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THE MANUSCRIPT TRADITION OF TACITUS' *GERMANIA*

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FORTY years ago the American scholar Rodney P. Robinson published the results of his long investigations into the manuscript tradition of Tacitus' *Germania*.¹ His conclusions, and particularly the critical edition he based on them, won little commendation. R. Güngerich finished what can only be described as a vicious review² with a justification for his heat—decades of German research on the topic could not be left undefended. J. G. C. Anderson's excellent text (Oxford, 1938) made use of some of Robinson's material; Jacques Perret's Budé (Paris, 1949) pillaged it wholesale. Yet neither (any more than had Robinson himself) succeeded in reducing it to simple and convincing shape; and the editions of Lenchantin de Gubernatis (Turin, 1949) and Koestermann (most recently in his third Teubner edition, Leipzig, 1970) returned to pre-Robinson simplicities.

My own researches suggest that Robinson's details, though not his most startling conclusion, are largely sound; and this paper is designed not to confute him, but to show how his material, marginally modified, can be put to proper editorial

use. I am encouraged in this by a remark of another, hardly more sympathetic reviewer of Robinson, the veteran A. Gudeman:³ the problem of the *Germania* will only be solved when the parallel tradition of the *Dialogus* has been investigated. My view of the *Dialogus* tradition is already in print;⁴ and what I have to say about the *Germania* will be seen to interlace most intimately.

The *Dialogus* has a three-pronged descent: three lost hyparchetypes, X,⁵ Y, and Z, can be reconstructed with more or less labor and more or less precision. The same is true of the *Germania*. There is no coincidence here; a number of the same manuscripts reappear, to play familiar roles. It will be convenient to look at only a selection of the witnesses first.

Corresponding to Z of the *Dialogus*—and no doubt originally forming part of the same book—is ζ⁶ of the *Germania*. Common witness to both Z and ζ is the Vienna MS s.n. 2960 (=W: V of the *Dialogus*). But, whereas in the *Dialogus* we can put alongside V the excellent Ottobonianus E, in the *Germania* W finds only an inferior stable-companion, the

1. *The Germania of Tacitus* (Middletown, 1935).

2. *Gnomon*, XIII (1937), 257–64.

3. *PhW* (1937), p. 426.

4. *Philologus*, CXVI (1972), 114–28. A planned revision of the Oxford Classical Text of Tacitus' minor works will incorporate R. M. Ogilvie's edition of the *Agricola* and new editions by myself of the *Germania* and *Dialogus*.

5. But I have reservations about X: see below, p. 2.

6. I use Greek letters to distinguish from the hyparchetypes of the *Dialogus*, which doubtless formed part of the same books. The sigla for the minor works of Tacitus are in a state of hopeless confusion, to which I am doubtless adding. All the manuscripts I shall discuss date to the fifteenth century. All citations are by chapter, section, and line of Koestermann.

wayward and untrustworthy Monacensis 5307 (m), to leave out of account the intangible Hummelianus, which exists only in scholars' collations. The margin of W often displays the variants that doubtless go back to the archetype; m does not. Where m differs from W, it is normally in error not found in the other two branches of the tradition. Where it does seem to stray into another class, this is sometimes because W and m made different choices between variants in their common ancestor (thus 38. 2. 23 ipso *B^m*: solo *B*: ipso solo *CW*), sometimes, doubtless, the result of sheer coincidence (thus 28. 4. 7 germanic(a)e *BW*: germani(a)e *Cm*). m is no paragon of virtue, but I doubt if it is contaminated. We can be sure that the agreement of Wm will give us the reading of ζ; and when they disagree we can hope that the agreement of one of them with the rest of the tradition will represent the reading of ζ.⁷

I have argued that for the *Dialogus* hyparchetype X is for all practical purposes Vaticanus latinus 1862 (B), because the other important member of this class, the Leyden Periz. XVIII q 21 (b), is probably descended in some manner from B. The same problem arises in the *Germania*, which appears in the same pair of manuscripts. Robinson doubted⁸ whether b is independent of B. The hypothesis was greeted with exclamation marks by the reviewers. But once we rid ourselves of the prejudice that b has stemmatic virtues just because it has good readings introduced by independent conjecture, we can see the strength of Robinson's view. Ignoring cases where b's correct readings are not found elsewhere in the tradition (cases,

that is, of free conjecture), b's advantage over B is limited to the following cases:⁹

