Pl90* contains very slight fragments of the *Carmina* on f. 169v (besides the corpus which it inherited from its exemplar *M*): *Carm.* 15.136–43 (inverting 137 and 138 and omitting 140) given with the title *Eiusdem reperti versiculi de Hercule breviter et perinde eleganter*; *Carm.* 23.158–61 (*Item de Nasone*), then *Carm.* 2.190–3 and 184–5 (until *tubas*), given with the title *Item alia*.<sup>63</sup> I do not think that these fragments could have come from anywhere but *T*, directly or via one of its descendants.<sup>64</sup>

We thus obtain the stemma in Fig. 16.6. I have left out *Reg203/2*, and *Leid* and its relatives (whose place is uncertain) as well as *Pl90* (for its fragments of the *Carmina minora*), which is in all likelihood derived from *T*.

## 7 Synthesis

The stemma of Sidonius is essentially bipartite, with two branches deriving from the *Ur-Archetyp*. The manuscripts forming the first branch are derived from an ancestor (which we shall call  $\alpha$ ) dating from the eleventh century at the latest, which must have been located somewhere in Aquitaine, even though it has at least one Italian descendant (*Vat1661*). Since this last manuscript is relatively late, it is possible that the archetype, or at least one of its descendants, left Aquitaine for Italy. One could retain *Vat1661*, the highest in the stemma (even if the latest in time), as a representative witness of  $\alpha$ , provided that its readings are checked against *A* before or after correction. Before correction, *A* occupies a fairly low position (yet, in contrast to *C*, it is relatively free from interventions *ex ingenio*); after correction, its situation is comparable

<sup>61</sup> It may be interesting to note in this respect that *Leip*, together with *C* before corrections and *P*, kept the spelling *excidium* (for *excidium*) at *Carm.* 2.350. Naturally, *Leip*’s value lies only in the fact that it was contaminated: in other respects, I think that its text does not have any particular authority; however, if one were, for instance, to conclude (see n. 27 above on the gaps in *L*) that there really is a lacuna in the whole tradition at *Ep.* 1.1.3 where Lütjohann prints *litterulas*, *Leip* would offer an excellent solution with *lucubrationculas*, which is added in the margin.

<sup>62</sup> *Leid* and *Ven* both omit the end of *Ep.* 1.5, 1.11, and 3.3. In *Leid*, at the end of 3.12, another hand has noted in the margin the absence of 3.13, referred to by its first words: *Unice deest hic*. I think *Ven* comes from *Leid* because, at this point of the text, *Ven* does not have the note but changes the end of 3.12 by adding the word ‘unice’; a misunderstanding of the note in *Leid* is the most likely explanation for this innovation.

<sup>63</sup> Incidentally, Crinito, surprisingly for such an erudite scholar, kept *T*’s mistake *flumine* for *fulmine* at *Carm.* 2.191.

<sup>64</sup> *Br2* could be a possible source, yet it seems that the manuscript was no longer in Italy at the time when Crinito was active: he was born in 1475, and Nicolaus Cusanus, the owner of *Br2*, died in 1464, from which date *Br2* must have been kept in St Nikolaus-Hospital at Kues with the rest of the cardinal’s library.



