

## ON THE LEIDENSIS OF TACITUS

THE purpose of this paper is twofold: to comment on certain remarks of E. Koestermann, and to examine briefly some passages adduced by K. Wellesley as evidence for the alleged independence of the Leidensis (hereafter L).<sup>1</sup>

In a paper in *CQ* N.S. xv (1965), 299–322, I attempted to demonstrate by various arguments that the readings of L are not such as to support the claim that this manuscript has authority independent of M. Those arguments may be summarized as follows: that the majority of L's readings fall into a pattern of systematic normalization, that they give virtually no help in solving really deep corruptions, that L is particularly unreliable in transmitting proper names, that many readings of L show clear signs of being derived from M, that few (if any) of the good readings in L could not have been extracted from the corrupt text of M, and that other fifteenth-century manuscripts of Tacitus contain good corrections, not inferior to corrections found in L. No defender of L has yet, as far as I know, answered the arguments I put forward, and E. Koestermann has not even understood them, for he writes as follows (vol. iii of his commentary on the *Annals* [Heidelberg, 1967], p. 21): '[Goodyear] legt den Finger darauf, daß die abweichenden Lesarten in L in ihrer Diktion stärker ciceronischen Charakter tragen, demnach eher als Konjekturen zu verstehen seien. Aber dies Argument ist nicht durchschlagend, da die späteren Annalenbücher . . . eine gemässigte Tendenz aufweisen und damit näher an Cicero heranrücken.' First, I did not say what Koestermann alleges: that many of the readings of L involve a change to more Ciceronian vocabulary is incidental to my main arguments. Secondly, Koestermann appears to believe that the change of style after Book 12 of the *Annals*, which Löfstedt and others have detected, provides some support in general for those readings of L (they are very numerous) which introduce a more 'normal', colourless, hackneyed expression. It is obvious that he has not realized the nature of the change for which Löfstedt produced evidence, nor its limitations. Really striking change is restricted to a small number of words and usages. And further, the whole pattern of stylistic development in Books 13–16 on the evidence of M is subtle and complicated, and the changes we find are consistent with what we know of Tacitus' continual reshaping and refinement of his style (see *JRS* lviii [1968], 22 ff.). In contrast the numerous additional changes attested by L are often enough haphazard and superficial, not comparable with changes found earlier; they do not closely correspond with the sort of changes for which Löfstedt found evidence. This is a matter which requires detailed analysis and Koestermann would have been wiser to look at the evidence before making generalizations. Thirdly, if Koestermann thinks that Löfstedt's evidence for a movement towards greater 'normality' in *Ann.* 13–16 makes L's normalizing readings serious possibilities, what does he suppose the position to be in *Ann.* 11–12 and *Hist.* 1–5, since there too L offers a multitude of readings of precisely the same character? Neither Löfstedt nor anyone else has yet suggested that *Hist.* 1–5 show the same kind of

<sup>1</sup> The controversy over L is by now so well known that I need not explain it here. For a select bibliography see Heubner's commentary on *Hist.* 2 (Heidelberg, 1968), p. 5.

approximation to 'normality' which in some ways *Ann.* 13-16 are believed to show. So it follows either (a) that the established view (since Wölfflin) of Tacitus' stylistic development must be abandoned or most radically changed, or (b) that one half of L has authority and the other half has not, or (c) that Koestermann is wrong. *Quantum non datur.*

In a paper in *Rh. Mus.* cx (1967), 210-24 K. Wellesley attempts to show that 'the good readings in L . . . are not the product of an eccentric and gifted emendator, but reflect a tradition independent of, and in some ways superior to, that of the Second Medicean'. The evidence he offers for this view consists of four passages of the *Historiae* which, he believes, 'contain unique and excellent contributions from L not hitherto observed or evaluated'. I want to consider whether these passages are such good evidence as he supposes, but first one or two general remarks are necessary.

No one can now seriously doubt that most of L's readings are worthless, nor again that a minority are good and right. The question at issue is whether, amongst L's good or plausible readings, there are some by their nature such that independent tradition is either the only acceptable explanation of their existence or at least clearly the most probable explanation. Two or three readings might suffice to establish independent tradition, if they were of surpassing excellence and not easily extracted from anything transmitted by the rest of the manuscripts, such as the Blandinius's *lusumque trigonem* at *Hor. Serm.* i. 6. 126 or the Gyraldinus's *ingenium sacrare* at *Aetna* 227. So far, however, I have not discovered a single reading of L which clearly comes into this class.

