

OVID'S *AMORES*: THE PRIME SOURCES FOR THE TEXT

I. THE 9TH-CENTURY MSS

Within the increasingly complex picture which has emerged in recent years of the manuscript tradition of Ovid's *Amores* the relationship of the two earliest MSS appears to remain firm: cod. P or Puteaneus (Par. Lat. 8242) of the 9th or early 10th century, which begins at *Am.* 1.2.51, was copied, probably directly, from the second half of the 9th-century cod. R or Regius (Par. Lat. 7311), whose first half now ends at *Am.* 1.2.50. This view, which originates in S. Tafel's dissertation of 1910¹ and lies behind the stemma constructed by E. J. Kenney for his OCT edition of 1961 (p. vi), has come to be taken by Ovidian scholars (with the exception, however, of Munari, who left the question open)² to be the truth. My purpose in this first section is to show that this idea is unlikely to be the truth and, in the form in which it has most strongly been put forward, cannot be the truth. In the second section consequences for the manuscript tradition as a whole are explored.

First we shall need some details.³ P, the slightly later manuscript, consists in all of 99 folia, of which 1–54 contain most, but not all, of the *Heroides* – not all, because they are in a lacunose state, a point to which we shall return in greater detail later. Foll. 55–6 are blank sheets of paper, not parchment, clearly inserted at a much later date during rebinding. The *Amores* (starting at 1.2.51) begin after these sheets and continue to 3.12.26 (foll. 57–96). Foll. 97–8 are two more blank sheets of paper. Fol. 99, the last surviving, contains 56 lines, 3.14.3–3.15.8. P contains 28 lines to the page. It is thus immediately clear that the two sheets of paper at foll. 97–8 mask the loss of just one leaf of the manuscript in its original state containing the 56 lines missing from 3.12.27 to 3.14.2. No space should be allowed for titles to individual elegies in this missing portion of P: the scribe's practice in *Amores* – in contrast to *Heroides*, where he regularly separates poems by an interstice of one line – is to mark a new poem with an enlarged capital letter and to place titles, where he has them, in the margin.⁴ Twelve lines are missing from the end of the book on the page which originally followed fol. 99. It is significant that the leaf now missing before fol. 99 was the first leaf of a new gathering.

The early Caroline minuscule script of R places it a little earlier than P, but not

¹ *Die Ueberlieferungsgeschichte von Ovids Carmina amatoria. Verfolgt bis zum 11. Jahrhundert* (Tübingen, 1910), 26–32.

² *P. Ovidi Nasonis Amores* ed. F. Munari (Florence, 1951), xv–xvi; different pagination in later eds.

³ These have been drawn from the MSS themselves. For earlier descriptions see Tafel, 4–5, and Munari's collations in *SIFC* 23 (1948), 113–152 [= *Kl. Schriften* (Berlin, 1980), 32–71]. I am indebted to Professor Kenney for access to materials used largely in the second part of the article and to Dr Denis O'Brien of the Centre National de la Recherche Scientifique for additional information. Throughout I have gratefully accepted suggestions made by Professors Brink, Kenney, and Reeve.

⁴ On these titles see E. J. Kenney, 'The manuscript tradition of Ovid's *Amores*, *Ars Amatoria*, and *Remedia Amoris*', *CQ* 12 (1962), 7 n. 3. It will be convenient to refer henceforth to this article, which contains much essential information, by page number alone. Roman numerals refer to the same author's OCT preface.

much earlier. This difference finds expression in 9th-century dating for R, 9/10th-century for P. Both hands admit instances of half-uncial *n* and open *a*, while *scriptura continua* lies not far in the background. Neither scribe is a wholly competent divider of words, although P is better than R. The French origin of each hand is clear.⁵ R is now a composite volume: after some 12th-century material on foll. 1–49 of no concern to us here, our 9th-century MS runs from fol. 50 to fol. 103 and contains three works: on foll. 50^r–88^v, *Ars Amatoria*; on foll. 88^v–102^r, *Remedia Amoris*; on foll. 102^v–103^v, *Amores* (epigram, 1.1.3–1.2.19 and 1.2.25–50). The works occupy 6 regular quaternios (foll. 50–97), and then the first 6 leaves only of a seventh (foll. 98–103), of which the last leaf is torn at the bottom. Clearly the *Amores* come to an abrupt halt.

It is this abrupt ending of one MS at just the point where the second begins that has led scholars, not unnaturally, to connect the two. The explanation which, after Kenney's edition, has come to be commonly accepted⁶ is that, since P's different hand and later date mean that it cannot itself be the second half of R, then it must be a copy of the second half of R, made after R had at an early stage been divided into two parts. The important consequence of this is that R is likely to have contained, in addition to the other amatory works of Ovid, the *Heroides* also, following on after the *Amores*. They are, we have seen, in P in the same hand as the *Amores*, and the rebinding of P makes it easy to accommodate the notion that the *Heroides* originally followed the *Amores* in P instead of, as now, preceding them.⁷ Hence the belief in a medieval archetype containing all of Ovid's amatory works, from which were drawn the two hyparchetypes, α (parent of R/P and the 11th-century MSS, S and Y) and β (less reconstructible source of the more numerous 12/13th-century MSS, whose descent separate from α is nevertheless guaranteed by the presence in them of 12 or 14 apparently genuine lines [1.13.11–14 and 2.2.18–27] omitted by α).⁸

It is clear that a great deal has come to depend on the descent of P from R. And yet it has not at any point been subjected to particularly close scrutiny. Tafel advanced his theory with considerable qualifications at every stage;⁹ Kenney, in his edition, refers only briefly to this discussion (p. vi n. 1) and, in his article, after granting that Tafel had explained the relationship between P and R 'with some plausibility, though no certainty' (p. 6), proceeds to accept the conclusions and to build on them.¹⁰ But the theory does have its difficulties, of which a number become apparent as soon as one goes beyond the initial information that R ends at *Am.* 1.2.50 and P begins at *Am.* 1.2.51. The picture is in reality more complex. This is not to say that both MSS do not now each end and begin at these places, but, simply, that such was not always, or need not always have been, the case.

To take R first: the tear on fol. 103 leaves it quite clear, and no one has ever suggested otherwise, that R did not always end at 1.2.50. Its pages each contain 30 lines, and the tear, which runs at something of an angle across the page, has removed 5 lines from the foot of fol. 103^v. It is evident that it removed 5 lines here, because it removed

⁵ Corbie is suggested for P: B. Bischoff, *Mittelalterliche Studien* i (Stuttgart, 1966), 59.

⁶ See, e.g., the summaries by John Barsby, *Ovid Amores I* (Oxford, 1973), 33 and R. J. Tarrant in L. D. Reynolds (ed.), *Texts and Transmission* (Oxford, 1983), 260 (*Am.*), 269 (*Her.*).

⁷ Tafel, 31; Kenney, 7, n. 2.

⁸ The total remains at 12 if two of these lines (2.2.23–4) are not genuine; cf. the other suspect couplet supplied only by the β MSS at 1.13.33–4.

⁹ E.g. (31): 'Die Annahme, dass P aus einem Teil von R abgeschrieben wäre, bleibt also möglich, aber sie ist nicht zu beweisen.'

¹⁰ Stemma on 7, General Conclusions on 24–9.

5 lines also on the other side of the leaf. This explains why the list of R's contents above contained nothing for *Am.* 1.2.20–4. These 5 lines were not omitted by R, but were torn out, as the verso of the leaf (beginning with 1.2.25) makes quite plain. In the same way 5 missing lines are to be allowed for at the foot of 103^v itself. That the text did not come to a halt at the tear on 103^v is sufficiently indicated by line 50 itself, parts of which still survive.¹¹ The 4 lines wholly missing on 103^v are then 51–2 of poem 2 and 1–2 of poem 3.¹² The page as it stands has 25 complete lines (1.2.25–49) before the tear. Now the realisation that the last page of R contained lines beyond 1.2.50 has important consequences for any argument which seeks to connect the copying of P with R. It must take those lines into account and this will in itself determine the shape of that argument, if not infect it with certain difficulties which are in the end ineradicable.