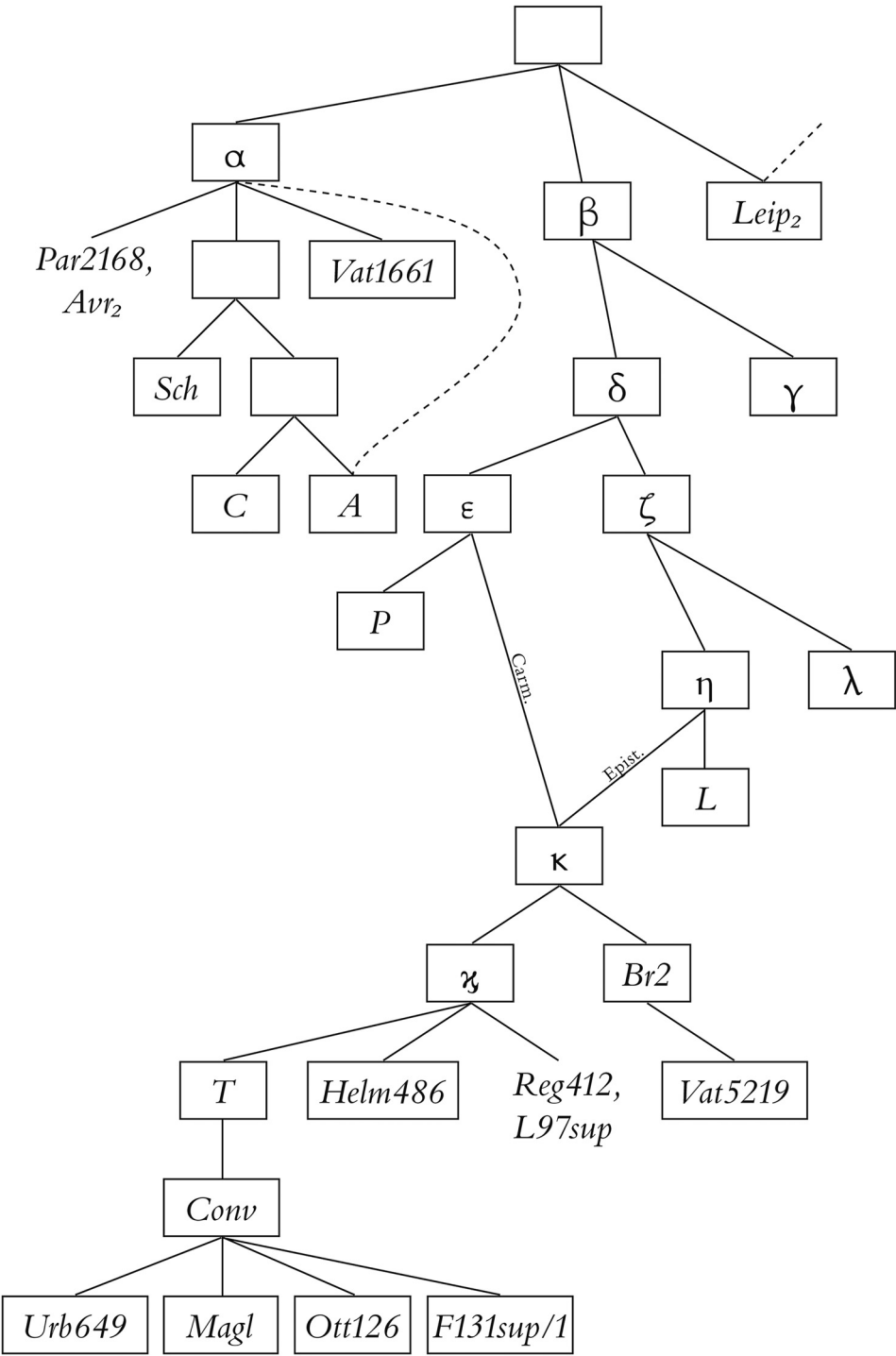


Figure 16.6 Stemma of the complete manuscripts

to that of *Vat1661*.<sup>65</sup> It would also be worth attempting to determine the precise position of *Par2168*: if it turns out to be high, it will have to be collated.

A second branch comprises in practice all the other manuscripts; its archetype may be called  $\beta$ . A first split generates the *Carmina* of the English family, which derives from an archetype which we may call  $\gamma$ . It is unfortunate that, since Lütjohann, *F* has been chosen to represent this family, not so much because it is the group's latest manuscript, but above all because it is the least faithful and, moreover, was contaminated. The best solution would be to use *Roy* instead as the most complete manuscript, provided that its readings are verified using *Auct*, and possibly *Hfd* (for the letters) and *Rawl* (for the *Carmina*), whenever they are likely to be accepted in the text. The date and origin of  $\gamma$  are uncertain: I assume that the manuscript was imported to the British Isles at the time of the Norman Conquest, so in the second half of the eleventh century;<sup>66</sup> yet it was perhaps much older.