It is not my purpose to show that the four readings of L discussed by Wellesley are indisputably wrong, but only that they are not indisputably right or not of conspicuous excellence. This is all that need be shown to render them useless for Wellesley's argument, since in cases like this the *onus probandi* rests inexorably on those who would prove independent tradition. The passages are:

*Hist.* i. 49. 3 *famae nec incuriosus nec uenditor; pecuniae alienae non adpetens, suae parcus, publicae auarus M: . . . uenditor; alieni non adpetens, sui parcus, publice auarus L*

Wellesley's first argument is against M: 'The Thesaurus distinguishes between two senses of *auarus*: (a) *auarus sc. comparandae pecuniae*, = "covetous" and (b) *auarus sc. retinendae pecuniae*, = "miserly". If it is associated with an objective genitive, the former sense is intended. To this rule we are told that the passage under discussion provides the only exception.' And so we are to believe that *pecuniae . . . publicae auarus* = 'miserly with public money',<sup>1</sup> as the context shows it must, is suspect as a 'unique exception to a well-attested rule'. But who excogitated this rule, and who attests it? Certainly not the writer of the article on *auarus* in *TLL*, who knows nothing of it.<sup>2</sup> His article gives no example of *auarus*+a genitive = 'covetous' of money. It does give a number of examples of *auarus*+a genitive = 'covetous', in reference to matters other than money (e.g. *Hor. A.P.* 324 *praeter laudem nullius avaris*), and two examples of *auarus*+a geni-

<sup>1</sup> It is hard to find a good translation, not because there is anything wrong with the text, but because Tacitus is stretching the language to get an effect, the gradation *non adpetens* to *parcus* to *auarus*. Galba was niggardly with his own money and more than

niggardly with public money: to get a word stronger than *parcus* Tacitus uses *auarus* rather abnormally.

<sup>2</sup> Wellesley does not say that the 'rule' emanates from *TLL*, but it is unfortunately very easy to take that as his implication.

tive = 'miserly', again not of money, namely Symm. *epist.* 1. 90. 1 *auarus officii* and Ennod. *epist.* 4. 17 p. 111. 15 *uerborum auarus* (two examples which incidentally show that, in spite of what Wellesley has been told, the passage of Tacitus is not altogether unique). No rule is formulated in the article, presumably for the excellent reason that the evidence does not suggest one. Wellesley's remarks certainly need further justification than he gives. The true position appears to be that *auarus* + a genitive in reference to money, whether the meaning is 'covetous' or 'miserly', is equally unusual, that in so using the word with the latter meaning Tacitus breaks no rule, and that, in view of his freedom in the use of the genitive in other ways, his innovation here is unremarkable.

Wellesley proceeds to argue for the merit of L's reading, and is at pains to show that *alieni non adpetens, sui parcus* is acceptable Tacitean Latin, a proposition which no one familiar with Tacitus would dispute. Indeed, I suspect Wellesley has misunderstood why various scholars say that *pecuniae* is necessary in this passage. The reason is not that *alieni . . . sui* is 'per se' objectionable, but (a) because of the harshness of the change of idiom from *famae* to *alieni* in L's text, for *famae* at least suggests that, e.g., *pecuniae* is needed,<sup>1</sup> and (b) because the phrase *publice auarus* is singularly vague ('greedy' or 'a miser in public life'), while we need a reference to public money. The ambiguity which Wellesley thinks he finds in M's text is even more easily to be found in L's. It is *publice auarus* which he should have defended, not the innocuous *alieni adpetens, sui parcus*.

Wellesley further argues that L's reading is closer to Sall. *Cat.* 5. 4. This is certainly true, but it is not clear what follows. There is no cogent reason to assume that Tacitus imitated Sallust so closely here: sometimes Tacitus imitates Sallust closely, sometimes not.<sup>2</sup> Wellesley further points out that there are several occurrences of the contrast *aliena/sua* in this book (20. 1, 22. 1, 52. 2) and that in an author given to self-repetition this may support L's text. This is a just point, but not very weighty. One could argue equally well that these examples help to explain the alteration, if such it is, in L.<sup>3</sup>

Since no case has been proved against the text of M, and since the text of L raises one or two difficulties which cannot lightly be dismissed, the passage provides no clear evidence of L's independence.