With this in mind let us turn to the one attempt that has been made to consider the basis of the R/P relationship in any detail, which is also the form in which the copying of P from R has been most strongly argued. It occurs in G. P. Goold's discussion of the manuscripts in his monograph 'Amatoria Critica', *HSCP* 69 (1965), 1–107. For once a full picture is given¹³ of what it really means to say that P was copied from the second half of R:

The principal manuscript of the group is R (9th century), which once contained as much as the archetype in its mutilated condition, i.e. *Ars*, *Rem.*, *Am.*, and *Her.* in part. All we have now is half of it. The following is a hypothesis to account for the facts. Years after it was written, the manuscript was broken in half, so that the *Amores* and *Heroides* might be removed. Unfortunately, the *Amores* commenced on a leaf which contained the concluding verses of the *Remedia*; and these the owner of the manuscript was unwilling to part with. Accordingly, the prospective recipient of the latter portion, being anxious to oblige the vendor, agreed to forgo the first poem in the *Amores*. Now R happens to join the first two poems together without advertising the fact, and since the apparent second poem began near the bottom of the verso of a leaf, the purchaser – *uix mihi credetis, sed credite* – deliberately tore off the horizontal strip of paper [*sic*] containing the first few verses of *Am.* 1.3; he tore the page a little higher than the beginning of the poem (so as to be on the safe side); and, as a result, he took a couple of verses at the end of *Am.* 1.2 as well. Consequently R, in which the tear described is plainly to be seen, finishes at *Am.* 1.2.50; and P (which is an apograph of the abstracted portion of R) begins its text of the *Amores* at *Am.* 1.2.51.

Such a hypothesis may well be felt to raise as many questions as it aims to settle. It forms, however, a convenient starting-point and, once its consequences have been examined, we may hope to be in a better position to see what really happened.

To begin with we have to imagine a vendor who wishes to sell off one half of his manuscript¹⁴ and who is willing, or finds it necessary, to have the last page of the portion he retains torn in order to achieve this. In addition, there is a purchaser interested in obtaining the *Am.* and *Her.* of Ovid, but unwilling, or unable, to acquire the same author's *A.A.* or *Rem.* and thereby forced to forgo the first 84 lines (or leaf and a half) of the text he aimed to have – a Shylock, it seems, intent on having his bond of the *Amores* but constrained to spill no drop of *Remedia* blood. Such

¹¹ Of *Parce tuas in me perdere uictor opes*, the tear has left, at the beginning, *Par* and, at the end, *uictor opes*. On the preceding side two letters and the tops of 9 or 10 others survived from line 20, also the top of the capital letter *N* from line 21.

¹² Tafel, 29; Munari, xx; Kenney, 6. There is no possibility (cf. p. 219, above) of allowing, as Kenney suggests, one line for a title, which would take us only to 1.3.1.

¹³ Pp. 3–4.

¹⁴ The case is in fact worse than this: *A.A.* and *Rem.* occupy together some 50 leaves of R; *Am.* and *Her.* each occupy some 50 leaves of P. So the portion for sale was much closer to $\frac{2}{3}$ of the whole.

characters, however, already incredible, soon become impossible when we see how ill the visible effects of their actions square with their supposed motives. If it was aimed to separate *Amores* from *Remedia*, then indeed the position of these works on different sides of the same leaf presented an obstacle, but it made no sense to go on to what seemed to be the first break between poems, in order to leave the first poem intact, if the effect of splitting at that point was, patently, not to achieve this aim. The tear, we have seen, removed 5 lines from the middle of that apparent poem, leaving the first owner with an ugly lacuna from 1.2.20 to 24 and the second with a fragment of 5 meaningless lines for which he had no use. No attempt was made to recopy these lines in the retained first portion, which casts immediate doubt on the vendor's wish to keep at least one poem of the *Amores* whole. In that case, a more obvious solution would have been to break the codex after fol. 102 rather than 103, in order to give the purchaser as much of the *Amores* as possible. The purchaser could then, if he wished, copy out the first side (30 lines) afresh and have the work complete. And further, if the aim of the tear really was to remove just 2 lines from the bottom of 103^v, was it not rather large an overshoot to remove—'to be on the safe side'—not only 2 further lines and most of one other from that side of the page but 5 lines also from the other side of the page? Again, would it not have been a great deal simpler to copy those 2 lines into the margin of the next leaf, i.e. the first page of the new codex, instead of relying on the survival of the tatter torn from the foot of the preceding page? And finally, what reason can we think of for the new owner's failure to add the book-heading to his now untitled and so inelegantly mutilated portion of a codex?

These creatures of fantasy, the vendor and the purchaser, may surely be dismissed with safety from the scene. It was a bad bargain for each of them. And with them too may go one other possibility, that of an owner who split his codex in two, in order to make it more manageable. There is not one of the preceding arguments which does not apply with equal force to him also, and, in any event, a codex of some 150 leaves (even allowing for the presence of *Her.*, on which see below, pp. 228–230) would not be by any means excessive for the time. It is easily matched, among 9th-century MSS, by the Bernensis of Horace (197 foll.), the Oblongus of Lucretius (192 foll.), and the Montepessulanus 362 of Lucan (174 foll.). Many of the 9th-century MSS of Virgil had when complete not less than 200 folia and often more; the 4/5th-century Palatinus had close on 300. Among prose texts, the 9th-century Reginensis of Livy has, what is left of it, 257 folia. The earlier uncial MSS were much bigger.

Internal considerations have, then, made clear what the eye might perhaps have suggested all along, that, however codex R came to be in the condition it is today, it did not arrive in that condition through deliberate intent. The rough and jagged nature of the tear, paying, as it does, no respect to the integrity of individual lines, is very far from the clean excision one would anticipate as the end to a supposed vol. 1 (out of 2) of Ovid's amatory works. The tear is, as it looks, accidental (Plate I). Closer inspection reveals that it extended even to fol. 102, where it removed also some 5–7 letters from the last 4 lines of *Rem.* (later supplied by a hand of the 11/12th century). What the eye suspects, internal evidence therefore confirms, with the result that the notion that P was copied from the second half of R can only now be justified in the light of the *accidental* termination of R at this point. Some allowance was made for this as a possibility by Tafel (p. 30), and Kenney inclined to it more definitely (p. 7 n. 1). Let us now see whether this modification to the idea will itself survive closer examination.

Here the evidence of P becomes important. But, to begin with, a practical implausibility arises with R. For, in order to believe that it was copied by P, we have

