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On the Manuscripts of Propertius

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The Classical Review / Volume 9 / Issue 03 / April 1895, pp 178 - 186

DOI: 10.1017/S0009840X00201637, Published online: 27 October 2009

Link to this article: http://journals.cambridge.org/abstract_S0009840X00201637

How to cite this article:

J. P. Postgate (1895). On the Manuscripts of Propertius. The Classical Review, 9, pp 178-186 doi:10.1017/S0009840X00201637

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notation for the instruments shows this very plainly, the signs recurring there in groups of three, F $\underbrace{\text{u} \text{ 7 } \text{C}}_{\text{u}} \text{ 3 } \text{u} \text{ 3 } \text{K} \text{ 3 } \text{A}$, and so forth. In the notation for the voices, which is adopted in the hymn, κ and λ are the supplementary notes to μ , while ι is an original note with η and θ for its supplements.

When the notes within the octave were reduced from twenty-one to twelve, nine of the supplementary notes had to be omitted. The result was that, while μ had previously been followed by two supplementary notes, it now was followed by only one; and this was known indifferently as κ or λ . Hence κ and λ became what Gaudentius¹ calls $\delta\mu\acute{o}\tau\omicron\nu\alpha$ and Aristeides² calls $\sigma\upsilon\mu\phi\omega\nu\acute{\iota}\alpha\iota$. But there does not seem to be the least foundation for the notion that κ and ι could become a pair of *homotones*.

Even if κ and ι were capable of forming such a pair, they could hardly form it here. As already stated, the *paranêtê synêmmenôn* and the *paramesê* are indicated by a single letter in four of the chromatic scales, and by two different letters in the rest. And this distinction would be meaningless, if two different letters could be used when the two notes were the same.

One of M. Reinach's reasons for adopting the *chroma toniaion* was that 'il est le seul qui puisse s'exécuter sur un de nos instru-

¹ Gaudentius, p. 23, ed. Meibom.

² Aristeides, p. 26, ed. Meibom; p. 16, ed. Jahn.

ments à tempérament'; and his transcript has the modern notation which is associated with the tempered scale. Dr. Crusius uses this notation also—pp. 152 ff.—and thus seems to be assenting to the notion that the tempered scale was known in ancient times.

This notion cannot be based on any authority but Aristoxenos; and there is a passage in Aristoxenos which disproves it altogether.³ He begins by saying that a fourth and a fifth together made an octave. Then he says that a tone was the difference between a fourth and a fifth; thus making his tone a major tone, not a mean tone. And then he says that a fourth contained two tones and a half. The tones being major tones, the so-called half-tone must have been (approximately) half a minor tone. So he followed the Pythagoreans in their division of the octave.

That gives five major tones and two semitones of (approximately) half a minor tone apiece, in place of the five mean tones and two mean semitones, which together form an octave in the tempered scale. And if the *chroma toniaion* had been used, its half-tones would have been the halves of major tones.

No doubt, the discrepancy is very slight indeed. But it cannot safely be ignored. Greek music with the tempered scale would be as bad as Greek architecture with straight lines substituted for its subtle curves.

CECIL TORR.

³ Aristoxenos, pp. 45, 46, ed. Meibom.

ON THE MANUSCRIPTS OF PROPERTIUS.

MR HOUSMAN in his polemic against my pamphlet *On Certain Manuscripts of Propertius* has two objects in view, of which the connexion will not escape the observant,—to 'adjust,' so he calls it, my 'partial estimate' of the Holkham MS (L) of which the readings were there first published and discussed, and to demolish objections to theories of his own which were there brought forward. Mr Housman's style of controversy is now familiar to the public, nor needs it characterizing by me; and the present specimen seems fully to sustain its author's reputation.

He begins with the nine or ten cases where L has a better reading than the other MSS,—N(eapolitanus), F(Laurentianus), D(aventriensis), V(aticanus); A(Vossianus) being deficient in this part of Propertius. I had said (pp. 62 sq.) 'a doubt

greater or less according to circumstances must rest upon all unsupported lections in *any*¹ of the manuscripts AFLDV.' Little room for adjustment here, one would think. But Mr Housman goes through the list which I had collected in fairness to L and for the convenience of my reader, shows that all in it *may* be due to accident or conjecture, and adjusts as follows 'L *therefore brings*¹ nothing new and true to the constitution of the text' and again 'L *has* nothing good of its own.'² A promising beginning this.

¹ *Italics* throughout are *mine* unless I specify otherwise.

² Mr Housman says I 'essay to base a conjecture on an unsupported reading of L at III iv 22.' What I essay to base it on is that reading *taken together* with the serious discrepancy *media* and *sacra* between the other MSS. He again perverts my argument on this passage in discussing II xxiv 46.

Next comes a review of the evidence that L is an independent witness to the tradition of the family Φ (the family of A, F, and, in part, of N), which is not all satisfactory to Mr Housman. Mr Housman, having accepted my conclusion, is welcome to pick and choose among my arguments; but against a controversialist who sets up dummy hypotheses I must observe that by the end of this reply my readers will be able to judge whether I was not justified in neglecting theories which even Mr Housman does not venture to maintain.