*Hist.* 1. 52. 2 et Vitellius <ut> apud seueros humilis, ita comitatem bonitatemque fauentes uocabant, quod sine modo, sine iudicio donaret sua, largiretur aliena; simul auuiditate †imperandi† (ra in rasura) ipsa uitia pro uirtutibus interpretabantur M: . . . impetrandi . . . L

This passage may be dismissed very briefly. L's correction is probably right; certainly the change is slight and the sense apt. But why this passage should be

<sup>1</sup> I set little store by this argument, for it is hard to find the limits of Tacitus' use of variation. Still, I agree with those who think this a rather odd example.

<sup>2</sup> One might explain L's reading as an attempt to bring Tacitus nearer to his model. These fifteenth-century manuscripts of Tacitus do some arbitrary and irresponsible things, e.g. intruding *Oct.* 368 ff. at *Ann.* 14. 8. 5.

<sup>3</sup> Wellesley suggests that if L's *publice* were

corrupted to *publicae*, then *pecuniae* would obviously be supplied, and a series of 'trifling' consequential changes produce the text of M. This is not quite impossible, but it is, as Wellesley would say, 'a much lighter strain upon my credulity' to suppose that someone in the fifteenth century, for reasons uncertain, tampered with a text not unlike that of M, and derived from it, and produced the text of L.

considered evidence of independent tradition I do not see. The context clearly supplies the idea of giving and receiving, and, this being so, to suppose that to change *imperandi* to *impetrandi* (or even so to misread it) was beyond the scope of a fifteenth-century scholar seems to me absurd. The passage is irrelevant.

*Hist.* 3. 13. 3 id Basso, id Caecinae uisum, postquam domos hortos opes principi abstulerint, etiam militibus principem auferre †litem† *M*: . . . etiam militibus principem auferre, principi militem *L*: . . . etiam militibus principem, principi militem auferre *Koestermann*: . . . etiam militem principi, militibus principem auferre *C. Heraeus*: *alii alia*

I agree with Wellesley in accepting that we have here some sort of chiasmic word-play, and therefore that the text of *M* is truncated. And I acknowledged (*CQ* n.s. xv [1965], 320) that 'L's reading is a very clever correction, far above L's usual standard'. What I dispute is that it is clearly the best amongst several serious possibilities. Wellesley affirms that L's reading alone 'explains the mysterious *litem* of *M*. A continuous text has been wrongly corrected PRINCIPEMAVFERREPRINCIPIMILITEM, leading, at the next copy, to the form *principem auferre litem*'. Wellesley gives no reason for the mistaken correction, and, since AVFERRE intervenes between *principem* and *principi*, no explanation is obvious. *Contra*, if Koestermann is right, and the original text was PRINCIPEMPRINCIPIMILITEMAVFERRE, the omission of PRINCIPIM(I) by parablepsy is particularly easy, one of the most common types of corruption. Admittedly, the resulting muddle LITEMAVFERRE was for some reason rearranged. But, since stage (1) of the corruption supposed is much easier (parablepsy rather than unexplained correction), the fact that a stage (2) must be postulated hardly makes this solution less likely than one in which the first and only stage is much more difficult. Further, the structure of Koestermann's text seems quite as acceptable as that of L, where the insertion of *auferre* somewhat spoils the chiasmic pattern.<sup>1</sup> Koestermann's reconstruction is at least as likely to be right as the text of L. Therefore the passage offers no indisputable evidence of independent authority, though one may freely admit that, if not right, L's reading is not far from the truth.

*Hist.* 3. 66. 3 Fabium illis Valentem, captivum et †captis diebus reseruatum *M*: . . . casibus dubiis . . . *recc. nonnulli*: . . . paucis diebus . . . *L*

Wellesley dismisses the vulgate *casibus dubiis* very categorically: 'the grammar of the phrase is dubious and its sense non-existent'. As to the grammar it would have been helpful if he had explained in what essential ways the passage differs from, e.g., *Hist.* 2. 33. 2 *dubiis proeliorum exemptus summae rerum et imperii se ipsum reseruaret* and Liv. 3. 51. 3 '*melioribus meis uestrisque rebus reseruare*' inquit '*ista de me iudicia*'. As to the sense, it is in the nature of *casus* to be fickle. That the Flavians are successful and confident of victory does not make it senseless for them to keep an important hostage, at least while Vitellius is alive. But, the implied argument goes, though it was good sense, in view of the *incerta casuum*, to keep Valens alive, they could not resist killing him. So *a fortiori*, since Vitellius, unlike Valens, would be of more use dead than alive, they would certainly kill