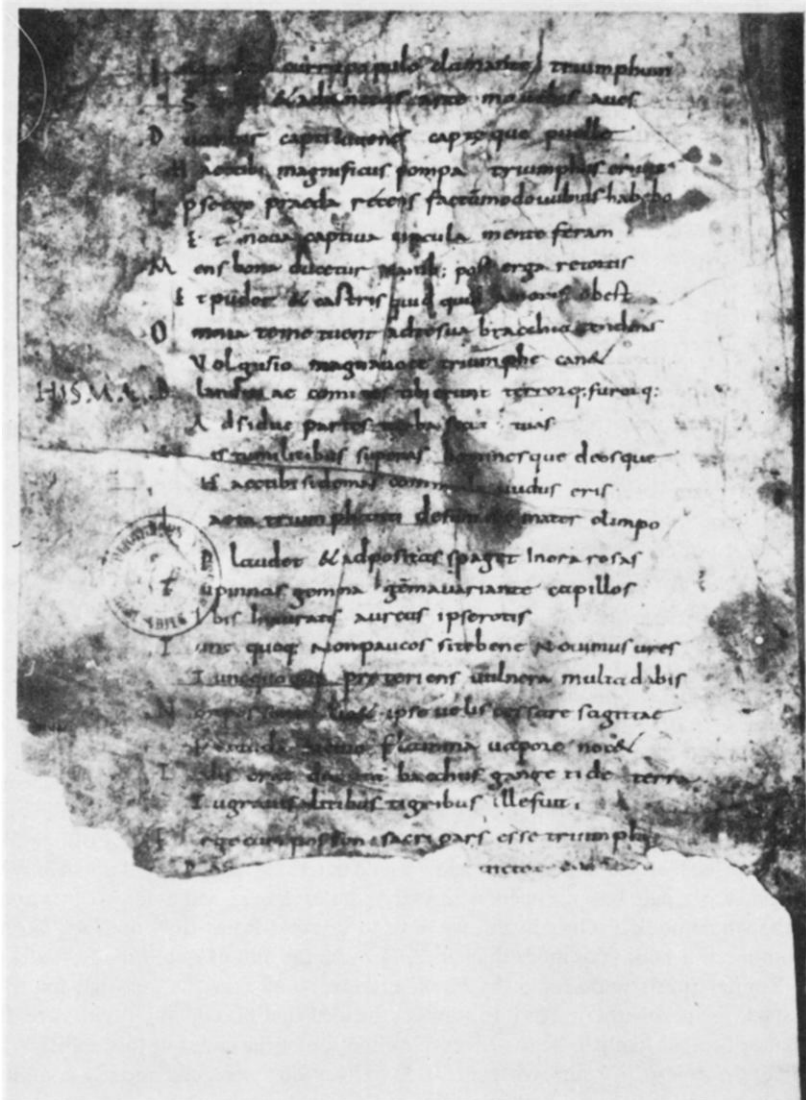


Plate I

to suppose that the piece torn from the bottom of fol. 103 survived loose, so as to come into the hands of P along with the rest of the second portion of R. It can only have been loose, as fol. 103 is the sixth leaf of its quaternio, making it certain that there was no attachment between the torn piece of fol. 103 and any part of the second portion of R. The seventh and eighth leaves of that quaternio must similarly have been loose and yet have survived. Was, then, the torn piece artificially attached to the next leaf or, after the accident, were the 4 lines 1.2.51–1.3.2 (but not the 5 lines 1.2.20–24) recopied onto the head of the next leaf and so preserved to be copied by P? We cannot say, but neither of these actions, taken together with the loose survival of the seventh and eighth leaves, assorts well with the notion of accidental damage, and we start surely with an intrinsic unlikelihood.

What then of P? Here we are concerned with fol. 57, where, after the first set of sheets of blank paper, the *Amores* begin at 1.2.51. But do they? Could it be that P no more begins with 1.2.51 than R ended with 1.2.50? Could it be that with P too we are dealing with a mutilated codex? We have seen already how one leaf containing 56 lines has fallen out between foll. 96 and 99, and there are more of these missing leaves to follow in the *Heroides* section (below, p. 229).

The problem is that to see fol. 57 (Plate II) as the beginning of a new book (that is, P beginning his copying of *Amores* at this point) is to accommodate a number of difficult, if not contradictory, points. First of all, P begins with 1.2.51 on the first of the 28 ruled lines of the page just as if it were the first line of any other new page, but hardly as if it were the first line of a new book. We may readily grant that P inherited no book-heading at this point,¹⁵ but can he really be expected to have begun with no initial space whatever at the head of his work? Would a scribe who is careful to preserve titles to individual poems, beginning just two lines later at poem 3 with '*DE AMICA VOTV(M)*', have despaired of finding a title for the whole work? He may have had no title to hand, but can we really suppose that he would make no allowance for a title? And even if he did make no allowance, is it possible that he would begin a work with an initial letter not in the slightest way different from all the other initial letters which begin every other ordinary line in the book? The *A* of line 51 is quite dwarfed by the *I* immediately below which marks the beginning of poem 3. What medieval scribe ever began a poem with a larger letter than he had used two lines above for the whole book?¹⁶ Once again, we have to say that P just does not *look like* the beginning of a book any more than R *looked like* the end of one. Finally, Tafel (p. 30), Kenney (p. 6), and Goold (by implication, p. 4) all take the fact that fol. 57 is the start of a new gathering in P to support the idea that his copying began here. But is that not just as likely to be the effect of mutilation of the codex at this point? What is left of the *Amores* in P survives from fol. 57 to fol. 96 in five regular and self-contained quaternios. The next leaf (i.e. the first leaf of the next quaternio) is missing; then only the following leaf, fol. 99, survives. If the work suffered mutilation at the join of its final surviving quaternio, can we rule out the possibility that it also suffered mutilation before the join of the first? We should note that this is precisely what happened with the *Heroides* in P. They begin, clearly in mid-stream (no enlarged capital letter etc.), with the pentameter 2.14. It has never been claimed that this was P's starting-point for the work. Perhaps such a claim might have been made before now, only in this

¹⁵ The inscription '*Ex libro primo Amorum finis elegiae secundae*' is patently recent.

¹⁶ The chances that P set out to reproduce the appearance of these lines just as they survived in the torn (or recopied) portion of R seem too remote to take into account and would still do nothing to explain his placing of them at the very top of the page.

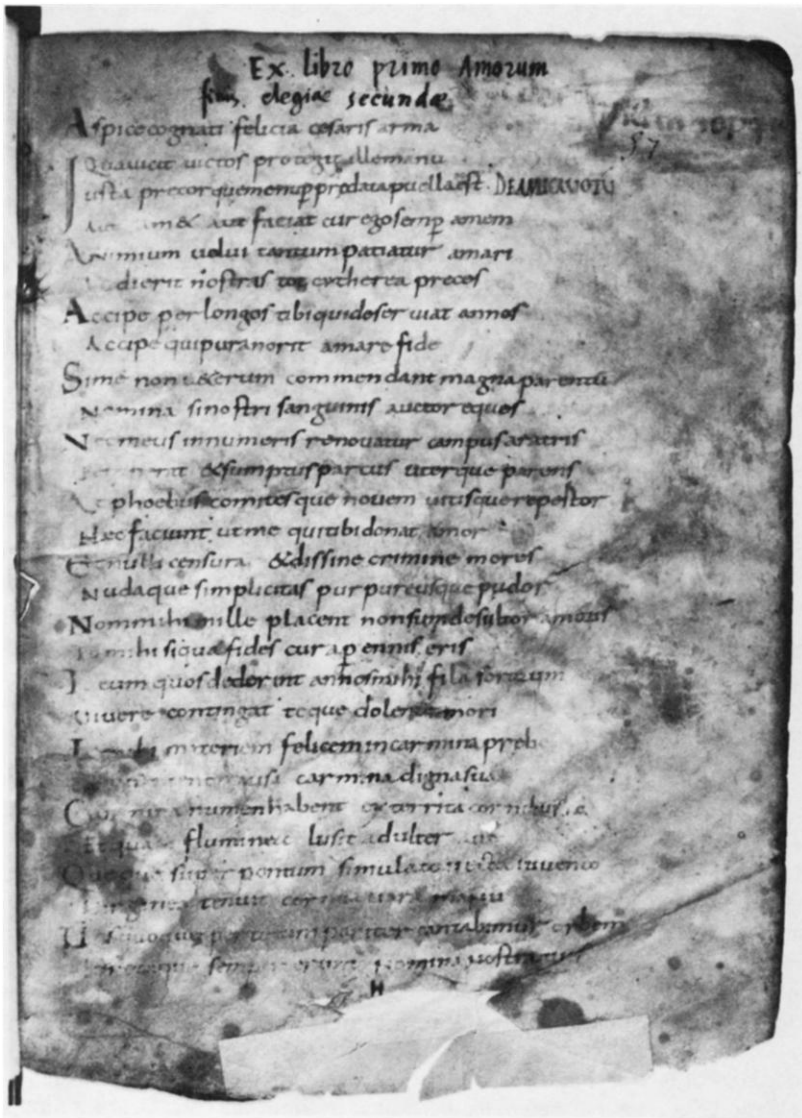


Plate II

case it can be proved that at least one leaf is missing (below, p. 229). In fact there is clear evidence that P survived for long in a disbound state: every first side of a new gathering and last side of the old is badly worn, in many cases severely affecting legibility. In two cases (foll. 29, 96) the leaves themselves are torn. Before being rebound with the insertion of paper leaves to cover obvious gaps, the disintegrated P must have been subject as much to the loss of whole gatherings as it was to the damage of those gatherings which do survive.

