THE LEYDEN MS OF TACITUS' MAJOR WORKS

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The days of Niccolò Niccoli and Poggio Bracciolini seemed with us once more when we read in *The New York Times* for Nov. 8, 1951 (p. 31, col. 7) that Professor Mendell of Yale had discovered in Leyden the manuscript of Rodolphus Agricola (Roelof Huusman or Huysman) that had been used by Ryck (Rycke) in his *Animadversiones ad Tacitum* in 1686 and in his edition of Tacitus in 1687.[1] Even the manner in which Mendell had pursued the literary trail of the MS brought color back into Classical scholarship. He early satisfied our curiosity by two articles in which he expounded the problems presented by Leidensis BPL. 16. B,[2] but as yet we still remain without widespread scholarly agreement on the question of whether this MS does in fact offer a new tradition for the text of Tacitus XI–XXI (as the MSS designate Tac. *Ann*. XI–XVI and *Hist*. I–V).[3]

[1] This manuscript had merely been noted by Giarratano on p. xi in the preface to his edition of the *Histories* (Rome 1939), and he had said nothing about its connection with Agricola and Ryck. He regarded all the fifteenth century manuscripts as unimportant, although he gave full reports of them (including Leidensis) for the two passages lost from the great Laurentianus Mediceus 68.2 at a problematical date (*Hist.* 1.69–75, 1.86–2.2).

[2] C. W. Mendell and S. A. Ives, "Ryck's Manuscript of Tacitus," *AJP* 72 (1951) 337–45; C. W. Mendell, "Leidensis BPL. 16. B. Tacitus, XI–XXI," *AJP* 75 (1954) 250–70. These papers will hereafter be referred to as *AJP* 72 and *AJP* 75. The articles remain the prime source of information in spite of the appearance of further discussion in C. W. Mendell, *Tacitus, the Man and his Work* (New Haven 1957), which will hereafter be referred to as "Book." The manuscript has been published in photographic form by the firm of A. W. Sijthoff (Leyden 1966) as vol. XX of *Codices Graeci et Latini* (Scato de Vries-Lieftinck), with a Preface by C. W. Mendell and Addenda to the Preface by E. Hulshoff Pol. This work is hereafter cited as "Sijthoff-Leidensis." I shall usually refer to Mendell's longer discussions rather than to his Preface in this volume. In Appendix IV I shall explain why I do not believe that our problem is resolved by Dr. Hulshoff Pol's brilliant work.

[3] St. Borzsák, "P. Cornelius Tacitus," *RE* Supplmtband 11 (1968) 506–9, presents a recent and valuable summary of the bibliography about the Leidensis; he reserves judgment. See also my Appendix I.

Reviewers of Mendell's[4] fundamental article of 1939[5] were convinced by his thesis that the Tacitean MSS of the fifteenth century were not necessarily descended from the eleventh century Second Medicean (hereafter referred to as L 68.2, since it is of course in the Laurentian Library in Florence). That point, however, is no longer the principal one at issue, for the problem now is whether Leidensis alone stems from a tradition apart from that of L 68.2. E. Koestermann, the editor of the Teubner Tacitus, was so convinced that it does that he has made extensive use of it in his latest editions (*Annals*, 1960, 1965; *Histories*, 1961) although, in the most recent statements of his opinion in the introductions to his 1961 edition of the *Histories* (p. xvii) and his 1965 edition of the *Annals*[6] (pp. xxi f.), Koestermann hesitantly decided that Leidensis was not wholly from a different or earlier tradition than that of L 68.2, but that it only exhibited assistance from a MS in a different or earlier tradition, since it lacks some chapters at the end of Book XXI that are also lacking in some other MSS.