The other descendant of  $\beta$  (which we may call  $\delta$ ) generates a hyparchetype  $\epsilon$  which recurs in *P* and, in the case of the *Carmina*, in  $\kappa$ . In addition,  $\delta$  generates a hyparchetype  $\zeta$  which in turn generates the archetype of the 'fourth family' ( $\eta$ ) and that of *M* and the related manuscripts ( $\lambda$ ).  $\eta$  is the source of  $\kappa$  for the *Epistulae*; we need retain only *T* and *Br2* to represent it (bearing in mind that  $\kappa$  is more authoritative for the *Carmina* than for the *Epistulae*). It goes without saying that one cannot eliminate *M*, which offers too many superior readings; that being said, in my view, these readings derive from an extremely intelligent recension rather than from a source which, given the manuscript's content, could not be situated very high.<sup>67</sup> Also descended from  $\lambda$  is the hyparchetype  $\nu$ , which notably generates *Leip* (to be retained not for itself, but for its text after contamination, which may go back to the *Ur-Archetyp*) and, for the letters only, the English family. It is apparently in  $\lambda$  that the corpus of glosses took shape, but it is better transmitted in  $\nu$  than in  $\mu$ .<sup>68</sup>

All this can probably be represented more clearly by the partial and simplified stemma in Fig. 16.7.

Ideally, an editor should collate all the manuscripts on this stemma (at least partially in the case of the lowest ones), keeping in mind what has been said above about *Roy*, which should be checked against one or several other descendants of  $\gamma$ . The same applies for *M*: it would be wise to check its text against one of  $\nu$ 's descendants, and probably also *Br1* for the letters. I do not believe that  $\theta$ 's descendants, such as *V*, *R*, and *N*, are very useful, except perhaps to verify *L*. In this case, *V* seems preferable because it is not contaminated, or else *R*, provided one uses it only in its initial state.

<sup>65</sup> The reason why Lütjohann (and his successors) preferred *C* must be that, at the time, it was the only known witness of Sidonius' epitaph; yet this is an addition which in no way guarantees the manuscript's text. On the epitaph see now Furbetta (2015b), who, incidentally, seems to me to provide definitive arguments to identify *C* with the *codex Cluniacensis* of Sidonius containing the epitaph: if the texts of *C* and *Sch* both derive independently from epigraphic transcripts of the epitaph, it is rather unlikely that there was ever a third manuscript containing exactly the same readings as *C*. The place of *Sch* in the manuscript tradition is also discussed in Furbetta (2014a).

<sup>66</sup> Such is also the opinion of Chronopoulos (2010) 268.

<sup>67</sup> We saw above that *M* and *Avr<sub>1</sub>* had a common model, from which *Br1* is also descended (as we saw for the *Caesares*, and as is confirmed by the order of letters in Book 7). A possible explanation for the fact that *M*'s text is superior to the rest of the tradition could be a second contamination, this time comparable to that of *Leip* and thus originating in a manuscript older than the *Ur-Archetyp*; yet why would *M* not have seized the opportunity to complete its corpus of the *Carmina*?

<sup>68</sup> In fact, unless I am mistaken, *Br1* is the only manuscript outside  $\nu$  to be properly glossed: could it be that its glosses are in fact derived from  $\nu$ , and that *Br1* contains them only through contamination?