The next paragraph deals with the use of L in determining the tradition of Φ . Mr Housman writes, p. 20:

'Where it (L) agrees with F, there the common reading of the two will generally be the reading of Φ ; not always, for at II xxxii 8 they both have *timeo*, but generally. But where L dissents from F, that fact in itself tells us nothing about Φ and shakes F's testimony not a whit. . . The adherence of L to N is absolutely weightless as witness to the tradition of Φ : both steal from other sources, and one of the sources whence L steals may very well be N itself: F remains the sole untainted channel of the family tradition when A is absent.'

Of these three independent witnesses to the tradition of Φ (F, L and N) F has obscured it by intrinsic corruption only; but in L often and in N oftener it has been replaced by readings derived from outside. It is our duty then to ascertain from examining undoubted instances in what proportions L and N have the Φ tradition or an alien one and apply the ratios to the doubtful ones. Suppose the ratio for L to be 80 p.c. and for N 70 p.c., then in at least 50 and possibly in 70 out of every hundred corrupt places in F, the Φ tradition or traces of it will be *preserved both in L and N*. If it be urged that this is of no use to us as we cannot tell *which* these places will be, the answer is; here is a critic's opportunity. He will closely scrutinize the MS indications and will for example conclude that at III xx 17 F's *pignita* means that *pignera* (LN) was in Φ rather than *pignora* (DV) because *i* for *e* is a far commoner error than *i* for *o*.¹ And he will prefer to hold that in these and similar cases (my p. 34) L has *preserved with N* the Φ tradition which F has corrupted rather than hold that it has replaced some absolutely unknown reading by another possibly derived from N. But Mr Housman, though he

To make unlikes like by confusing the circumstances seems to be out of many Mr Housman's favourite paralogism.

¹ Mr. Housman uses the indications of F at II xxii 22 and elsewhere in a precisely similar way *J.P.* xxi 140.

allows the 'adulterated' and 'untrustworthy' N to count against both F and L together (as at II xxxii 8), and again allows L to count with F against N, yet says that L's adherences to N are 'absolutely weightless' against F. Had the cabman of the story been a Propertian scholar, he would surely have called Mr Housman also a 'harbitorary cove.'²

Mr Housman next says:—

'The adherence of L to DV is equally unimportant: the tradition of that family is seldom doubtful, so that a third witness is superfluous, especially so poor a witness as L.'

If he is criticizing me here, he has misconceived my reasoning. Whenever I speak of L conferring a corroboration upon readings of Δ (*i.e.* DV or their family), I refer to the doubt which overhangs the *date* and *composition* of their parent MS, as p. 63 (n.): 'We now know from the evidence of L that some readings of Δ are older than 1421; and this is all we know for certain.'

Mr Housman finally directs us how often to cite L in a critical apparatus—'never...', he says, 'unless it agrees with F and dissents from N, or else presents a reading

² In his note on *peraeque* III xxviii 9 (p. 27 n.) Mr Housman, angry at being told he has not attended enough to manuscript abbreviations, surpasses himself. L (L writes *e* for *ae* always) having *peque*, D (and seemingly V formerly) *pareque*, F *paremque* and N *per aeque* and I having said the original of all was 'neither "pereque" nor "pareque" but "peque"', Mr Housman puts forth as 'probable' the peculiar hypothesis 'that the readings of DV F are due to *peque*, which their common parent O had misinterpreted as *pareque*.' Why? To deprive L of the honour of having preserved the common tradition more faithfully than DV and F. As to N's *per aeque*, I had derived that from *peque* because, as the variation of *ae* and *e* is unimportant and is disregarded here by Mr Housman himself, *J.P.* xxi 131 '*per aeque*, that is *pareque*,' there is no solid ground for separating N from the other MSS with which it agrees far oftener than it differs. But Mr Housman says 'N derives scores of readings from an older source than O, and there is not a hint that this source had *peque* rather than *peraeque*.' So, having provided one source older than O with *peque* against L, he provides another with *peraeque* against me. *Place aux ombres!* After contradicting my statement that N's reading was no better than DV's or F's (F I added, speaking to one point at a time) by saying that '*truth is more obscured by wrongly expanding the abbreviation*—let him add, to himself—'than by misdividing the word' as if that were the test to apply to a scribe, he ends with this outburst: 'On p. 23 an equally baseless charge of neglecting abbreviations is brought against Mr Leo and supported only by flat contradiction.' The wondering reader may be told that Mr Housman has discussed the place in question (II xxxii 8) in *J.P.* xxi 174 without referring to the abbreviation (*t* for *tibi*), a compendium also ignored *ib.* p. 129, on IV iii 51 and *ib.* p. 141, on IV v 35: compare my pamphlet p. 23 and n. 1.

peculiar to itself....If I were editing Propertius I should mention L in about thirty places.' On referring to the very much abbreviated apparatus at the foot of my text in the *Corpus* I find there about sixty-five instances where 'L agrees with F and dissents from N' and about thirty-two where it has a reading, correct or interesting, 'peculiar to itself.' Total about ninety-seven. Either then Mr Housman's apparatus criticus would be of the most meagre description or he has 'adjusted' L's admitted claims to consideration by dividing by three.