One possible objection remains, however: since we are now positing a lost first portion of P containing the first 84 lines of the *Amores* (and, possibly, *A.A.* and *Rem.* also, but see p. 230 below), are we not, since P contains 28 lines to the page, merely transferring back three whole pages some of the same problem of accommodating the book-heading? For there is no room, on this reckoning, for the book-heading at the top of P's text of the *Amores*. The problem, however, is illusory. Modern and medieval practice do not correspond in this matter. However accustomed we have become to different works, chapters, and even poems beginning on a fresh page, this was not the medieval convention. The plates of Chatelain's *Paléographie* are full of examples of new works beginning on a page at whatever point an earlier one leaves off. The *Incipit* of one work follows hard on the *Explicit* of its predecessor, often leaving room at the foot of the page for only two lines of the new text and even only for one. There are no fewer than twelve instances where no space at all has been left.¹⁷ There is no reason why this should not also have happened in P. The same process is to be seen in R, where *Rem.* begins on the same page (88^v) as *A.A.* ends. Thus P will have contained the *Explicit* to *Rem.* together with the *Incipit* to *Am.* at the foot of leaf 7^r of the quaternio before the one we now possess. Very likely it followed closely the form shown by R, but without the two errors of R: 'P. OVIDI NASONIS LIBER PRIMVS REMEDIORVM | EXPLICIT. INCIPIT EIVSDEM ANIMORVM | LIBER PRIMVS EPIGRAMMA IPSIVS'.¹⁸

Let us now take stock. We have argued from both sides against the copying of P from the second part of R: first from the ending of R at 1.2.50, which cannot be deliberate, and second from the beginning of P at 1.2.51, which seems unlikely to have been the original beginning of that MS. Between these no room is left for the theory that P is a copy of the second part of R.¹⁹ We are left, then, with the near coincidence of one mutilated MS ending at the point – in fact the lacerated line 50 prevents the coincidence from being exact – where a second independently mutilated MS now begins. The origin of the theory we have just examined lay in the feeling, as yet untested, that it was too great a coincidence for P not to have been copied from the second part of R. But that theory has turned out to contain too many difficulties of its own to be correct and we are led back once again to the coincidence. In a sense

¹⁷ See, for example, Plate LXXXIV, where fol. 32^r of the 10th-century cod. *ϕ* of Horace (Par. Lat. 7974) ends with 'Q. HORATII FLACCI CARMINV. LIB. II. / EXPL. INCIPIT. III. PRAGMATICÆ.' The other instances cover prose texts as well as verse.

¹⁸ On these errors, see further pp. 233–4. There is no need to suggest, as an alternative solution, that P, like R, omitted lines 1–2 of poem 1, which would reduce the amount of text before 1.2.51 to 82 lines and so leave two lines free for the book-heading at the top of the first page. The other MSS descended from the *α* parent, Y and S, both contain the couplet and, for what it is worth, R uses three lines rather than two for the heading. The only editor of the *Amores* to have made allowance for the loss of the first three pages in P is Munari, *SIFC* 23 (1948), 115, = *Kl. Schriften*, 34; cf. edition, xvi n. 6. But no mention is made of the book-heading.

¹⁹ Tafel's fall-back position (p. 31), that R and P were each copied from the two parts of an exemplar divided, deliberately or accidentally, after 1.2.50 is untenable for all of the same reasons. The damage was in any case clearly sustained by R itself.

it is more misfortune than it is coincidence, in that the accidental tearing of one MS coupled with the disintegration of the second has deprived us of one half of each of the two most valuable witnesses to Ovid's amatory poems. It is our good fortune, though, that it was not the same half. Nor should we seek to rule out coincidence simply on the grounds that it does not often occur. It is in the very nature of coincidence that at times it does occur. Here is another:

Codex S (Sangallensis 864), whose 11th-century script places it, together with Y (Hamiltonensis 471), also of the 11th century, next in date after P, begins its p. 352 with precisely this same line, 1.2.51, which we have seen at the head of fol. 57 in P. And, as if this were not enough, codex Y begins its fol. 39^v with, again, precisely the same line. Too much, you may say, for coincidence: a common cause must lie behind this placing. But there is no such cause; the origin of the coincidence is easily shown to be entirely mechanical: S (42, sometimes 43, lines per page) begins p. 350 with space for the book-heading, followed by 42 lines of text; p. 351 contains 42 more lines: thus 84 lines of text have been copied in total, and 1.2.51 heads the next page. Y (40 lines per page) begins the *Amores* at the foot of fol. 38^r with the book-heading and the epigram (4 lines); 38^v contains 40 lines of text; so too does 39^r: again 84 lines in total and 1.2.51 heads the next page. No plan lies behind this – the MSS have different numbers of lines per page and the *Amores* begin at different positions on the page – it just happens. And yet, if codex S and codex Y had been torn or copied in such a way as to begin their evidence at what are now p. 352 and fol. 38^v, it would surely be held without the slightest doubt that codices P, S, and Y together derive their text from a single parent, and that that parent was the lost second part of R, whose first part now ends at 1.2.50. It is greatly to be doubted whether any attempt to shake so cast-iron a conviction would succeed. And yet such an attempt would, as we see from the evidence we do have, be justified. Is coincidence then to be ruled out in the case of just one of these MSS for no better reason than a failure to take into account that its earlier pages seem most likely to be missing?

What then is the proper relationship of P to R? Two possibilities in theory exist: (1) That P, while not copied from the second part of R, was copied, when complete, from the whole of R when complete. Paradoxically it is easier to believe that the whole of P was copied from the whole of R than it is to believe that half of P was copied from half of R. Since what we have of P at no point overlaps with what remains of R, this hypothetical possibility is created, no more provable, though, than it is disprovable.

(2) The other, and surely more likely, solution is that R and P derive independently from the same source. It may be relevant that P, in his section, shows fewer examples of mistaken word-division than R does in his. This was taken by Kenney (p. 7) as an argument pointing to the copying of P from the second part of R, but, if anything, it points the other way: rather than allowing for the extra stage by which P corrects false divisions of R as he copies, it is easier to regard P as a more competent divider than R of the survivals of *scriptura continua* found by both R and P in their shared source. Whether R and P are each to be seen as deriving without intermediary from this source is a question we are not in a position to answer. But the French script and close dating of the two make it a distinct possibility.²⁰ Within the context of this

²⁰ It is in the end impossible to discover with sufficient precision exactly how many years may separate the two. It is worth recalling that the received 9th-century dating for R and 9/10th-century for P is only an approximation, designed perhaps to facilitate the derivation of P from the second part of R. Bischoff (above, n. 5) identifies P as one of the Corbie MSS associated

similarity an interesting difference perhaps develops between R and P when Y is brought into the picture. In *Philologus* 110 (1966), 270 Gerhard Perl made the suggestion that the readings of R and P, when seen in the more complete reflection provided by the newly re-emerged Y, would not be close enough to allow P to be directly dependent on R: Y may reflect the readings of P in *Am.* somewhat more closely than it reflects those of R in *A.A.* and *Rem.* If this could be shown (and it is difficult to be sure of an accurate standard of measurement), it would provide some useful, if distant, corroboration of the separate descent of R and P: we would not expect such a difference to emerge had P been in any way dependent on R.

The result so far is that the one 'closed' part of Kenney's stemma, showing P to be derived from the second part of R, is removed. R, P, S, and Y must each be depicted as deriving from *a* in a less determinate – or more 'open' – fashion.²¹ In addition, the need to believe in the mutilation of R in the at best narrow time-span before the copying of P is obviated. It is now possible for R to have suffered mutilation at any time before its rebinding. In this it satisfactorily mirrors what we have already (p. 226) seen to be the case with P. In a curious way this more 'open' position confirms possibilities originally proposed in outline by Merkel in the preface (p. vi) to his Teubner edition of 1852; an outline which unhappily disappeared from view in Ehwald's revision of the work and opened the way more freely for Tafel's hypothesis, embraced by Kenney. The hope must be that, with this earlier notion of the 9th-century MSS restored, further improvement will be possible in our conception of the *Amores* tradition as a whole, where we are fortunate in having a far fuller understanding now than had been reached then.

II. HYPARCHETYPES *a* AND *β* AND THE QUESTION OF AN ARCHETYPE

1. *Hyparchetype a as a collected amatory edition*

Among the questions which lead out of the reassessment of the relationship of P to R there is first the point which arose early in the previous section, whether the presence of *Heroides* as well as *Amores* in P points to a collection of Ovid's amatory works made earlier in the tradition and now represented by the contents of R and P combined. Hitherto the argument has run that, since *Her.* were copied in P by the same hand as *Am.*, both works are likely to have been drawn by P from the same source, the second half of R.²² Now we need to ask whether both works were drawn by him from *a*, the common parent of R and P.²³

If we look first, however, for signs that the two traditions had been joined before the *a* stage, the evidence is unclear. The text of *Her.* has come down to us in a radically different state of preservation from that of *A. A.*, *Rem.*, *Am.*, so that the notion of joint survival of the traditions since antiquity²⁴ must now be discarded.²⁵ But it

with Hadoard and thus to be placed securely within the 9th century (at most not later than the third quarter). R and P would then appear to be closely contemporary descendants of the same source.