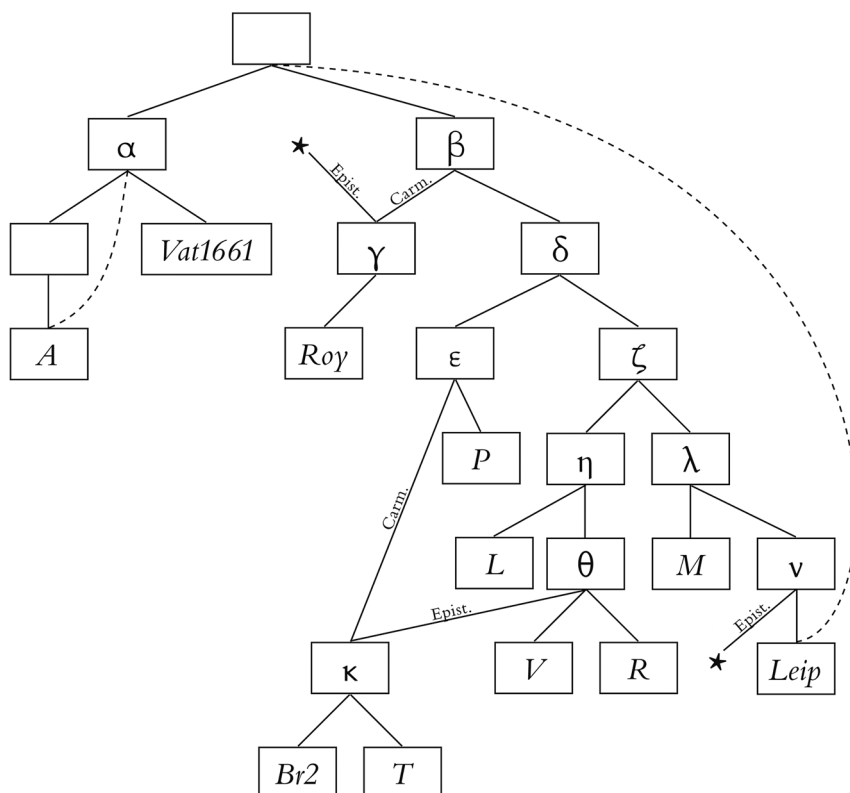


Figure 16.7 General stemma (simplified)

Here is a brief (and therefore considerably simplified) summary of the state of the corpus transmitted by the principal archetypes (unless otherwise specified, the characteristics noted here are transmitted):

- The *Ur-Archetyp* contained the entire works of Sidonius, either in the order given by  $\alpha$ , or in that given by  $\beta$ .<sup>69</sup>
- $\alpha$  inverted letters 6–7 and 8–9 in Book 7.
- $\beta$  inverted *Ep.* 7.10–11 and placed *Carm.* 24 before *Carm.* 22.
- $\gamma$  had the contents of  $v$  for the *Epistulae* (with the inversion of *Ep.* 8.1–2, inherited or not), and of  $\beta$  for the *Carmina*.
- $\delta$  probably omitted *Ep.* 7.6–7.
- $\epsilon$  and  $\zeta$  a priori had the same contents as  $\delta$ .
- $\eta$  had a lacuna between *Ep.* 6.12 and 7.5, omitted (besides 7.6–7) *Ep.* 8.2 and the final words of *Ep.* 7.18. It also omitted *Ep.* 9.1 and lost the end of this book, but it still contained at least part of *Ep.* 9.7 (which is found in  $L$ ).
- $\theta$  deleted what remained of *Ep.* 9.7 (and therefore only contained 2–6 from Book 9) and all the *Carmina*.

- κ inverted *Ep.* 5.2–3 and omitted *Ep.* 3.5–8. It reinserted the *Carmina* (with 16 and then 24 between 21 and 22) after *Ep.* 1.5.
- λ must have had the same content as ζ for the *Epistulae*. It lost the *Carmina minora* and the ending of *Carm.* 2, and placed *Carm.* 8 before *Carm.* 6.
- μ a priori had the same contents as λ but recovered *Ep.* 7.6–7.
- ν inverted *Ep.* 5.12–13; it inserted letters *Ep.* 7.12 and then *Ep.* 6.11 after *Ep.* 7.7; it perhaps inverted *Ep.* 8.1–2.

It will be clear that this stemma is radically different from what has been proposed hitherto. It is fragile and should be better defined in many points, and above all, it should be based on much more extensive collations than those which I was able to carry out myself; yet it seems to me that it opens the way for a finer comprehension of Sidonius' works and their tradition. It has been said that 'Sidonius is in need of explanation rather than emendation';<sup>70</sup> there will doubtless be less to emend and less to explain if the text is established on truly scientific foundations.<sup>71</sup>

Translated from the French by Alexandre Johnston

<sup>69</sup> The authentic order could of course be altogether different – and should in this case be reconstructed through conjecture.

<sup>70</sup> E.H. Warmington's preface to Anderson (1965) 2.xiv.

<sup>71</sup> For a preliminary attempt to apply some of the conclusions of the chapter, see Kelly and van Waarden's Epilogue to this volume.