We now have introduced the real gist and matter of the controversy—the validity of Mr Housman's own views upon other manuscripts of Propertius. These are set out in the *Journal of Philology* in what I had called (p. 61) a 'brilliant triad of articles,' from which I have myself learnt much and which after all deductions will remain a solid and durable contribution to Propertian studies. One might have thought that this constituted a necessity for criticizing the errors which it contains. Not so thinks Mr Housman. This 'necessity' was 'purely subjective.' Neither he nor his work must be profaned. He 'had hoped he had done with this matter for a long time to come.' But I come 'hacking at the fence' of his paradise, I 'let the boars in among the crystal springs,'¹ and 'like Nehemiah's builders' he must arm in defence of his Holy City. And then the injury to his feelings! 'If it were not for the humour of the situation, I might well resent the tone of placid assurance in which I, who think before I write and blot before I print, am continually admonished by the author of this pamphlet.'² Precisely. γέλωτα δὲ TON EME ἐν τοῖς λόγοις ἀπέδειξε Plato *Theaetetus* 166 A. There is humour in the situation; but not what Mr Housman supposes. The humour is this, that Mr Housman, who has rated half the scholars of Europe, should himself be so sore at reproof and should expect his indignation to move the readers of the *Classical Review*.

The first issue is the trustworthiness of F. Mr Housman gives out that he upholds it against myself who said 'we cannot trust it unconfirmed.' Let us collect his own statements. 'F, whose scribe was a most ignorant man, is defaced by a hundred

blunders which were not in the exemplar whence it was copied' *J.P.* xxi p. 181. 'F is carelessly written by an illiterate man' *ib.* xxii p. 128. 'I attach little weight to F's unsupported readings from I i 1 to II i 63' *Cl. R.* p. 23 n. 2. 'When at IV vii 25 DVNL give *fissa* and F *fixa*, no conclusion can be drawn respecting Φ. Maybe Φ had *fissa* and F, as often, blundered; maybe Φ had *fixa* and L, as often, stole from the other sources: no one can say,' *ib.* p. 21 (in other words between II i 63 and the end, where F stands by itself, we cannot trust it unconfirmed). Thus does Mr Housman maintain the value of F. What then is there left for me to do? What need for homicide when the suicide is so complete? He even subverts the good faith of F where I had left it intact. I will show him how. At III iii 11 F has correctly *lares* and Mr Housman has conceded that *lacres* was in Φ. Now (on p. 25) Mr Housman says, 'When critics' [that is Mr Housman] 'find a "uox nihili" [that is *lacres*] 'altered into a Latin word' [that is *lares*] 'they do not call it confounding two letters but they call it a conjecture.'

To turn to Mr Housman's replies to my criticisms, I had said that in many of Mr Housman's instances, where the difference between the variants was very slight, it seemed more likely that F was 'accidentally right, has in fact blundered into a correction than that one and the same corruption should have appeared independently in all the other manuscripts.' After stating this view (about which he says himself that he is 'quite willing to admit that this might sometimes happen') I considered some of the examples to which he attached exceptional importance: 'Among our examples there are very striking tokens of integrity; the recondite *lunarāt*, *Tiburtina* retained despite the metre, the form *Tarpeia* disguised but thinly, the form *cumba* preserved' *J.P.* xxi 197. And, imagining that it was because *lunarāt* was 'recondite' and because *Tiburtina* violated the metre, that Mr Housman thought their retention a very striking token of integrity, I argued that this was not striking in the case of F, which abounds in strange words and unmetrical lines. Mr Housman's 'head goes round' at being thus understood; and he presents me with the following explanation: 'A striking case' [in which F has abstained, from the absence of motive, from altering strange words and unmetrical lines] 'is a striking token' of F's 'integrity' [or of F's abstaining, from the absence of motive, from

¹ 'For no discoverable reason unless it is the hope of boasting "liquidis immisi fontibus apros"' (p. 22).

² Of the contumelious reference here to my discussion of III xiv 19 (p. 184 below) I intend to make Mr Housman ashamed.

altering strange words and unmetrical lines].¹ So exit Mr Housman by the same hole he went in at. I can only say that, if *lumarat* is not a 'striking' case for the reason supposed, it is striking for nothing else; for I take it from Mr Housman *J.P.* xxii p. 98 that '*lumarat* F' and '*lumarat* DVN'... 'are palaeographically almost identical'; and so that here too F may be accidentally right. *Tiburtina* on the other hand I believe to be actually wrong.²

Mr Housman lays stress on the number, now reckoned at 21, four of which however are also in L,³ of F's good readings, and leaves the reader to infer that they are certain. The re-collation of about one-tenth of the portion from which his good readings were drawn has shown that two (now omitted) II xi l ne and xxxiii 43 *absentis* were mistakes of Baehrens. What a woeful reduction in the total there would be should anything like this ratio be maintained!