I wish, in all friendliness, to present a dissenting judgment, and I shall undertake to set forth the objections to be answered and the problems to be resolved before it is proved that a new manuscript tradition exists.[7]

[4] I took my degree at Yale in 1936 with a dissertation written, under Mr. Mendell's excellent direction, on a MS of Tacitus then recently presented to the Sterling Library. The MS was known as the Yale MS of Tacitus (or Codex Budensis Rhenani, since it had been made for Matthias Corvinus of Hungary and had later belonged to Beatus Rhenanus); as Yale now owns three MSS of Tacitus, this MS has become Yalensis I. I published the following articles on the topic: "The Yale Manuscript of Tacitus (Codex Budensis Rhenani)," *The Yale Univ. Library Gazette* 11 (1937) 81–86; "Beatus Rhenanus, Editor of Tacitus and Livy," *Speculum* 12 (1937) 382–5; "The Four Corvinus Manuscripts in the United States," *Bulletin of the N.Y. Public Library* 42 (1938) 315–23; "Tacitus, Histories IV, 46–53," *Yale Classical Studies* 6 (1939) 29–38 (hereafter referred to as Allen). I trust that this present article will be looked upon as a work of *pietas* towards my erstwhile mentor. Mr. Mendell was kind enough to discuss this paper with me in its early draft more than a dozen years ago, and to save me from several errors, even though he disagreed with the views expressed.

[5] C. W. Mendell, "Manuscripts of Tacitus XI–XXI," *Yale Classical Studies* 6 (1939) 39–70, hereafter referred to as *YCS*. I have reference to the reviews by E. Harrison, *CR* 54 (1940) 168 f.; L. E. Lord, *CJ* 37 (1941–42) 99. The comments on Mendell's article by B. B. Boyer do not affect matters here under consideration: "The *Histories* of Tacitus," *CP* 44 (1949) 107–15, esp. 112 f.

[6] In his 1965 edition of the *Annals* Koestermann has added pp. xx–xxii to the unchanged *praefatio* of his 1960 edition; these contain additional remarks on the Leidensis.

[7] It should be kept in mind that I am not maintaining that all the fifteenth century MSS are descended from L 68.2. To maintain that would require a clear stemma, which Mendell once thought we might never have (*YCS* 67), and which he presented

It is to be hoped that this paper in no way detracts from Mendell's achievement, for it is a splendid achievement indeed to focus attention on a text previously believed to be dependent on the authority of a single MS. The study of Tacitus, to which Mendell has profitably devoted many years, will certainly benefit from this renewed interest.[8]

It is well, even at the risk of some repetition later, to view the problem first in its broadest aspects, for until recently no one except Mendell has really given serious attention to the complete problem of the extant MSS of Books XI–XXI.

For *Annals* I–VI, which will also enter into the matter, we depend wholly on Laurentianus Mediceus 68.1 (Medicean I, Med. I). Since we shall have to consider the possibility of a time when both the *Annals* and the *Histories* existed in a collected edition, as commemorated by St. Jerome's statement about the thirty books of Tacitus,[9] we cannot regard the history of this MS as entirely separate from the history of the MSS of the rest of the major works.

It is commonly agreed that L 68.1, itself of the ninth century, was copied from a manuscript in minuscules that had been copied from a manuscript in Rustic Capitals. R. P. Oliver[10] used the titulature of L 68.1 to support the latter conclusion and to designate as the

only tentatively in his book (344). My point is that Leidensis, if not descended from L 68.2, is descended from a MS more similar to L 68.2 than Mendell would allow. It would of course be highly valuable to discover a MS which was copied from a close cognate of L 68.2 and which preserved better readings from their common archetype, but such a situation will, in the case of Tacitus, be difficult to demonstrate on the basis of readings alone (Book 331 f.). The independent value of the readings of Leidensis is rejected by R. H. Martin, "The Leyden Manuscript of Tacitus," *CQ* N.S. 14 (1964) 109–19; and by F. R. D. Goodyear, "The Readings of the Leiden Manuscript of Tacitus," *CQ* N.S. 15 (1965) 299–322. Their value is convincingly maintained by K. Wellesley, "In Defence of the Leiden Tacitus," *RhM* 110 (1967) 210–24; cf. *idem*, "Was the Leiden MS of Tacitus Copied from the Editio Princeps?," *AJP* 89 (1968) 302–20. Hereafter I shall refer to the *AJP* article as "Wellesley." His answer to the query of the title of his article is in the negative; my discussion of this matter is in my Appendix IV, and not so restricted to a study of readings.