²¹ Cf. the remark (ix) '*Recensionem habemus, ut Pasqualiano more loquar, apertam*'.

²² Tafel, 31–2, Kenney, 7, 24, placing *Her.* in hyparchetype *a*; Goold, 3–4, placing them also in an archetype, *Ω*.

²³ Note that Kenney (p. 7) allows for an intermediary (*ρ*) between R and *a*, to accommodate the stemmatic requirements of *A.A.* and *Rem.* For the purposes of *Amores*, however, this MS and *a* come to one and the same thing and only *a* will be referred to as source of R and P.

²⁴ Advanced by Lucian Müller, *De Re Metrica*¹ (1861), 43ff.

²⁵ As it is, for example, in the extensive, if parlous, reconstruction of an early archetypal stage

remains possible that a medieval archetype of *A.A.*, *Rem.*, *Am.* could still have added *Her.* This is unlikely in itself to have been ruled out by Dörrie's uncertain assertion²⁶ of the unity of the *Her.* tradition, which led Kenney to conclude (p. 24, with n. 1) that the *Her.*, apparently lacking an equivalent of the second, β , branch of the other amatory works, could not have been drawn from the same archetype as them, so that α was first to join the two traditions. In fact 'linear depravation'²⁷ of P's text seems unlikely to be true as a description of the later *Her.* MSS, and a stemma not far removed in shape from that of the other works seems more probable. All the same, if α can be shown not to have included the *Her.*, then once again it becomes unlikely that an archetype of *A.A.*, *Rem.*, *Am.* contained them either. The chances of independent amalgamation of the traditions within a short time by both P and an archetype, though not impossible, must be remote.

In fact the evidence that α contained both traditions is thin, and amounts to no more than their appearance together in the same hand in P. There is nothing to show that it was not P himself who put *Her.* and *Am.* together, and difficulties, again of a practical nature, do indeed point to this solution. For, as we saw above, both Tafel and Kenney rely, in order to support the idea that P drew *Her.* from the same source as *Am.*, on the assumption that the original order *Am./Her.* was later reversed in P during rebinding to produce the present order *Her./Am.* This assumption is equally necessary whether one looks, as they did, for P's source in the missing second part of R or whether one looks for it in the source shared by P and R. For, in either event, R and, no less importantly, Y both lead off with *A.A.*, followed without gaps by *Rem.* and *Am.*, leaving room for *Her.* only after *Am.*, not before. But *Her.* cannot ever have followed *Am.* in P. To discover this, one need only, as Dörrie has indicated,²⁸ calculate the arithmetic of the matter:

Her. begin in P in their lacunose state at 2.14. This was not the first leaf of that gathering, because there are now only six leaves of it extant and the loss of 56 lines (4.48–103) after fol. 6 shows that first and last leaves of the quaternio have disappeared together.²⁹ The 56 lines contained on the missing first leaf will have been 1.75–116, an interstice of one line before poem 2, and 2.1–13. The first 74 lines of text will then have occupied the last leaf of the previous quaternio plus 18 lines of the side before that. Some allowance should also be made for the book-heading. The straightforward effect of this is that *Her.* cannot fit in with *Am.*, as they stand in P. We have seen already (p. 219) how the first leaf of the last quaternio of *Am.* (containing 3.12.27–14.2) is missing, and the single fol. 99 contains 3.14.3–15.8, leaving just 12 further lines (3.15.9–20) to run on the missing third leaf of that quaternio. Two leaves and 12 lines from the end of *Am.* plus one leaf and 18 lines from the beginning of *Her.* do not make a gathering. Try as one will, one can conceive of no way in which these works were once conjoined in this order.³⁰

of the Ovidian corpus by Georg Luck, *Untersuchungen zur Textgeschichte Ovids* (Heidelberg, 1969).

²⁶ H. Dörrie, 'Untersuchungen zur Überlieferungsgeschichte von Ovids *Epistulae Heroidum* Teil I, *Nachr. Akad. Wiss. Gött.*, Phil.-Hist. Kl. 1960, 124ff.

²⁷ *Gnomon* 33 (1961), 479.

²⁸ Pp. 186–8.

²⁹ There is a similar loss of a double leaf in the next quaternio, this time of the two middle leaves, where 5.97–6.49 (112 lines) are missing between foll. 9 and 10. There are 111 lines of text missing together with an interstice of one line between poems, P's regular practice in *Her.*

³⁰ Even if a smaller gathering of 4 leaves (*Am.* and *Her.* occupying different sides of the same leaf) or a gap of 3 complete leaves between the two works is posited (which might in itself suggest a separate source), it is difficult to see how the first 18 lines of *Her.* came to begin some way

Since, then, *Her.* did not originally follow *Am.* in P, and since *a* can have included *Her.* only after *A.A.*, *Rem.*, *Am.* and not before, a source for *Her.* other than *a* must be sought. Thus P rather than *a* will have joined in one volume – in all probability for the first time – the separate traditions of *Her.* and *Am.*;³¹ the notion of the hyparchetype as collected amatory edition disappears, and, with it, any residual belief that *Her.* might also once have been contained in the now missing part of R.³²

Little more can be added to this about the nature of hyparchetype *a*, but Kenney's suggestion that it too was written in French minuscule script and was thus not greatly older than R and P appears very likely to be correct. The Spanish symptoms detected by Tafel which led to the theory that a medieval archetype was brought from Spain by Theodulph of Orléans and may therefore have come originally from the library of Isidore turn out to be insufficiently founded, while closer study of Y in relation to P is needed in order to determine whether Oliver, following Lenz, is right in discovering signs of a Beneventan period in the earlier tradition. As to the reconstruction of P itself, it is equally clear that, while *Her.* cannot have followed *Am.* in P, which is sufficient to rule out their presence in *a*, they did not immediately precede *Am.* either. Similar difficulties in attaching *Her.* to the beginning of *Am.* (at any of the three points where other MSS of *Her.* leave off),³³ together with the obvious indication that *Her.* will not, except after another work, have begun on the verso of a leaf, make it clear that P must originally have contained other works in addition to *Her.* and *Am.* (in whichever order these two works once stood). Whether *A.A.* and *Rem.* were included, we have no guarantee, while speculation about still further works is fruitless.³⁴ Of P we have now simply *Her.* and *Am.* in that order, each surviving in more or less complete gatherings and each lacking those parts at beginning and end which ran by a leaf or two onto other gatherings. It looks as if *Her.* and *Am.* alone were saved when the codex began to disintegrate in later centuries and when the first and last leaf of each of its now loose or disbound gatherings suffered the extensive wear and tear observed above (p. 226). If, under such circumstances, an attempt was made to preserve single leaves of *Her.* and *Am.* from the incomplete gatherings, it seems doomed to failure and only the isolated fol. 99 survives in this way. The later rebinding, with paper leaves marking just two of the gaps, preserves for us all that was left of an enterprising experiment in joining the two works, otherwise not seen together before the much larger Ovid collections of three to four centuries later.³⁵

down on the verso of a leaf. Another work to fill this gap (if not also one or more complete gatherings in between) has to be postulated (below, with n. 34).

³¹ A conclusion happily congruent with identification of P as one of the classical texts assembled by Hadoard at Corbie (above, n. 20).

³² Y, it should be noted, is also mutilated at the end, having lost its final two leaves containing the last 4 lines of *Am.*, which were later replaced in the 12th century. Munari, *Il Codice Hamilton 471 di Ovidio* (Rome, 1965), 12 n. 4, while preferring to believe that fol. 70 and the remainder of fol. 69 were blank (hence, most probably, the reason for their loss), allows for the possibility that another work may have followed, but this, together with R. P. Oliver's suggestion, *Studies Presented to Ben Edwin Perry* (Illinois, 1969), 163, that the work was *Her.*, now becomes less likely.

³³ Dörrie, 187.

³⁴ In case Ovid's 'paruus libellus', the *Medicamina Faciei Femineae*, should be suggested for inclusion between *Am.* and *Her.* on the specious grounds that *a* appears to have contained 25 lines to the page (Kenney, 24) and that 100 lines, or one bifolium of *a*, are all that survive of the work, it should be said that (1) the tradition of the work when it surfaces in MSS of the 11th century and later bears no relation to our own, and (2) that this is to divorce the work from the spurious *Nux*, by which it is almost ubiquitously accompanied.