Mr Housman next argues that to shake the solitary witness borne by F will 'land' me 'in a conclusion' at which I do not desire to arrive. 'To depreciate F is to depreciate the whole family Φ (as distinct from N); for all the important readings peculiar to that family are found in F alone' (p. 22). Was a question ever begged more openly? All that we know of Φ is gathered from the agreements in AFL (and N). When F stands alone then, to quote Mr Housman again, 'no conclusion can be drawn respecting

Φ.' Readings then peculiar to A, F, or L must be put aside in considering the family Φ: being uncertain they cannot add to our knowledge nor must they affect our judgment.⁴

Another issue is the authority of DV (or Δ). Mr Housman writes, p. 22:—

'It was easy to foresee that the next writer on Propertius' MSS would disparage DV. Baehrens had disparaged N, Mr Leo had disparaged O, Mr Solbisky had disparaged AF, I had defended one and all; so to disparage DV was the only way left of being original.'

Mr Housman has nowhere insulted the intelligence of his reader more openly than here. Why, DV and AF are O, Mr Leo has disparaged both; and when Mr Housman thinks another scholar has taken half of a published theory of his own, he does not call that 'originality,' he calls it plagiarism. But what is to be said when it is observed that at the head of my discussion I have acknowledged my obligations to Mr Leo in these express terms?—'The integrity of DV has been already impugned by Leo *Rh. Mus.* 35, 442, and on good grounds as the evidence to be adduced will show.' Mr Housman however must provide me with a motive; and a craving after originality would do.

My distrust of DV arose differently. In the course of that close and constant scrutiny of the manuscript variants which the preparation of a text entails, doubts of their honesty formed in my mind. This impression, which I received to nothing like the same extent in the case of the other MSS,⁵ grew continually, and made it necessary to examine the evidence, which is collected on pp. 69 *sqq.* of my pamphlet. As dishonesty has various degrees from deliberate falsification to culpable carelessness in copying, and as with the number of proved interpolations grows the probability that the doubtful cases are interpolations too, I have divided it into cases of interpolation and of grave suspicion. Mr Housman now charges me with using 'divers weights and divers measures' and describes my procedure as 'the good old rule, the simple plan, of "heads I win, tails you lose."' I 'denounce' in one manuscript

¹ The matter I add in square brackets is taken almost verbatim from Mr Housman, p. 21.

² In this connexion I had written that 'Mr Housman's advocacy of the value of F's isolated witness involves him in a curious inconsistency. He follows Baehrens in maintaining that "A is the most faithful representative of its family"... It certainly then "happens curiously" that in the poems in which we have both A and F, A should give of itself but one true reading... and F three or four, which Mr Housman thinks after all may be an accident.' His defence, with Dick, Tom and Harry to the rescue, is 'wherever A gives a true reading that reading is also given by F or by N' (his italics) 'or by both.' Those acquainted with the laws of probability will still think it curious that the faithful A, unlike the less faithful F, should never be right *by itself*. But the imputation of 'inconsistency' I will withdraw, as I can find no evidence that Mr Housman has any such acquaintance.

³ Mr Housman's reference to these instances gives curious indication of the bias and confusion in his mind. 'In four of them F is now confirmed by L, so Dr Postgate must confess that in those four I was right,' p. 21. Why, when did I say otherwise? I never criticized his views on F till the pamphlet he has attacked; from it alone does he derive these confirmations and even the remark is 'plagiarised' from me, p. 29. He should have written 'In four of them F is now confirmed by L, as I have learnt from Dr Postgate.'

⁴ The only 'important' unsupported readings are those readings which, being apparently genuine, diverge too much from the rest to be due to accident, and those readings which reveal what the scribe had before him. To this latter class, of which I have spoken (on p. 35), belongs F's *Tarpetia* which, as Mr Housman says, indicates *Tarpeia* in the exemplar.

⁵ This is why I have 'refrained from collecting the similar evidence' against Φ. Mr Housman also has refrained from collecting it.

'the same offences' that I 'extenuate' in another. Mr Housman's own method is simpler still, it is to examine a fraction of the evidence, *selected by himself*, and to ignore the rest; a 'transparent iniquity' which the reader will not fail to detect.

Though the test is such an unfair one, I will not decline it, and refer first to II xxiv 45 sq. 'iam tibi Iasonia uecta est Medea carina | et modo *ab infido* sola relicta uiro,' so DV, *seruato* N, om. FL. I wrote that 'one of the two (readings) must be an interpolation. In deciding which, we ask first which gives the easier construction; and the answer is *ab infido*; and secondly which presents the more obvious sense; and the answer, as we see from another supplement "fallaci" in D, is again *ab infido*.' Mr Housman retorts with the dilemma (p. 26) that 'if the supplement "fallaci" shows that *ab infido* presents the more obvious sense, then it equally shows that *seruato* gives the easier construction. If it does not show that *seruato* gives the easier construction, then neither does it show that *ab infido* presents the more obvious sense.' Pretty, but for the flaw in 'equally'! 'fallaci' does not *equally* show that *seruato* gives the easier construction because the greater obviousness of the sense may have overpowered, as I believe it has, the greater ease of the construction. After this ebullition of fallacy Mr Housman addresses himself to the point as follows: 'Nor does there appear to be any tangible difference in obviousness between the two senses or in ease between the two constructions.' No tangible difference, that is, between the constructions of the ablative with *ab* and the dative with a passive participle; none in obviousness of sense between that of saying the lover who has left Medea lone was 'faithless' and that of saying that she 'had been his saviour.' Let the reader judge whether I have not rightly argued that Δ's reading has *two* marks of an interpolation. But Mr Housman, having once swallowed the explanation of Baehrens that the scribe's eye slipped from *modo* to *infido*, refuses to disgorge it, and though forced to allow that such slipping is not the sole cause of omission, insists that the 'rational' inquirer must recognize it here. The answer is obvious. Baehrens' explanation is palaeographically the easier one; but other considerations are against it. Nor am I inconsistent in adopting the explanation at III iv 22 (above p. 178 n.) where those other considerations are in its favour.