- 5. 2. 19 negauerint *b*ζ: nauigauerint *B*: negauerant *C*
- 5. 2. 22 et *b*Cζ: etiam *B*
- 9. 1. 4 sueuorum *b*Cζ: suenorum *B* (*the name is frequent*)
- 10. 2. 23 gentis *b*Cζ: getis *B*
- 21. 2. 1 poposcerit *b* (*corr. from -is?*), *C*ζ: poposcerunt *B* (*obviously impossible after abeunti*)
- 33. 1. 25 bructeris *b*Cζ: bructeriis *B* (*the correct form appears just above*)
- 39. 1. 7 celebrant *b*Cζ: celebrant *B*

These are trivial; all can, as Robinson says, easily be attributed to conjecture. It is commonly assumed that b is the fair copy of a manuscript containing corrections by Pontano; and it contains far more impressive conjectures than these. I feel even more confident here than in the *Dialogus* that b has no independent value. It should appear in an apparatus only when it offers worthwhile conjecture.¹⁰

The third hyparchetype, *I*, corresponding to Y of the *Dialogus*, may for the moment be represented solely by C, the slovenly Vaticanus latinus 1518.

We must now ask the same question that I raised about the *Dialogus* tradition. How are ζ, B, and C related to each other? In a Maasian system, untouched by contamination and uncluttered by coincidence, where manuscripts α, β, and γ descend independently from a common ancestor, they will have some errors in common (these will be the errors of their ancestor); they will each have their own special errors; but no two of them should ever agree in error against the other. In the *Dialogus* this was not so. Each of the three witnesses, on several occasions, gave

7. W and m are independent of each other. W, but not m, omits 21. 1. 23 *enim* and 22. 1. 10 *ut*. m, but not W, omits 13. 2. 17 *-que*, 16. 3. 8 *frigorum* and 18. 2. 4 *uiro*.

8. Pp. 194–203.

9. Robinson (p. 200) adds a few other passages which are either completely trivial or irrelevant.

10. "Codex Leidensis quin praecipuum locum obtineat inter eos quidem codices, qui extant, dubium non esse potest" (Koestermann, p. xix). If that is true at all, the quality is not, for the most part, *inherited*.

the truth where the other two agreed in error. Indeed Z provided so impressive a list that there was some temptation to regard B and C as having a common ancestor below the archetype. On balance I thought it best to resist the temptation; and in the *Germania* the temptation hardly exists, though Robinson succumbed to it. In this he was probably wrong, and his critics were right to fasten on his error. For ζ's unique virtues amount to no more than the following:¹¹

- 2. 2. 25 herminones ζ: hermiones BC
- 28. 2. 28 signatque ζ: significatque BC
- 29. 1. 16 bataui W: batami m: batauii BC
- 36. 2. 12 tracti ζ: tacti BC
- 37. 3. 29 et ipse ζ: et ipso et ipse BC
- 40. 1. 17 nobilitat ζ: nobilitas BC
- 45. 5. 20 radiis ζ: radius BC

That ζ *does* give the truth in some of these cases (particularly the first two) is disputable. And it would be difficult to be sure that the list cannot be quite whittled away by assuming free conjecture (particularly at 29. 1. 16, for the correct form appears just below at 29. 2. 28) or coincidence of error.¹²

We may compare with this list the virtues of B:

- 5. 2. 22 perinde B: proinde B^vCζ
- 13. 3. 19 h(e)e Bm: h(a)ec CW
- 14. 2. 3 tum B(?)m: cum CW
- 15. 1. 18 hebent BW²: habent Cζ
- 30. 3. 24 propior B: prop(r)iora Cζ
- 33. 2. 31 at B: ac Cζ
- 37. 1. 18 ambitu B^v: ambitum BCζ
- 38. 2. 23 ipso B^vm: solo B: ipso solo CW
- 43. 3. 19 religionis B: regionis Cζ
- 45. 6. 26 differunt B: differuntur Cζ
- 46. 2. 10 figunt B: fingunt ζ (def. C)

11. I ignore cases where these virtues are shared by that part of the *Γ* tradition that has been subject to contamination from ζ (see below, p. 5), and places where Robinson seems to be wrong in preferring the reading of ζ. I do not note, either, that some of the readings of ζ given here are supported by other members of the *Γ* tradition besides C. And I do not give cases of a three-cornered split, as at 38. 2. 23 religant m: rei ligant W: religatur B: ligant C; that ζ preserves inherited

Again, it is easy to raise doubts about this list. The truth is not always certain (e.g., at 30. 3. 24). Some of the words are constantly confused in manuscripts, making coincidental error likely (e.g., at 43. 3. 19). And where variants are at large (as at 38. 2. 23), it is always possible that unique truths can appear in a single manuscript by accident. Finally, free conjecture may account for some of these cases (e.g., 13. 3. 19).