³⁵ In these, as can be seen from Kenney's list (3–6), *Her.* occur in conjunction with various works by Ovid in widely differing orders. The order *A.A.*, *Rem.*, *Am.*, though it occurs in 3 MSS

2. The β tradition

When the 12/13th-century *recentiores* are brought into the picture we are presented with two altogether new problems. First, though the presence in them of genuine lines omitted by α shows that these MSS stem from a stage in the tradition separate from α 's divergence,³⁶ they point only with difficulty, if at all, to a hyparchetype which is to be set against α . To a large degree this must be the inevitable result of the widespread crossing of readings between MSS of Ovid in these centuries, which is more than sufficient to obscure the outline of any hyparchetype lying behind them, but it does leave the fundamental question entirely open whether a single source or more than one source lies behind the β MSS. Second, a sharp chronological division arises between our MSS, by which, if a MS was copied in the 9th to 11th centuries, it descends from the α hyparchetype, but, if it was copied in the 12th or 13th centuries, it belongs instead to the second, β , branch.³⁷ This is not what one would have expected: the tradition is not a closed one in which the β MSS descend from an α MS, nor can we assign all or most of the good readings characteristic of β MSS to conjectural activity of the 12/13th centuries. This is a pattern for which parallels can to some extent be found in other traditions, but so clear-cut a division here remains puzzling. Why does no β MS survive from before the 12th century? The problem may be in part an illusion brought about by the paucity of evidence available: who is to say that, if more MSS had survived from the 9–11th centuries, some would not have come from the β side? But the nature of the β side itself would have to become clearer before this could be accepted as an answer, which again returns us to the problem of the source of the β tradition, from which this second aspect cannot be separated. Both questions must be considered together.

The problem of contamination apart, the greatest difficulty presented by the β MSS is that, assembling as they do their different works of Ovid in different combinations, they deprive us of any chance of detecting a source behind them which contained (like α) the specific works *A.A.*, *Rem.*, *Am.* Kenney is at pains therefore to make clear that the siglum β represents for him not a single MS but the 'convenient fiction' (p. 25) of a single MS containing these works and serving as the source for their inclusion in the wider 12/13th-century tradition of Ovid. More recently, however, he has substituted the label 'non- α ' for β , preferring a less precise term to one that may be 'misleadingly simple'.³⁸ The question, as he acknowledges, goes beyond mere nomenclature. But is the new term an improvement on the old? For, if no commitment is to be entered into as to what group or groups the later MSS form, it is difficult to see how the whole emphasis will not inevitably be shifted back to the α side by the term 'non- α ', which cannot fail to introduce a pejorative note. The real test comes when the text of any one of the three works is to be established: the editor, as both Munari and Kenney show in the *Amores*, is in practice forced to look behind the 12/13th-century MSS to a source more substantial than is allowed for in 'non- α '. Even if the outlines of their class are unclear, the β MSS do after all stand for more than a residue and, as Kenney himself made clear (p. 9), any MS on the β side, late and amorphous though that side may be, remains for the editor a 'presumptive variant carrier'. It becomes difficult in the end to say where overall the mainstream of the

and one florilegium (F, P_a, W; p₂), seems by that time to be quite random and retains nothing sacrosanct about it.

³⁶ Above, p. 220; Kenney, 9.

³⁷ The 14/15th-century MSS are too interpolated to have any textual significance.

³⁸ *The Classical Text* (Berkeley, 1974), 134.

tradition lies: in the chance survival of earlier, related, MSS or in later MSS now too contaminated for their relations to be traced. Certainly it lies in these later MSS when the earlier ones concur in omitting apparently genuine Ovidian lines, and in practice no editor of the *Amores*, for all his adherence to one MS or one editorial system, has ever prevented himself from adopting the best reading wherever in the tradition it may be found. Clearly there are times when the α MSS are as much 'non- β ' as, at others, the β MSS are 'non- α '. It remains that the bipartite classification arrived at by editors when confronted with the problems of the separate texts, however much it has had to be surmised rather than securely established, still seems to provide the most practically satisfying view of the tradition.

Why should this be? Is it possible that the surmise satisfies because it rests on firmer foundations than have so far been identified? This we shall now examine, recognising that the problem of establishing a single source for all three texts cannot be overcome, but hopeful that for one text at least (the *Amores*) a start can be made. We return first to the second aspect of the β problem, the question why it is that no β representative survives from before the 12th century.

An inroad is made into the problem by codex S. We have in R and P two close survivors of the early-9th-century French hyparchetype α and in Y an 11th-century descendant of the same MS copied, Munari believes, in Italy, but S (containing, of Ovid, only *Am.* and a small fragment of *Met.* 3), to which Chatelain attributed a German origin,³⁹ presents the strange case of a MS lacking the 14 lines whose omission does so much to establish the existence of α , while at the same time containing in its text readings which have clearly infiltrated at an earlier stage from the β side, as they cannot be sufficiently accounted for by the theoretical possibility of double readings in α and, in any case, no trace of such β readings turns up in the original text of R, P, or Y.⁴⁰ It is puzzling, then, that the omitted lines have not been supplied in S or a predecessor of S. They must either have failed to be drawn from the β side at the time when the β readings were brought over or, having been brought over, they were not incorporated into the text by the next copyist and so, in a sense, were omitted again.⁴¹ S, presupposing as it does one or more intermediate links (σ) between itself and α ,⁴² does at least provide evidence for activity on the β side in the period before the 12/13th centuries. But at the same time such activity, together with the apparently now widespread diffusion of the text, leaves it surprising that we still lack any more specific evidence for a β MS in this period. As it stands, however, the position does suggest the possibility that a β MS (itself an older MS or a copy of one) came to light near the beginning of the 11th century – to put it no further back than the evidence requires –, disseminated readings to σ , and then, most importantly, fathered the β side in the rapid proliferation of MSS demanded by the 12th and 13th centuries, the '*aetas Ovidiana*', a time when even (to judge from the additions of later hands) the now battered MSS R and P were called back into service.

But does any evidence exist for such a MS? A great difference would be made if evidence could be found. But, to carry weight, it would need, like the omission of lines on the α side, to consist of a clear error made by one scribe and affecting the whole of the β side, and yet be one from which the α MSS were free, showing that the error

³⁹ *PCL*, pl. XCI.

⁴⁰ Which much not be taken to preclude the possibility of double readings higher up in the tradition (below, p. 237).

⁴¹ For which evidence emerges below, n. 53.

⁴² Munari, edition, xvi; Kenney, 8.

had occurred at a stage after the α stream had diverged. Nor could it be anything less than a clear error, where the chance is ruled out of α having corrected it for himself.

Unfortunately contamination has made sure that absolute agreement among the β MSS is no longer to be found. The best example under the circumstances is the obviously false *non* for *nos* in line 3 of the epigram, but even here one β MS (D) sides with α and, with the change of only one letter involved, there can be no final guarantee either that α has not corrected the error or that a single scribe introduced the error on the β side rather than different scribes erring independently. In the end there is only one error, fortunately a large one, which is sufficient to meet the requirements in full. Despite Munari's reports in his apparatus and R. P. Oliver's article 'The text of Ovid's *Amores*' published in 1969,⁴³ little attention has been paid to the curious fact that none of the scribes of the β MSS has a proper idea of what the title is of the work he is copying. Or rather the β scribes do have a title to give, but the title is this: '*Ouidius sine titulo*'. In its regular form it appears as '*Incipit ovidius sine titulo*' at the head of the work and is usually matched by '*Explicit ovidius sine titulo*' at the end.⁴⁴ Even codex D, with all its α influence, has '*Ouidii nasonis liber sine titulo incipit*' and '*Ouidii liber sine titulo explicit*'. A fine variant appears in a number of MSS: '*Ouidii liber de sine titulo*'.⁴⁵ Others are blank, or have different forms,⁴⁶ or the false title has later been altered to a more correct form,⁴⁷ or, which comes to the same thing, the title is added by a later hand in the space originally left blank.⁴⁸ The error has every sign of being significant. If the scribes of the 12/13th centuries did not know the proper title to the work, this can only mean that, at some point before they wrote, the title had been lost. And, if all the β scribes are ignorant of the correct title, then an answer is given to the first question asked of the β side, in that one rather than a number of MSS is pointed to at the head of the β tradition, as the correct title can scarcely have been lost independently in a number of MSS separately responsible for engendering the β side.