Of II xxxiv 83 and III viii 11 I say briefly that *laudabat* for *ludabat* is suspicious because its sense is easier, and *externas* for *hesternas* because L indicates the intermediate stage *hesternas*; and of II xix 26 that if Mr. Housman can persuade any one that *pedes* for *boues* comes from I xx 8 or IV xi 16, he is welcome to his proselyte.¹

Mr Housman accuses me of Iago-like malignity in imputing fraud to DV. So the reader may be surprised to hear that I have deliberately omitted from my list a passage where Mr Housman, on insufficient grounds, has pronounced them interpolated: II xiii 47 (*J.P.* xxi 140) '*me-minisset* [DV] is a metrical correction of *minisset*.' But amazed he will not be, as soon as he observes that in the very case (I xix 10) selected by Mr Housman as a 'specimen' of my 'uncharitable imputations' he has himself declared their reading an interpolation and rebuked the scholars who have built upon it: '*uerberat* is soon explained as an attempt to correct the easy mistake *ueberat*: there is no cause or defence then for Baehrens' suspicion *peruolat*, or for Mr Rossberg's surprising *uerterat*' (*ib.* p. 183).

A word on the age of Δ. Mr Housman had written in *J.P.* xxi 180 n. 'it was *probably* earlier than 1400, certainly not *much* later.' These words, especially those in italics, mean that he was then of opinion that the approximate date of Δ was about 1400: they are inconsistent with a date one or two centuries earlier. Now he says that that was 'merely his modesty; he claimed no more than he wanted for the point he was then discussing.' So when an unexpected and unpalatable use has been made of this statement, his 'modesty' throws it to the winds, and he declares that 'nothing ties down Δ to the 14th century or near it: it can easily be older than any date yet assigned to N.' I tell him then that of the date of Δ he is wholly ignorant; it can easily be later even than L.² All he knows

¹ Mr Housman errs in supposing I think D interpolated at II xxx 36 from I i 14; but I see now I did not make my meaning clear. The phrase (p. 37) 'the scribe allowed himself to write' (not 'wrote' or 'interpolated') I intended to be taken with the preceding, not the following context; and to exemplify the way in which the scribe allowed his mind to work upon his subject.

² There is nothing against this but Baehrens' opinion that D's date is 1410-20. As Baehrens appears to have mis-dated all the Propertian manuscripts except F, whose first owners' names are written inside, this counts for nothing. As to V, which he places at the end of the 14th century, MM. Stevenson fils, Maurice Faucon, and Pierre de Nolhac agree

is that some of its readings are earlier than 1421; and this he has learnt from me.

Lastly Mr. Housman disputes my estimate of the Neapolitanus. Before replying I concede to him (p. 28) that the argument I based on III v 35 *planustra Bootes* to show N was an independent witness to the readings of O, the supposed common parent of Δ and Φ, is faulty. I ought to have treated this O throughout for all that at present it is, a convenient algebraical symbol for the common readings of AFLDV.

Summing up (p. 73) I had said first that it was 'best as being the oldest of our witnesses.' Mr Housman comments (p. 23):

'But age is no merit. . . Till we have examined two rival MSS, we presume that the older is the better. When we have examined them, we judge them by their contents. . . Useless then to call the Neapolitanus "best as being the oldest of our witnesses," unless you can keep it out of our reach.'

This appears to mean that a manuscript is to be appraised by internal evidence only; or that codices of the first, fifth and fifteenth centuries, if they each have equally good texts, are of equal authority. Unless Mr Housman maintains this absurdity, he must imagine that I believe the oldest codex to be always the best, although I wrote (p. 65) '*Ceteris paribus*,'—*Ceteris paribus*, I repeat—'the older testimony must be believed.'