8 K. Wellesley and R. Hanslik have in prospect an edition that will supply the readings of all the MSS of Tacitus XI–XXI: R. Hanslik, "Zur Überlieferung des Tacitus," *Anz. Oester. Akad. Wissen.*, phil.-hist. Kl. 104 (1967) 155–62.

9 It is irrelevant to the present study, but still to be noted that a codex containing both the major and the minor works may have existed at Fulda in the ninth century (Koestermann *Annals* 1965, p. v).

10 "The First Medicean MS of Tacitus and the Titulature of Ancient Books," *TAPA* 82 (1951) 232–61, esp. 232–34, 261.

hyparchetype of L 68.1 a Rustic Capital codex that originally contained both the major works of Tacitus and that dated from the fourth or even the third century.[11]

On p. 237, n. 4, Oliver further accepted the theory, tentatively put forth by E. A. Lowe,[12] that L 68.2 was copied in the mid-eleventh century directly from a Rustic Capital manuscript of the fifth century or earlier.

On p. 260, n. 88, Oliver recognized that Dom Quentin evolved an importantly different theory of the manuscript tradition. By counting letters and estimating the amounts of text involved in transposed material in Book III of the *Histories*, Dom Quentin concluded that the first two transpositions took place in a MS which had a folium of the content of the folia of L 68.1.[13] (The transpositions are described in my footnote 17.) Mendell (Book 239) seemed to subscribe to a theory that L 68.1 at one time was part of a MS containing all that we have of the *Annals* and the *Histories*, and that the lost portion is the archetype of L 68.2 as well as of all our other MSS of Books XI–XXI. In Sijthoff-Leidensis (p. xiv) Mendell has now, for palaeographical reasons and influenced both by Koestermann and by Dom Quentin, decided to place the archetype of L 68.2 and Leidensis in the period between the fourth and the eighth centuries. We thus have a third theory to add to the other two chief theories of the origin of L 68.2,

[11] G. Poghirc maintains that the consecutive numbering of the *Annals* and the *Histories* goes back to the third century and the emperor Tacitus: "Sur la répartition des livres de Tacite entre *Annales* et *Histoires*," *StudClas* 6 (1964) 149–54, an article that I know only through *AP* 35 (1964) and H. W. Benario, "Recent Work on Tacitus (1964–68)," *CW* 63 (1969–70) 255.

[12] E. A. Lowe, "The Unique Manuscript of Tacitus' Histories (Florence, Laur. 68.2)," *Casinensia* (Monte Cassino 1929) I, 257–72, esp. 271 f. Although Mendell refers to Lowe's article in both the *YCS* article and in his book (240 n. 3, 297), he nowhere mentions the problem evoked by Lowe's theory of the archetype of L 68.2 since it does not allow of intermediaries. Giarratano (above, note 1) wrote in his preface (pp. viii f.) that L 68.2 was derived from a MS written in Rustic Capitals, and with one or more MSS intervening between this MS and L 68.2.

[13] H. Quentin, *Essais de critique textuelle* (Paris 1926) 176 f.; cf. H. Goelzer, *Tacite: Annales, Livres I–III*[5] (Paris 1958) pp. xxv f., for the suggestion that L 68.1 itself once contained the now-lost material between *Annals* VI and XI. Mendell (*YCS* 46, Book 239) cited Dom Quentin's article with approval and apparently agreed with him, although such approval could well have had the difficulty for him of forcing the crucial points in his stemma (Book 344) into the period between the ninth and the eleventh centuries.

one that it was copied directly from a Rustic Capital MS of the fifth century or earlier, and the other that it was copied from a ninth century MS of the same type as L 68.1.

Mendell, without considering elaborately the possible text history of L 68.2, for some time applied himself to the study of the numerous fifteenth century MSS (more than thirty), with the thought that one of them might represent a tradition different from that of L 68.2. In Leidensis he has found a manuscript that he believes represents the tradition at a stage older than that of L 68.2.

We must, before we go further into the hypothetical early history of the MSS of this portion of Tacitus, clarify what we mean by "new tradition" or "different tradition." Mendell has nowhere claimed that any of these MSS is descended from a tradition entirely different from that of L 68.2. Although he now stresses the uniqueness of Leidensis, we should also note his earlier assertion in *YCS* 46 that it has not been proved that they are descended from L 68.2, and that there is a strong possibility that they might offer more accurate readings of the archetype from which both they and L 68.2 are descended.