<sup>1</sup> This is not a decisive point, but it is worth mention that the three parallels which Wellesley cites which seem closest to the present passage all have the verb outside the chiasmic pattern, namely *Hist.* 1. 36. 2 *modo*

*imperatorem militibus, modo milites imperatori commendare*, *ibid.* 3. 33. 1 *stupra caedibus, caedes stupris miscerentur*, *Sen. Contr.* 2. 1. 9 *ne auferam patri filium, filiis patrem*.

him, in spite of any promises. Further, a small argument in support of *casibus dubiis* or something similar, Tacitus, like other writers, normally states the purpose of the preservation of something or somebody. Still, though normal, this is not invariable. All I wish to show is that *casibus dubiis reseruatum* is perfectly acceptable, and that yet again Wellesley is in too much of a hurry to lay down the law.

L's reading is very attractive, for two reasons: (a) because it could easily have been corrupted into *captis diebus* after *captiuus*, and (b) because it gives very apt sense, since, as Wellesley shows, it seems clear from other evidence that Valens was kept prisoner for a period sufficiently short for it to be described in this context as *pauci dies*.

We thus have two serious possibilities, *paucis diebus* and *casibus dubiis*, both first found in fifteenth-century manuscripts. The immediate question, however, is whether *paucis diebus* must be considered a vestige of separate tradition, since it accords with independent information. I think it is not clearly such, for, given the corrupt text *captium et captis diebus*, etc., *paucis* is amongst the first and most obvious changes which would occur to any critic (there are, of course, many such corrections in L). The immediate context tells us that Valens was captured and killed, the general context that all this happened within a fairly short time. *diebus* needs definition and the corrupt *captis* suggests an adjective or participle. *paucis* is readily available. By contrast *casibus dubiis* is a much more subtle and sophisticated change (not necessarily preferable for that reason). It is odd that it gained the field at the expense of L's more simple and palaeographically convincing solution.<sup>1</sup>

Wellesley has usefully shown the merits of L's reading here, and I for one would accept it into the text. But once again, as with *impetrandi* at *Hist.* 1. 52. 2, he seems to have confused the rightness of the reading with its value as evidence of independent tradition. To establish the former is not the same thing as to establish the latter.

The four readings Wellesley adduces fall very far short of proving what he would like them to prove. Further discussion is unnecessary, but I will add two observations. In his last paragraph Wellesley makes much of the fact that, along with plausible readings in L, there is much patent nonsense, and takes this variety in the quality of L's readings as evidence that L's good readings are vestiges of tradition, not the work of an emendator. Now it is quite true that such a mixture is what one would expect in a report of readings from a lost manuscript,<sup>2</sup> and which one would not expect from an edition.<sup>3</sup> But there are various possible explanations of this mixture of sense and nonsense, of which faithful reporting of an old manuscript is only one. Another is that L is indeed not a systematic edition, but contains, upon the basis of the text of a descendant of M, a conflation of diverse material, in particular (a) corrections and interpolations shared with other *recentiores*, (b) corrections perhaps by one corrector, perhaps by more, not shared with other *recentiores*, some of them of considerable skill and intelligence (hardly more, however, than R. Agricola, amongst others, may be presumed to have possessed). This theory seems to me fully to account

<sup>1</sup> It is because *casibus dubiis* has been so universally accepted that *paucis* has not been conjectured a dozen times, not because it is difficult to conjecture.

<sup>2</sup> I have discussed precisely such a case in

the intr. to my *Aetna*, pp. 49 ff.

<sup>3</sup> My earlier remarks (*CQ* n.s. xv [1965], 322) about L being a sort of edition should be modified.

for the evidence. My final observation is one I hesitate to make, but it must be made sooner or later. If the admirers of L cannot shortly produce some firm evidence to support the claims they make for it, then it will be neither unreasonable nor precipitate to judge that the controversy is over and that L remains *unus inter recentiores*. The controversy is one that admits of being settled to a high degree of probability, and indeed, with all respect to Koestermann, Wellesley, and those scholars now working on the *recentiores* as a whole, it is very nearly settled already. Those on whom the *onus probandi* rests have not proved their case, and they do not seem likely to do so. It does not require years of microscopic work to establish that a manuscript is an independent authority in such an author as Tacitus; there has been time enough, since Mendell rediscovered L, to prove the case many times over, if the evidence were there. It is not.

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