The next point involves the composite character of N. I had said that 'it was best again as the one that presents the greatest amount of truth with the smallest amount of falsehood.' Mr Housman, unable to advance a step without ascribing to his opponent his own vices of reasoning, says 'Then if I set a clerk to copy out the Teubner text the result will be in Dr Postgate's opinion a still better MS than the Neapolitanus, because it will present a greater amount of truth with a smaller amount of falsehood,' as though, forsooth, the composition of the Teubner text were not matter of knowledge and that of the Neapolitanus matter of conjecture! Mr Housman writes of conflated manuscripts with a strange appearance of passion. 'N adulterates its text.' N and L 'steal from other sources.' This insinuation of dishonesty is quite beside the mark. Manuscripts were the books of ages that preceded printing, and it was the producers' business to gather up all the truth they could from

the sources at their disposal. And well for us that they did, or our texts would be much worse than they are. The 'integrity' which Mr Housman values so extravagantly was an accidental merit, due to the fact that only one source was accessible to the scribe. When therefore, to use his own phrases of N, Mr Housman 'rejoices' to have a MS of the integrity of F, he finds 'a wrong vent for that joy' in extolling F; 'the proper vent is to thank providence' that it did not vouchsafe to F the means of correcting its errors. The value of A and F's integrity is just this. Through it we can restore with some certainty the readings of their immediate source (call it P). But it tells us nothing about P or P's relation to Φ. For all we know P may have been a conflated MS and this the explanation of some or all of its differences from N.

With the next paragraph which deals with beings who believe N to be the best MS because it has no near relation to control its witness, I have no concern. The view is not mine, though Mr Housman makes a quotation from me the text of the paragraph. And the speculation what scholars would or would not hold were the manuscripts of Propertius other than they are, is an airy field where Mr Housman may disport himself at will.

The next question is the relation of DV (Δ) to N. Baehrens asserted that their common readings were derived by N from Δ. This was reasonable enough in Baehrens, who placed Δ in the 14th century and N in the 15th; for conflated 15th century MSS of Propertius abound. It is not so reasonable in Mr Housman, who knows no more about the date of N than he knows about the date of Δ.¹ This ignorance vitiates the only two arguments he advances for his hypothesis. The first is: 'What perversity, then, in order to avoid assuming that the MS known to have blent two elements has blent a third as well, to assume that the MS not known to contain more than one element contains three!' The second is this: 'The simplest hypothesis which will account for any given facts is held to be the likeliest hypothesis'—a statement illumined by a reference to the superiority of Copernicus' account of the planetary system to Ptolemy's.² The like-

that this date is at least half a century out. And Mr E. Maunde Thompson's opinion, based on the photograph in Chatelain's *Paléographie Latine*, is that V was probably written 'after the middle of the 15th century.'

¹ The starveling theory then is his own, not that of Baehrens, on whom he would father it.

² Mr Housman vapours here as follows: 'The College of Cardinals rejected the simple account because it seemed to threaten Holy Writ. Dr Post-

liest hypothesis is however that one which, whether simple or otherwise, best explains the facts. When Mr Housman has ascertained these, when he knows the date of Δ and the date of N, we will talk about the number of elements and on the simplicity of hypotheses. Mr Housman objects that a theory similar to what I have suggested for the resemblances of N and Δ might be set up against the account I have accepted for those between N and Φ , and that then my lips would be sealed, as 'there is not a pin to choose between them.' Mr Housman mistakes. The immediate progenitor of A, F [and L] is referred by Baehrens to about the middle of the fourteenth century; but I think it was probably earlier.¹ The indications appear to suggest a 13th century MS. Φ was earlier still, and may well have been in existence at the earliest date to which N can be assigned. Thus the inference (I should call it no more) that N has borrowed from Φ stands on quite a different footing to the hypothesis that N has borrowed from Δ . But 'observe that the only one of the two which occurs to Dr Postgate is the one which jumps with his own prepossessions: I had considered and rejected both before ever I set pen to paper' (p. 24). The comparison is crushing if Mr Housman be infallible: otherwise it has merely an autobiographical interest.

We now come to the interpolations in N which I had said were much fewer than those in Δ . To show that Mr Housman was unfair to N I took first the passage which Mr Housman chose himself for the first place among his examples of N's dishonesty. At III ii 1 *sq.* 'Orphea *delenuisse* feras et concita dicunt | flumina Threicia sustenuisse' (Ayrmann) *lyra* 'detenuisse' FV, *detinuisse* N, *te tenuisse* D, appear to be all descended from *detenuisse*. Now here, according to Mr Housman, changing *l* to *t* and inserting *u*, thus making the strange *-lenisse* into the familiar *-tenuisse*, is accident, pure accident; 'a slight and honest error.' But writing *tin* for *ten* is quite another thing; 'a bad attempt at amending.' It is nothing that *e* and *i* are constantly confused in our MSS; nothing

gate rejects the simple account because it is derogatory to the scarce less sacred Neapolitanus! Thus does the habit of intemperance in language destroy the perception of proportion and even the sense of humour in its victim.

¹ I think his date for A (1360) also somewhat too late. [Since the above was printed I have received Mr E. Maunde Thompson's opinion based on the page of A photographed in Chatelain's *Paléographie Latine*. He is 'decidedly of opinion that the Vossianus was written about 1300 and just as likely before it as after it.']

that the scribe's eye might have been caught by the *sustenuisse* in the line below; nothing that this estimate of a change of spelling would sow 'interpolation' broadcast over our manuscripts; nothing that at II xxx 26 the same MS shows no dislike to this very form.² It is 'a "uox nihili" altered into a Latin word,' and therefore, by the critic's definition, a conjecture. But here observe a curiosity. Mr Housman must needs add that 'the term *uox nihili*' (mine, in giving the point of view of the copyist of D) 'is somewhat too harsh,' i.e. that 'detenuisse' is in a sense a Latin word. It means then the same as 'detinuisse.' And the honest O has done the very thing which is alleged against the dishonest N. Thus does Mr Housman saw at the branch on which he is sitting. Of this example I say that as evidence it is worthless and ought to be expunged; but Mr Housman, that it furnishes 'evidence of *invincible cogency* for the superior integrity of O.' Vtri creditis, Quirites?