First, however, in order to establish that the error affects only the β side of the tradition, it is necessary to be sure that the correct title originally stood in α . This is easily shown. R, we have seen earlier (p. 226), presents the work as '*ANIMORVM LIBER*'. Up to a point it is of no importance whether this misreading was his own or was made in α ; the fact is that it preserves the truth, from which it is perhaps separated only by the small leap – from *AMORVM* to *ANIMORVM* – of a pious mind.⁴⁹ Difficulty with a title in capital script is, however, shown elsewhere by R,⁵⁰ and, despite the absence of P through mutilation, here too the finger is pointed conclusively to error on R's part alone by Y – again proving the value of Y – when he writes (fol. 37v) '*P. OVIDI NASONIS REMEDIORVM(M) EXPLIC(IT). LIB(ER). PRIMVS. INCIP(IT) EIVSDE(M) AMORV(M) LIB.I. EPIGRAM(M)A IPSIVS.*'

⁴³ Reference given above, n. 32.

⁴⁴ MSS P_{as}, P_h, P_r (*Explicit* only), O_h, X, Z.

⁴⁵ A_h, P_h, V_{as}, W.

⁴⁶ H: '*Incipit ille liber cuius non nomen habetur*'.

⁴⁷ '*Incipit ovidius sine titulo*' F; '*Incipit ovidius amorum*' I².

⁴⁸ N (ut uid.): '*Liber ovidii amorum siue de sine titulo*'; T: '*Liber ovidii sine titulo qui uocatur amorum*', conflations of correct with incorrect title redolent of the 14/15th-century interpolated MSS.

⁴⁹ NI for M is a constant misreading in capital script. The scribe thought he saw *ANIORM* and adjusted accordingly. False reading of an abbreviation would not explain the error.

⁵⁰ Fol. 75r: '*P. OVIDI NASONIS ARTISSIMATORIAE/LIBER II EXPLICIT. INCIPIT LIBER III.*'

Not only does this confirm the individual reading *AMORVM* in α , but, agreeing with R in so many other details, it leaves us in little doubt as to the form of the whole title in α . No trace is to be found, therefore, of the incorrect title in α . Nor, supposing an archetype devoid of correct title and the incorrect title as yet uninvented, does α appear at all likely, acting within the constraints of his time, to have hit on correct reconstruction of the title himself.⁵¹ The testimony of codex S, as being so heavily infected with β readings, naturally has less bearing on the matter. In fact S has no title, probably because it was never embellished with coloured initials, rubrics etc. Which of the two titles, the α form or the β form, it would have received is now impossible to say, but a marginal note of β character⁵² begins with the words '*Iste liber intitulat(ur) o(uidius) sine titulo*' and suggests (if, as it seems, it is by the first hand) that S had already taken the first step in the transition to full amalgamation with the β side.⁵³

The stark contrast of the incorrect β title with the correct α title forms the strongest possible evidence for the influence of a single MS on the β side. '*Ouidius sine titulo*' permeates, in one form or another, virtually all MSS of the work in the 12/13th centuries as well as extracts contained in florilegia and is even referred to as such in other works of the period.⁵⁴ And yet exceptions among the β MSS do exist. Not to the extent that any β MS sides wholly with α in preserving the correct title, but traces of the correct title do nevertheless appear: one 13th-century florilegium, p₃, writes '*Ouidius in P amoris*', to some extent a flash in the pan, since its two congeners descended from ϕ have '*Ouidius in primo*' (p₁) and '*Ouidius in .i°. sine titulo*' (e), but its appearance is still not insignificant and it occurs again in another, slightly later, florilegium.⁵⁵ More importantly the 13th-century MS E_a, though it displays '*Sine titulo*' in its list of contents and leaves the heading to the work itself blank, ends with the words '*Expliciunt epygramata amorum siue ouidius sine titulo*', an undeniably first-hand incorporation of the correct form. The title in N (above, n. 48) could prove to be earlier than expected. These exceptions alone may explain why this curious error on the β side has received little attention hitherto. For, unless it is possible to prove unanimity among the β MSS in error, evidence for the existence of one MS and one MS only at the head of the β side (together with the explanation which the emergence of that one MS would provide for the lack of β MSS before the 12th century) ceases to exist. Can that unanimity still be shown in the light of these exceptions? Fortunately there is a reason, sufficient not only to exclude inherited transmission of the correct title on the β side but also to show why the correct title appears only here and there in the β MSS and as a complement to the incorrect title rather than as a replacement.

Above (pp. 232–3) we established that the error must affect the whole of the β side, only the truth being preserved in α and without the possibility of α having corrected the error for himself. This we have found to be the case: the correct title is well attested in α with no trace of the incorrect title remaining to suggest that the truth was not

⁵¹ See further n. 56.

⁵² Deciphered by Lenz for his collation of S in *Rend. Ist. Lombard.* 69 (1936), 635.

⁵³ These notes affect only the first six pages in S and may explain why the lines omitted in α were not supplied in S when so many other β readings were (above, p. 232). The marginal additions come to an end too soon to include these lines, and suggest that, as with the title and initial letters, the scribe never succeeded in completing his MS.

⁵⁴ E.g. '*Ouidius... ait in ouidio sine titulo*': cod. Bern. 411, a 12/13th-century *accessus* to the *Metamorphoses*. See E. H. Alton, 'The mediaeval commentators on Ovid's *Fasti*', *Hermathena* 44 (1926), 121; cf. also *ibid.* 95 (1961), 74.

⁵⁵ Cordoba, Arch. de la Cat. 150, 13/14th century (Rubio no. 43).

genuinely inherited from above. Now the same question needs to be asked of these exceptions on the β side – did these MSS inherit the truth or acquire it by other means? That they acquired it by other means becomes plain when the 13th-century commentators are taken into account who discovered the valuable clue to the correct form of the title given by Ovid himself as it is found in the β MSS at *A.A.* 3.343: '*Deue* (or *-que*) *tribus libris titulus quos signat amorum*'.⁵⁶ This revelation could have led to widespread correction of the false title already entrenched in the β MSS, but it did not, the reason being as usual the inertia which, born of confusion, results in the conglomeration rather than selection of material. This may be judged from the state of 13th-century knowledge preserved for us in a commentary on the *uersus bursarii* (vexed passages) of Ovid drawn from Berlin Lat. 4° 219 and Leiden Lat. Lips. 39 and printed as an appendix to E. H. Alton's greatly informative 'Ovid in the mediaeval schoolroom', *Hermathena* 94 (1960), 21–38 and 95 (1961), 67–82. The commentator explains (extract printed on p. 72) that the first problem of the work is its title and gives three proposed versions: '*Incipit ouidius amorum*', '*Incipit o. armorum*', and '*Incipit o. sine titulo*'. The first is supported by quotation of *A.A.* 3.343–4, the second by reference to the first word of the work, and the third by the theory that Ovid originally used love in his title, but, when '*ouidius de arte*' was condemned, feared for this work too, suppressed its first title, and substituted instead '*o. sine titulo*'. Whatever the value of this information at the time, our surviving MSS of the period show quite clearly that in practice the third title continued to be read in profusion, the second not at all, and the first was only just beginning to trickle through, generated evidently in commentaries just such as this. Its force was insufficient to supplant the inherited false title, with the result that, for long into the 14/15th centuries, both titles continue to appear together.

Thus the limited and sporadic appearance of the correct title in later β MSS need not be taken in any way to point to a better β source, and the way is open, as before, to belief in a single MS at the head of the β tradition. It would help if, in addition, we could discover how the fault arose in that one MS. Goold suggested (p. 6) that the title '*ANIMORVM*' in the archetype was preserved by α , but, as making no sense, was omitted by β . We have seen already that this mistake was not in α but is due to R and, in any event, it is hard to argue for the rejection of readings by medieval scribes for the reason that they made no sense. Another possibility, raised by Oliver (p. 150), is that the *Incipit* of *Amores* was joined to the *Explicit* of the preceding work at the foot of the page before *Amores* began (cf. the likely case with P, p. 226 above). Either the *Amores* became detached from this page or the *Incipit* was overlooked in a subsequent copying. But there is a grave difficulty with this, seen by Oliver but not satisfactorily accounted for by him, namely that the MS must, most improbably, have lost not only its *Incipit* but also its *Explicit*. For this too would have been perfectly capable of transmitting the correct title to the β MSS – indeed the very purpose of repeating the title at both ends of a book was to guard against just this type of accident. Unfortunately none of the arguments adduced by Oliver is sufficient to explain how the MS came to lose both of these titles.