Lastly, C can pride itself on:

- 10. 2. 28 plebem sed C: plebem Bζ
- 44. 2. 9 oceano C: oc(c)eanum Bζ

No one would base anything on the first of these passages; the addition is almost certainly conjectural, and it may not be correct (Koestermann rejects it). As for the second, the preceding *ipso in* made correction easy.

I conclude that B, C, and ζ are independent witnesses. If any two are connected against a third, it will be BC against ζ. But in my view Robinson was unwise to commit himself to such a connection, and still more so to prefer the readings of ζ in places where they did not deserve preference. The prudent editor will assume a tripartite tradition; and in most cases he will find that two witnesses against a third will give him the truth—or at least reconstruct the archetype.

If only these witnesses existed, there would never have been any difficulty about the transmission of the *Germania*. The other manuscripts are responsible for confusing the issue. Can they be made to help?

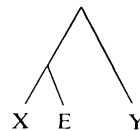
truth in such situations proves nothing about the interrelation of BC. So too below with B, W and m are occasionally right against the whole tradition (e.g., 39. 1. 5 patrum W; 42. 1. 26 bois m), doubtless by conjecture.

12. I agree, that is, with J. Perret, *Recherches sur le texte de la "Germanie"* (Paris, 1950), p. 35, and with many reviewers of Robinson.

There is no help for ζ; nor does it need it. But B has allies. This was one of the heresies of Robinson that most infuriated his critics. When the Jesi manuscript was discovered, with its fragment of a ninth-century *Agricola* alongside a fifteenth-century *Germania* (E), there was a natural tendency to see the later part as a direct copy of the lost archetype. This view, held by Cesare Annibaldi, was combated first by R. Wünsch, then by R. Till, though neither brought forward decisive arguments.¹³ Koestermann accepted their refutation, while clinging to the view that E descends from the archetype independently of BC. In this way is formed a third class¹⁴ (for Koestermann has no time for ζ)—in which, naturally, E is far superior to its known relatives, some of which were known before E appeared and more of which were brought to light by Robinson.

This procedure ignores Robinson's view that E and B descend from a common ancestor, the hyparchetype β. The view has never been refuted. Koestermann "doubts" it. His proof is: "Saepissime enim YX vel YE¹⁵ contra ceterorum codicum fidem verum indicant, quod fieri non potuit, nisi unaquaeque stirps e proprio quodam apographo derivata esset. Quare quodsi (*sic*) . . . Y decem omnino locis contra auctoritatem familiarum XE

propius ad veritatem accedit, causa non est, cur sententiam viri doctissimi sequamur, cum ea res alio modo facilius declarari possit" (pp. xxiii-xxiv). The first contention is just not true, as a second's reflection on the following stemma will show:



Under these conditions, there is nothing to prevent—indeed everything to encourage—the agreement of XY in truth against E, and of YE against X, from time to time. The second contention, that XE's agreements in error against Y (in my sigla, the agreements of BE against C) can be explained in other ways, is true, but not decisive.

Two things suggest to me that Robinson's view is correct. One is the number of agreements in error (or highly questionable truth) between B and E against Cζ.¹⁶ The second is the behavior over variants.¹⁷ Both B and E habitually preserve the same alternative readings; and these variants are sometimes unique to them. Observe especially¹⁸ 39. 1. 6 eiusdem nominis *Anderson (after Robinson)*: nominis *W^v*: nominis numinis *B^vE^v*: omnes *BE*: omnis *Cζ*.

13. Summary in Koestermann, p. xxii. As to Koestermann's own arguments, the fact that E leaves out many words does not prove that it is not a direct copy of the archetype. And the omission of the words *terrisque . . . solis* at 45. 5. 19–20 is due to homoeoteleuton (*insulis-solis*) and need have nothing to do with the length of lines in its ancestor.