As Mendell points out (*YCS* 45 f., Book 326), general considerations are enough to demonstrate that none of these MSS could be descended from a tradition completely distinct from that of L 68.2, which shares with them a single archetype. All MSS of this portion of the *Annals* begin at the same point in Book XI and end at the same point in Book XVI. They then continue with Book XVII (*Histories* I), and all the complete MSS end in XXI. The fifteenth century MSS can be divided into general groupings by the points at which they end in Book XXI: Group I ends with *evenerant* in XXI 13; Group II with *potiorem* in XXI 23; and Group III (formerly Groups III and IV in *YCS*) with *Pannonia* in XXI 26.[14] L 68.2 ends with *Pannonia* and Leidensis ends with *potiorem*. Hence it is impossible for us to expect from any of these MSS a different tradition in the ideal sense of one which would

[14] These general groupings are part of Mendell's paper in *YCS*, somewhat modified in his book (326 f.), notably where he combined Groups III and IV into Group III, although noting some points of difference. I maintain this numbering of the Groups because they are so designated in several of the works to which I shall constantly refer. Mendell (Sijthoff-Leidensis xii), following Koestermann, now designates Group I as Group III and Group III as Group I, solely to acknowledge the relative lengths of the MSS in diminishing order.

supply us with more of the text than we have from the tradition of L 68.2, or even with a text wholly distinct from that of L 68.2.

Yet we are not entitled to decide summarily that these fifteenth century manuscripts are direct or distant *apographa* of L 68.2. For one thing, L 68.2 has lost the second sheet from the center in the eighth quaternion, which meant the loss of XVII 69–75 and XVII 86–XVIII 2. Since the manuscripts of the fifteenth century have these portions, we must conclude either that they come from some other MS, or that they were copied before L 68.2 lost those folia, or the final alternative (*AJP* 72.337) that they may descend from the immediate ancestor of L 68.2. Mr. Mendell remarked (*YCS* 46 f.) that L 68.2 is in a difficult script, with a number of pages that are nearly illegible.[15]

On the other hand, if they are descended from the same archetype as L 68.2, but not by way of L 68.2, then we are confronted with the all but impossible task of demonstrating which of all these MSS acquired the fewest poor readings and preserved the greatest number of true readings in the process of its descent from the archetype. This is exactly the task Mendell set himself with regard to Leidensis in *AJP* 75.253–69 and Book 330–37, with what success each reader will have to judge for himself, although I grant that his arguments are impressive.[16] My own sentiment, not especially original, is that fifteenth century manuscripts often present both dull and brilliant readings that have no ancient authority. I think that we must look for something larger, and we are fortunate in having a larger item in the case of the "three inversions" (or "transpositions"),[17] which will

[15] E. A. Loew, *The Beneventan Script* (Oxford 1914) 286, mentions L 68.2 in noting that in some portions of eleventh century MSS written in Monte Cassino the ink faded within a century or two.

[16] Leidensis also, however, supplies a large number of most unsatisfactory readings (*AJP* 75.255–57). Wellesley (above, note 7) 305 remarks that L "has suffered demonstrable interpolation to a greater degree than other recentiores."

[17] In the case of the first inversion, a wrong folding of the innermost sheet of a gathering placed XIX 7 *reuirescere* through XIX 9 *inimici* in XIX 5 between *satur* and *nino* of the word *Saturnino*, while *satur* became *satium*. A little line-counting in the Oxford Classical Text shows that there are about 41 (full) OCT lines contained in the section from XIX 5 *Saturnino* up to XIX 7 *reuirescere*, and also about 41 (full) lines contained in the section from XIX 7 *reuirescere* through XIX 9 *inimici*. If we number the pages of this folded sheet 1 2 3 4, as in a modern book, we can see that a wrong folding would have produced this situation and the numbering 3 4 1 2.

The same sort of wrong folding produced the second inversion, where XIX 67 be the subject of constant reference in the rest of this paper. The "third inversion," or better still, the absence of the third inversion in Leidensis, is acknowledged to be crucial in our estimate of Leidensis, both by Mendell (Book 328–30) and Koestermann (Preface, p. xvii, 1961 Teubner *Histories*), who find in favor of Leidensis, and by Heubner (see Appendix I), who rejects Leidensis.