The passage of my pamphlet which deals with III xiv 19 *sq.* I have already said (*Class. Rev.* p. 133) does not and did not represent my views. By an inadvertence which I much regret, I allowed it to be printed off in the confusion in which the reader finds it. Directly I found this out I wrote to Mr Housman to the effect that the passage had escaped my revision and that I regretted this occurring in a place where he himself was criticized. Does Mr Housman then inquire what it was I would have written? No. There was a weapon here too precious to be lost. So he betakes himself to his art of conjectural emendation and corrects, forsooth, my words into the form he desires them to assume. The reader will draw his own conclusions.

I shall here refer to two charges of Mr Housman's. He accuses me (p. 28) both of garbling and plagiarising, the first because I wrote that he 'said' certain readings of *f* and *v* were derived from the same lost MS whence N derives them, in place of writing that he had embodied this

² Mr Housman disputes the relevancy of this parallel because 'scribes are sometimes awake, and sometimes asleep.' But there is a reason why the scribe should not have slept at II xxx 26. There was something the matter with the exemplar there; so the variants of Φ declare. '*detenere*' N¹, '*tenerere*' N²F, '*tedere*' L, different combinations of the same

elements, appear to indicate *tedere* in Φ , an error and its correction (as I have said p. 23 in a passage unfortunately misprinted). This the scribe of N misinterpreted, but the second hand corrected his mistake.

view in his stemma codicum as being the simplest hypothesis; the second because I took (as he says) from him without acknowledgment the observation that 'This cannot be proved or disproved.' These trivial accusations (the 'mis-statement is harmless' he says) come from a scholar who has distorted L. Mueller's views upon the parchment of the Neapolitanus (*J.P.* xxii p. 127, compare my pamphlet p. 63) and appropriated my observations on the confirmations of F by L (above p. 181 n. 3). I too 'acquit him of any intention either to garble or plagiarise; but he has done both.'

Another instance he says is more injurious. I had inferred from the substance rather than from the precise terms of a somewhat obscure sentence of Leo's¹ that he thought that ϵ or the vulgar MSS 'may yet have some revelations in store for us'; and in this I daresay I went too far. But Mr Housman has no grievance worth mentioning. For what I was criticizing was the tone of his own remarks on these MSS. This tone, to which he has forced me again to refer, will be made clear by my italics. He says—

We have tried and condemned the only three among the vulgar MSS which have been selected by any recent critic. When Mr Leo specifies his selections, it will be possible to examine their merits; till then it must suffice to say that I have scrutinised the mass of critical material collected by Burmann and Hertzberg without discovering a fragment of genuine tradition unknown to us from NAFDNf.

The vulgar MSS is a term of much latitude. Besides a few MSS, like the Groninganus, known to be inferior, it includes an unknown number, most untraced, some it may be untraceable, such as the 'codices Passeratii' and the 'codices Gebhardi,' from which lections are cited by Baehrens (as ϵ) Burmann and others. The prejudice against the latter class which Mr Housman's words would create I have endeavoured to dispel, first by the no doubt injurious observations that to scrutinize Hertzberg's collations would appear to be a waste of time, and that others besides Lachmann may err in using Burmann's collections, and secondly by a collection of noteworthy readings of the 'codices Passeratii' (pp. 75 *sqq.*) about which

¹ 'Librarii errores arguere ualebit e melioribus vulgaris notae libris quicumque eligetur.' Any suitably selected MS (this, not 'any' MS, appears to be the meaning of the Latin) of the better vulgar sort which would refute the errors of N, would have extrinsic validity comparable to that of the 'unicus codex' and to the world, if not to the writer, would be a sort of revelation.

Mr Housman preserves a significant silence.

I conclude with the omissions of N. I must first thank Mr Housman for the fresh material he has furnished. There are then five places in which N omits the end of a line: II xxxiv 53 *erumpnas*, 83 *minor ore canorus*, III i 27 *cunabula parui*, v 39 *gigantum*, IV iii 7 *Bactra per ortus*. Two of these omissions Mr Housman says 'may be placed to its credit.' That is, the omitted words were absent or illegible in its exemplar. For the other three one might have expected the same explanation. But no. Mr Housman, forgetting Copernicus and Ptolemy and the simplicity of hypotheses, provides two causes where I am content with one; and infers that N omitted them because of their 'obvious difficulties.' Yes, obvious to Mr Housman but not therefore obvious to the scribe. The text of IV iii 7 has been accepted by most editors of Propertius, the shortened pre-accentual vowel in *erumpnas* would never have troubled a mediaeval copyist, and the scribe of N might well have slumbered through all these passages as the scribes of all our other MSS have done. What Mr Housmann calls inference is mere hypothesis, not absurd, it is true, but simply arbitrary. Its sole support is the pleasing fiction of the slumbering scribe. This remarkable sleeper, setting at nought the laws of probability, wakes only in the hexameter; wakes there only when he comes to the end of the line, the place where exemplars suffer most from injury. Yet not always then. For though with *Bactra per ortus* before him he is more vigilant and suspicious than half the editors of Propertius, let it be *flamma boon* (III v 35) and, at Mr Housman's bidding, he is fast asleep!