14. To be fair, there had been talk of a third class in connection with relatives of E even before its discovery: see R. Wünsch, *Hermes*, XXXII (1897), 47; and R. Reitzenstein, *Philologus*, LVII (1898), 307–17. The fallacy has often been a statistical one: E does not always agree with either of the other two classes, therefore it must be a *Mischhandschrift*, poised between the classes, or a third class in itself. This is particularly clear in F. F. Abbott's discussion of the Toledo MS, a copy, it would seem, of E (*Chicago Decennial Publications*, Ser. 1, VI [1904], 217–58, esp. 244). T agrees with Bb in truth 48 times, in error 10 times; with Cc in truth 39 times, in error 3 times. This is taken as proof that T is independent

of Bb and Cc. It is independent in the sense of not being descended from any of them. But the figures are perfectly consistent with Robinson's view of the E-class as being descended from hyparchetype β, for the agreements in error of TCc are not decisive.

15. That is, in the sigla I have so far employed, BC and CE.

16. See the list in Robinson, pp. 191–93, to which add 37. 1. 15 *situm* and 44. 2. 9 *ips(aje)*. I am especially impressed by 37. 2. 23 *consulatam Cζ*: *con^{tum} B*: *conuentum E*. The abbreviation causing the trouble was in β. At *Dial.* 13. 1. 9–10 the same abbreviation leaves traces in B and Y.

17. So Abbott, *op. cit.* (n. 14), p. 249; and Robinson, p. 192, n. 1.

18. Add, e. g., 8. 2. 28 *albriniam*; 36. 2. 13 *aduersariis*; 43. 2. 15–16 *lygiurum*; 43. 2. 18 *halisionas*. All these are marginal readings peculiar to BE among the primary manuscripts.

Nor does E appear to have been contaminated from C or ζ. Where it crosses class boundaries in error, it is always easy to see some explanation. Thus:

7. 1. 7 ne BC: neque Eζ (*influenced by previous neque*)
 14. 2. 3 tum B(?)m: cum ECW (*the words are hardly distinguishable in some scripts*)
 28. 3. 2 ab Osis] a boiis BC: a bois Eζ (*cf. error at 42. 1. 26*)
 30. 2. 17 romane Bζ: rōe (*i.e., ratione*) EC (*ambiguous abbreviation*)
 46. 2. 10 figunt B: fingunt Eζ (*def. C*) (*coincidence?*)
 46. 4. 25 corpora Bζ: et corpora EC (*coincidence?*)

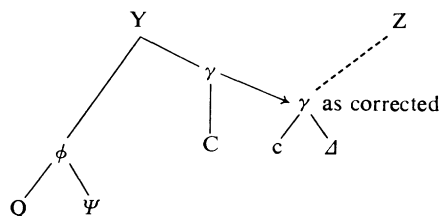
Occasionally E hits on the truth¹⁹ (37. 4. 3 *mallio*). But that is no more than B and ζ were able to do; and unless there were a two-pronged tradition, E on the one hand and BCζ on the other, these occasional flashes could hardly be *inherited*.

E then is in the main a B-class manuscript.²⁰ We should therefore follow Robinson in constructing a hyparchetype β from the agreement of B and E, largely ignoring the peculiar errors of the two individually. This is in itself a marked relief to an apparatus.

E has a number of relatives, correctly diagnosed by Robinson. Some of them may well represent β more faithfully than does E: I think especially of Ottobonianus lat. 1209. But no doubt the editor will do well to stand by E; apoplexy might greet its complete removal from the scene.

β, then, may join ζ as a primary witness. What is to be done about C?²¹ As in the *Dialogus*, so here: traditionally editors have reconstructed a lost manuscript

Y from the agreement of C and the Naples manuscript IVc21 (c). But I have tried to show that for the *Dialogus* an ancestor of c (and of its twin, Δ, the Vaticanus latinus 4498) was contaminated²² from the Z-group, where C remained pure; and that therefore, though readings common to Cc do go back to a common ancestor, there may be other readings in C which are inherited from that ancestor even though they do not appear in c. I also suggested that account should be taken of other relatives of C. These descend from a single lost manuscript φ, to be reconstructed from Marcianus 4266 (Q) on the one hand and a group of deteriores, Ψ, on the other. The situation seemed to be:



I rejected the possibility that γ and Y were identical on the grounds that not all errors of γ reappear in φ; but I should have stated with more stress that contamination in φ from elsewhere would explain this phenomenon, just as the fact of contamination in γ as corrected explains why not all errors shared by Cφ reappear in cΔ.

These results were obtained independently of Robinson's treatment of some of the same manuscripts in the *Germania* tradition, and it is comforting that our conclusions are very similar. There is no

19. Like C (above, p. 3), E has *plebem* <sed> at 10. 2. 28, but the word is added above the line: independent conjecture? At 13. 3. 20 I cannot see why the editors favor E's addition of *et* before *electorum*.