In view of the fact that such able scholars have not been able to arrive at agreement on the interpretation of what they acknowledge to be the crucial point, our study of this matter will be better informed if we postpone it until we have first given some thought to the highly speculative matter of exactly what might be possible and what might be impossible in the prehistory of the text of all Tacitus' major works.

In particular, we must reflect further upon the question of the century in which we can place the archetype of all the major works of Tacitus. It has previously been noted that Oliver wished to place the archetype of L 68.1 in the fourth or even in the third century. Mendell (Book 344, cf. 326) wished to place the common archetype of all the MSS of Books XI–XXI as the next stage after the thirty-book tradition noted by St. Jerome, a point of view that would incline perhaps also to the fourth century. The difference in the two traditions is that L 68.1 is supposed to have an intermediary MS in minuscules before one reaches back to the Rustic Capital codex, whereas Lowe's suggestion allows no intermediary between L 68.2 and a Rustic Capital

ferebatur through XIX 69 *acciuit* was placed in XIX 65 after *senecta sed* (*seu* L 68.2). Line-counting shows that there are approximately 45 (full) OCT lines from XIX 65 *senecta* up to XIX 67 *ferebatur*, and about the same number of lines between XIX 67 *ferebatur* and XIX 69 *acciuit*. Here also, therefore, the innermost sheet of a gathering was incorrectly folded, which in a modern book would have produced the pagination of 3 4 1 2 instead of 1 2 3 4.

In the third inversion XX 52 *ferunt* through XX 53 *defuisse crede* was inserted in XX 46 between *pecunia* and *tanta*. There are about 112 (full) OCT lines from XX 46 *pecunia* up to XX 52 *ferunt*, and about 37 (full) lines between XX 52 *ferunt* and XX 53 *defuisse crede*. Since the first unit is three times the length of the second, we may conclude that in this case the reverse folding occurred in the next to the innermost sheet of a gathering. This means that 1 2 3 4 5 6 7 8 became 7 8 1 2 3 4 5 6, if we number the two innermost sheets of the gathering like modern pages.

Two further comments are needed on the third inversion. I have given the lemma of *defuisse crede* instead of *credebatur*, because *crede* is a problem to receive special consideration later. The second point is that a little experimentation with slips of paper folded and numbered to correspond to the two sheets will show that it is remarkable that the binder now had these two sheets folded side by side instead of one within the other.

manuscript. Dom Quentin, of course, found an intermediary between L 68.2 and the Rustic Capital archetype.

Our problem, then, becomes closely related to the history of the ancient book if we wish to imagine the fortunes of our text through the centuries.

While much opinion now favors reliance on Martial's poems about parchment codices in his own day,[18] and while it can definitely be demonstrated that at least one (otherwise unknown) Latin historical work was available in the very late first century A.D. in the form of a parchment codex,[19] the present form of the major works militates against our supposing that the large losses in those works occurred when Tacitus was in the form of a codex or codices rather than in the form of volumina.[20] A major objection is that, although we might be able to believe that as much as a codex was lost between *Annals* VI and XI, and also that as much as a codex was lost after the present end of the *Histories*, it is impossible to imagine that a codex of similar size ever existed for what is lost after the present end of the *Annals*. Even the most sanguine scholar would believe that there were no more than two books after *Annals* XVI, and many would think that the work ended with the sixteenth book itself.[21] It is also difficult to devise any adequate hypothesis for these losses if all thirty books had been incorporated into a single codex, although that possibility is not to be disregarded. The hypothesis would, however, have to account for these large losses from the text when in the form of a single codex, while most of the surviving books remained essentially intact.

[18] H. A. Sanders, "The Beginnings of the Modern Book: the Codex of the Classical Era," *The Michigan Alumnus Quarterly Review* 44 (1938) 95–111; C. C. McCown, "Codex and Roll in the New Testament," *Harvard Theological Review* 34 (1941) 221 f., 237 f.; C. H. Roberts, "The Codex," *Proceedings of the British Academy* 40 (1954) 169–204, esp. 177, 180. See now, on Martial's codex of some of his own books, W. Allen Jr., and the Martial Seminar, "Martial: Knight, Publisher, and Poet," *CJ* 65 (1969–70) 352 f.