The examination of Mr Housman's polemic has shown then that it fails throughout: that his adjustment of the claims of L was not more needed than his murderous defence of F, and that, as I said, the former must be added to the manuscript sources of Propertius, and that, as I said, we cannot trust the latter's witness unconfirmed; that, so far from proving against me the charges of spite against Δ and partiality to N which he has strewn about his pages, he has failed in his own attempts to whitewash Δ and bespatter N, the latter still remaining, what every one believed it to be till Baehrens and Mr Housman arose, the best manuscript of Propertius, owing nothing that we know of to DV and in honesty much their superior. The futility of the attack might

however have been predicted from the spirit in which it was undertaken : *ὅστις γὰρ αὐτὸς ἢ φρονεῖν μόνος δοκεῖ, ἢ γλώσσαν ἦν οὐκ ἄλλος*

ἢ ψυχὴν ἔχειν, οἷοι διαπυθθέντες ὥφθησαν κενοί. Sophocles *Antigone*, 707 sqq.

J. P. POSTGATE.

ARCHAEOLOGY.

SCHILLING ON THE LEGIONS I. MINERVIA AND XXX. ULPIA.

Leipziger Studien, Vol. xv. Erstes Heft. 1893. OTTO SCHILLING. *De legionibus Romanorum I. Minervia et XXX. Ulpia* (pp. 1—128). Leipzig: Hirzel. 7 Mk.

A. The legio I. Minervia was created by Domitian, as the title F(laviae) M(inerviae) P(iae) F(idelis) D(omitianae) shows: and probably before 88 A.D., as it acquired the title P.F. almost certainly from its loyalty during the revolt of Antonius Saturninus; that title was not given in the first century except for loyalty shown in civil, not foreign, wars. The reason for its creation appears to have been to fill up the gap caused by the destruction of V. Alauda in 87 under Fuscus who 'kept his vitals for the Dacian vultures.' If this view is allowed, Ritterling's statement, that from the time of Augustus no legion was created to fill the place of one which had been destroyed, is refuted. The legion was generally stationed at Bonn. At first Italian recruits appear in it, afterwards a few from Noricum, but it was mostly composed of natives of Lower Germany. This gives the reason why the birthplaces of the soldiers in this legion and in XXX. Ulpia are seldom mentioned. These legions were stationed for long stretches of time in the same place and were recruited from the same place. A soldier living all his life in the same quarters would not consider it necessary to mention his native country. I. Minervia took part in the first Dacian War: for in that portion of Trajan's column which represents events in the first Dacian War a legion appears with a ram on its standard. This is the symbol of I. Minervia, probably because Minerva was supposed to have the constellation Aries under her protection (Manil. ii. 439). After this expedition, it returned to Bonn, where it remained until 161, when it was transferred to the Parthian War. It returned, however, in 166 to face an invasion of the Marcomanni.

B. The legion XXX. Ulpia was estab-

lished in 98 or 99 to guard the Danube in place of XXI. Rapax annihilated in the Sarmatian War of 92. The nucleus of the legion came from Africa, to which some recruits were added from the Danubian provinces: but most of the legionaries came from Lower Germany and Belgica, Treviri being especially numerous. XXX. Ulpia came to Lower Germany after VI. Victrix had been transferred to Britain to take the place of IX. Hispana destroyed in 120 by the Brigantes. While I. Minervia was in Asia from 161 to 166, XXX. Ulpia resisted German attacks. In 193 or 194 portions of both these legions were led against Pescennius Niger. After the defeat of Albinus, Severus dealt hardly with the vanquished, and some of their confiscated estates near Lyons appear to have been given to the discharged veterans of these two legions. This accounts for the great number of inscriptions belonging to this legion found near Lyons.

Besides the main history of the legions many side-questions are acutely discussed. Perhaps the most interesting is—What was the twenty-ninth legion on the death of Nerva? *C.I.L.* vi. 3492 (between 120 and 170) enumerates 28, including indeed XXX. Ulpia, and II. Trajana, but omitting IX. Hisp. and XXII. Deiot. both of which were certainly in existence in 98 A.D. Schilling proves that it was XV. Primigenia. He shows that I. Germanica and XV. Prim. were both implicated in the revolt of Civilis, but the former much more deeply than the latter. Hence it was disbanded by Vespasian; whereas XV. Prim. was merely transferred to serve in the East, where it remained until it was cut to pieces by the Parthians in 161. All the inscriptions of these two legions are given in full as an appendix. We trust we may soon have from Schilling a book (and it will have to be a long one) on the history of all the legions, treating each with the completeness, patience and learning which characterize this article.

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