20. Incidentally, E is not a copy or descendant of B, for it avoids B's dislocation at 25. 1. 19; many omissions in E prove that it is not an ancestor of B.

21. The discovery of Ottob. lat. 1434, a copy of C, does not help (see my *Dialogus* article [n. 4], pp. 120–21).

22. This argument proved particularly unwelcome when Robinson propounded it for the *Germania*. See, e.g., Koestermann, p. xx: "Quodsi multas lectiones cum veras atque rectas tum falsas et plane absurdas per contaminationem, ut ita dicam, e Vindobonensi in codices stirpis Y defluxisse contendit, quis est qui tantam audaciam concludendi tolerare possit?" I, for one: this is just how contamination does work.

need to go again over common ground. I need merely state that for the *Germania* CcΔ are related to the hyparchetype just as they are in the *Dialogus*;²³ and that φ—to avoid begging questions let us call it Φ—is to be reconstructed from the same Marcianus Q and a twin,²⁴ the Parisinus nouv. acq. 1180, which I call p. By means of this further witness to the reading of the hyparchetype (which here may be called Γ), we can avoid burdening the apparatus with the many trivial errors unique to C; and we can also leave out of account the readings agreed by CcΔ if they are not confirmed by Φ.²⁵

This last principle has to be somewhat modified in view of the admitted fact that Φ has been subjected to a certain amount of contamination from a manuscript related to E.²⁶ That the contamination is in this direction and not the other is shown by the appearance of variants in the margins of p and Q that clearly arise from their descent from a manuscript that has undergone correction from elsewhere.²⁷ I note some typical passages:

11. 1. 15 ut BCζ: in EpQ

13. 1. 9 pater BΓζ: ipsi Ep^vQ^v

18. 3. 11 denuntiant BC(ζ): renuntiant EpQ

23. Robinson thinks that C, too, was copied from γ (to use my own sigla) after γ was contaminated from ζ, but almost always rejected the corrections. That is only a difference of emphasis. But I hold to my own picture. If C, as well as cΔ, was contaminated with some *good* readings from ζ we should never know; this *could* be the case wherever Φ stands alone in error against BCcΔζ (though one might expect it to leave traces in the shape of variant readings in C). If C, as well as cΔ, was contaminated with some *bad* readings from ζ, we should expect to find βΦ uniquely preserving the truth more often than they do. In my article on the *Dialogus* (p. 122 with n. 26) I remarked that cΔ did not seem to agree in error against C and the rest. There are cases of such agreement in the *Germania*; thus 13. 2. 17 *primum* and the omission of (*quoque* at 37. 4. 3 (marcoque βζ: mi quoque C: m. quoque Φ; M cΔ). But at least one of the agreements can be explained by the supposition that the scholar who corrected γ from ζ also made corrections of his own as he went along. Thus at 25. 2. 24 C omitted the correct *sunt*; *est* in cΔ will represent a correction in γ, made *suo Marte* by its scribe. I am not sure that the evidence forces us to postulate a γ₁ descended from γ and propagating c and Δ. But, as with the other issue, little difference is made in practice.

25. 1. 16–17 uerberare BCpQ^vζ: uerbere Q:
uerberant Ep^vQ^v

25. 1. 19 est BCζ: om. EpQ

Such errors are eliminable. But they do suggest that in some cases where CcΔ agree against BΦζ, CcΔ may be correctly reporting the hyparchetype Γ. And indeed, cΔ themselves being subject to contamination from another direction, C alone may occasionally give the reading of the hyparchetype. Fortunately, in a three-pronged tradition like this, the exact constitution of one of the hyparchetypes may remain a little uncertain without unduly dismaying the editor. For the agreement of the other two hyparchetypes makes the behavior of the third relatively unimportant.

In the *Dialogus* tradition a group of inferior manuscripts Ψ descended from φ. What group corresponds in the *Germania*? Only²⁸ two manuscripts, apart from those already discussed, contain both works. One is Vaticanus lat. 2964, clearly a Ψ manuscript for the *Dialogus*, clearly an E manuscript for the *Germania*. The other is the Walters Gallery, Baltimore, MS 466, not known to Robinson, but as clearly a member of his group related to Urbinas

24. That it is a twin is shown by the fact that p, but not Q, omits 28. 1. 24 *sedes*, 32. 19 *haec iuuenum*, and 35. 2. 4 *arma*; while Q, but not p, omits *donec* at 1. 2. 10 and has, among various other errors, *contendunt* at 24. 2. 7.