There is, however, one further omission which may hold the key to the problem. In addition to omitting the correct title at the beginning and end of the work, the β MSS also all concur in omitting the correct title from the breaks between its individual

⁵⁶ *titulus* Y and β MSS, *titulo* F¹, *titulos* R. The α MSS made nonsense of the beginning of the line: *deue cerem* R *deueterum* Y, a corruption which reduces still further any chance (above p. 234) that α was in a position to recover the correct title for himself.

books. Several of the β MSS have no spaces here and others leave them blank. When it does happen that the space is filled, only the numbers of the books appear – ‘*Explicit liber I. Incipit liber II*’ and similarly between books II and III – or some form of the β title occurs. Again no trace exists of the correct α title. Now medieval MSS do not always repeat the title of a work at the division between each of its component books – P for instance does not⁵⁷ –, but it is certain that α did, because this is the practice at every break in Y for *A.A.*, *Rem.* (only one book but still called ‘*Liber primus*’), and *Am.*, just as it is also in R for *A.A.* and *Rem.* and doubtless would be for *Am.* too if still complete. The total absence of the correct title in the β MSS, not just at the beginning and end of the work but also throughout it, leads strongly to one, very simple, answer: that the single MS which stood at the head of the β side lacked the correct title in all of these places for no violent or sinister reason, but simply because the titles were never added. Quite possibly, as in many MSS, the titles were left to be added in red and this never happened, a not at all uncommon phenomenon. Among MSS of the *Amores*, S, we have just seen, is a case in point.⁵⁸

Thus a still more complete picture emerges of the single β hyparchetype established by the false title on the β side. Once this untitled MS, in its own way the full equal of hyparchetype α , had come to light at some time, our evidence has suggested, in or near the early 11th century, it may have acquired the false title itself, or it may be that the false title and the variations on it arose amongst the many MSS which then originated from this one. Scholarly comment was soon aroused: the marginal note in S (above, p. 234) offers speculation on Ovid’s reasons for his title ‘*sine titulo*’, material which may also have formed part of a commentary on the *Amores* by Arnulf of Orléans in the next century.⁵⁹ In any event the existence of a single β hyparchetype at this point helps both to clarify the nature of the β tradition of the *Amores* and to explain the otherwise puzzling lack of β MSS in the period before the 12th century. The β MS which Ovid’s editors, not having found in existence, have laboured to invent may after all be posited with greater confidence, and stemmatic justification exists – should stemmatic justification be required – for the editor to choose, as he always has, true readings from among the β MSS. The α side has turned out to be somewhat less ‘closed’ than expected, while the β side becomes to this extent less ‘open’. Whether codex β of the *Amores* contained *A.A.* and *Rem.* also, it is not of course possible to say. If it did, the usual order *A.A.*, *Rem.*, *Am.* will have been preserved, with title still to be supplied in the last work only, since they are found in the other two. But it must also be regarded as possible that the *Amores* survived alone or separate from the rest of β , untitled but nevertheless immediately attributable to Ovid on the strength of the first line: ‘*Qui modo Nasonis fuereamus quinque libelli*’.

3. *The question of an archetype*

Is it possible, finally, to reach back any further into the tradition than this? The establishment of two separate hyparchetypes should, after all, lead back at some point to an archetype, and the establishment of an archetype has bearing on the question

⁵⁷ Though it is perhaps important that he leaves three lines unfilled at the division between books 1 and 2 and four lines at the division between 3 and 4.

⁵⁸ The same seems likely also to have happened in the Matritensis (M) of Manilius, where the lack of a book-heading among its descendants indicates the absence of one in M itself, now missing its first leaf. Quite possibly Poggio’s scribe left the heading for insertion later in red or by a hand more accomplished than his own.

⁵⁹ F. Ghisalberti, *Memorie dell’Istituto Lombardo* 24 (1932), 166.

whether the tradition stems from one or more than one majuscule codex which survived the Dark Ages.

But the difficulty is that we have no sure means of fixing an archetype in time. α and β have each diverged by omitting material, either text or titles, contained in the source they hold in common, and we can only guess that in each case the divergence is recent, that α and β themselves omitted the material rather than any one of a number of possible predecessors of each MS. Everything depends on the length of line drawn between the archetype and α in the early 9th century and, again, between the archetype and β (the date of whose copying could well be quite different from its period of influence). The lines could converge at any point. In practice, however, two considerations suggest that they converge quickly. First an overall proximity in the readings of the surviving representatives of α and β (given, especially, the vicissitudes of 12/13th-century copying on the β -side) suggests that α and β do not spring from sources long separately developed, and second the relative lack of errors in β stemming from false word-divisions other than those found in α (some may lie disguised beneath interpolation) suggests that both hyparchetypes originate from a stage in which word-division had already begun to be made. The narrowness of the tradition at this point would indeed allow for its origin in the transcription of just one majuscule codex which had re-emerged by the beginning of the 9th century.⁶⁰

There is also a further point to be taken into account: a certain degree of 'contamination' is already contained within the tradition which is best, if not only, explained by variant readings in the higher MSS. For the split between the readings of α and β is seldom clear-cut, and it is not uncommon for some of the β MSS to agree with α in an incorrect reading when the correct reading is preserved (unless it is due to conjecture) in others among the β MSS.⁶¹ It seems unreasonable in these cases to suppose that incorrect readings have crossed over from the α side by contamination, although we would have to allow that this could more reasonably be so in the more numerous cases of agreement with correct readings in α . But, had this happened, we would then expect the correct title of the work to have filtered back from the α side, and for this we have found no evidence.⁶² Nor is it at all likely that α readings could have entered the second branch of the tradition in predecessors of the single β MS established by the loss of titles, a remote possibility which merely multiplies the entities for little gain.

It is far simpler to believe that the common parent of α and β contained alternative readings, an interpretation aided by the significant fact that incorrect readings held in common by α and some of the β MSS differ only in small particulars and often just by a letter or two from the correct readings found in others among the β MSS. Such differences could easily find their origin in the mistakes, corrections, and genuine

⁶⁰ The very close relationship proposed by Oliver (152), that β represents the parent of α mutilated in some way after α had been copied from it, is ruled out by the explanation given above for the loss of β 's titles. As mutilation does not appear to be involved, β demands at least one separate copying of a parent it holds in common with α . Nor does the *Somnium*, transmitted as 3.5 by α , but in various positions (or omitted) by the β MSS, shed any light on the matter. Even if it could be shown to have been in the archetype and, if so, in what position, this would not help to date the divergence of α and β .

⁶¹ No classes among the β MSS can be established by these divisions; as usual, contamination has made it possible for us only to classify readings, not the MSS themselves.

⁶² It is not totally impossible that scribes on the β side would draw correct readings from α MSS and still prefer not to add to or alter their own '*Sine titulo*', but this remains much less likely than that a composite title would already have emerged, as it did in later MSS.

doubts of a single scribe copying from an older MS in a majuscule script with which he had no great familiarity and in a language which he did not even fully understand. The tradition of Ovid's *Amores* may turn out to be not the only one in which the generation of variant readings can be traced back to this crucial stage of transcription.

It may be questioned whether one MS only of Ovid, a popular author, survived the Dark Ages to beget the later tradition. Here two points are to be noted. First, the notion of the survival of a manuscript should be carefully separated from the notion of its begetting others: they do not come to the same thing. When we talk of one MS of late antiquity having survived the Dark Ages, we really mean that one MS of this age was copied and thereby produced the medieval tradition which ramified from it. This is not to say that other MSS in capital script did not also survive in different parts of the Empire and perhaps even find readers, but there is no guarantee that there were in these places readers with sufficient interest or sufficient means to cause fresh copies to be made of the older codices. Many such must have survived, only to perish *sine prole*. Second, we should remember that, for all his general popularity, the particular tradition we have examined is that of Ovid's *Amores*. There is no reason to suppose that this was a book for which, any more than for Rome's other amatory poets, the early medieval world had much regard, and it is unnecessary to assume that a great number of copies survived the transition from pagan antiquity. Even at the height of the fashion for Ovid in the 12th and 13th centuries proliferation of the work seems, if anything, to have been helped rather than hindered by its circulation beneath the seemly and somehow not altogether unpleasing title: '*Ovidius sine titulo*'.

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