25. In fact, my edition will ignore most such readings. An apparatus could safely make no mention of c and Δ except for the very few passages where they give us the truth against the archetype. In practice, I shall asterisk readings supported by either or both of these manuscripts; readers may thus judge how far the contamination of γ progressed.

26. Another relation of C, the editio princeps, is not pure either. It may not be contaminated in the *same* way as pQ, but it is certainly a scholar's conflation that cannot serve as a true witness to the hyparchetype.

27. The scribes of p and Q, faced with the variant readings in their exemplar, did not always react in the same way. p, but not Q, notes *uiuentes* (so E) in the margin at 18. 3. 11. At 24. 1. 4 it gives *spectantium* (so E) in the margin, the reading of the archetype, *expectantium*, in the text; Q knows only *spectantium*.

28. I ignore the Vindobonensis 49, for which see my *Dialogus* article, p. 123, n. 28.

412 (u) as it is a Ψ manuscript for the *Dialogus*. It would be easy to construct hypotheses to account for a copy of the lost ϕ - Φ containing, besides Ψ of the *Dialogus*, a *Germania* related either to E or to Urbinas 412. But in neither case is there much temptation to think of class u or class E of the *Germania* as corresponding to class Ψ of the *Dialogus*. For neither is in any sense a descendant of Φ . That is obviously true of class E; and, as we shall see, it is true of class u also.

Urbinas 412, to let this one manuscript stand for its class, displays errors culled from all sides, as well as many of its own. It agrees with β in error at (e.g.) 38. 1. 16 *quam* (*quamquam* $\Gamma\zeta$); with Γ at (e.g.) 10. 2. 27 *hinnitus* (*hinnitusque* $\beta\zeta$); with ζ at 38. 2. 23–24 (*h*)ornatorem (-torem $\beta\Gamma$). It shows occasional signs of readings taken by Φ from class E (*statuto*²⁹ at 39. 1. 5), but none of the group listed above (p. 6). It is not impossible that it is descended from a separate hyparchetype, as Robinson thought;³⁰ but, if so, it is so overlaid with subsequent contamination that it is quite useless for the purposes of an apparatus. To its contamination the following passages bear eloquent witness:

13. 1. 8 tum *CW*: cum $\beta\Phi$; tumeūtu *m* (*that too a sign of contamination*); tum cum *u*
 24. 1. 4 spectantium *Ep^vQ*: expectantium *BC p ζ* : spe expectantium *u*
 28. 2. 28 nomen $\beta\Gamma W^v$: onem *B^vW*: nomen adhuc boiihemionem *m* (*conflation!*): onem nomen *u*

However virtuous the ancestor, the corrupt progeny can hardly be trusted to act as

arbiter between the classes already established; Robinson would have done better to leave u out of his apparatus.

Nor was his use of a fifth witness to the archetype better advised.³¹ The Stuttgart manuscript hist. Q 152 (s) might, like u, seem to have something of everything, particularly, perhaps, ζ (e.g., 2. 1. 20 sibi *Ws*; 33. 2. 1 in gentibus ζs ; 45. 2. 4 omnique *W²ms*), but also Γ (e.g., 20. 1. 6 educationibus *C^l[Q^l]s*; 42. 2. 29 quadisue *CQs*) and the class of E (e.g., 45. 4. 13 profertur *Es*; 45. 5. 17 ergo *Es*; 46. 3. 14 sola *Es*); not to speak of u itself (e.g., 16. 1. 30 nostri *us*). The process of contamination is vividly illustrated by the reading at 40. 3. 2 concessum $\beta\Gamma\zeta$: commissum seu concessum *s. s*, like u, is a deterrimus, and we should ignore it—except when, exceptionally, it gives us the truth.

It is true that “magis necessarium esse videtur editores in Tacito iterum iterumque legendo tempus atque labores consumere quam in codicibus deterioribus innumeris haud segniter perlustrandis, qui nullo modo apti atque idonei sint ad scriptorem melius cognoscendum” (Koestermann, p. xxv). But it is also true that if apparatuses have to be constructed they might as well cite the primary witnesses and ignore the secondary. The labors of Rodney P. Robinson were directed toward distinguishing between the two; and it is time his results were better heeded.³²

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29. Perhaps accidental; m concurs.

30. Pp. 211 ff.

31. He discusses s at pp. 233–35.

32. The collations for this article were done mainly from

microfilms bought with a grant from the Board of Literae Humaniores in Oxford, to which, as to the libraries concerned, I am very grateful.