[19] J. Mallon, "Quel est le plus ancien exemple connu d'un manuscrit latin en forme de codex?," *Emerita* 17 (1949) 1–8; *idem*, *Paléographie romaine* [*Scripturae Monumenta et Studia* III] (Madrid 1952) 77–92, 177 f., 180; also to be noted is that a fragment of Cicero is from a papyrus codex of the third or fourth century: W. H. Willis, "A Papyrus Fragment of Cicero," *TAPA* 94 (1963) 321–27.

[20] F. W. Hall, *A Companion to Classical Texts* (Oxford 1913) 18 f., states that such losses often took place before or when the material was transferred from roll to codex.

[21] R. Syme, *Tacitus* (Oxford 1958) Appendix 35.

If we reflect further, it is true that the losses at the end of *Histories* V, as well as those at the beginning of *Annals* XI and at the end of *Annals* XVI, could all have happened either in codex/codices or in volumina. The same is not so true of the few chapters we have left of the beginning of *Annals* V, along with the shorter loss at the beginning of *Annals* VI, as the text has been usually divided since the time of Haase.[22] The losses in *Annals* V and VI would appear to point in the direction of a time when the text was in the form of a volumen for each book and when each volumen confronted its own destiny.

The indications are, thus, that we are dealing with a period that makes us think of an era not too long after St. Jerome's statement[23] about thirty (intact) volumina. It would be possible to believe in a "collected edition" contained in thirty papyrus rolls. It is fair to assume that this edition was still completely in existence when Orosius in the early fifth century (Book 230–32) exhibited knowledge of portions of the *Histories* that are now lost.[24] If our assumption is correct that the losses in Tacitus' major works occurred when the text was still in volumina, it then follows that the text could not have been in the form of codex/codices before approximately the middle of the fifth century.

Except for what conclusions may be drawn from the losses that occurred in the major works, all present characteristics, especially of the Laurentian MSS, lead one to think of the archetype in the form of a codex or codices, a point to which we shall return presently. For the moment we should pause to note that it is not impossible to be

[22] *Philologus* 3 (1848) 152 f.

[23] The allusion in *Comm. in Zach.* 3.14.1, 2 [Migne, *Patrol. Lat.* 25, col. 1522] is less than pellucid for our purposes: *Cornelius quoque Tacitus, qui post Augustum usque ad mortem Domitiani Vitas Caesarum triginta voluminibus exaravit.*

[24] There is no way of determining the possible question of whether Jerome and Orosius knew the same copy of that edition, although the possibility seems unlikely. Since Orosius referred only to the *Histories* and without citing the book numbers that might give us a clue as to whether he possessed the "collected edition," he might have had only the copy of the *Histories* before him. It is hard to believe that Tacitus then existed both in the "collected edition" and in separate editions of the *Annals* and the *Histories*. The lives of Jerome and Orosius are close enough together that such considerations need not greatly disturb us. There is the subtle point, however, that Orosius' practice of explicit quotation indicates the use of codices rather than of rolls: Hall (above, note 20) 14 f. This fact raises the possibility that the loss in the *Histories* occurred later than the losses in the *Annals*.

convinced that at one period (mid-fifth century?) the major works of Tacitus had come to exist in only one set of defective papyrus rolls that contained only what we now possess. It is also possible to believe that the numbering XI–XXI already existed at that time,[25] for it must have originated in a "collected edition."

It must have been this somewhat tattered set of papyrus rolls that was copied on to parchment codex/codices. It is generally thought that the transfer of Latin authors from papyrus roll to parchment codex was completed in the fourth century of our era,[26] and that it was this transfer that determined the survival of the individual Latin authors.[27] If our hypothesis is correct, the transfer in this case came later than the fourth century, and not a moment too soon for Tacitus.

The transfer of incomplete papyrus rolls to parchment codex/codices facilitated the loss of the distinction between *Annals* V and VI. There soon was lost also the distinction between the *Annals* and the *Histories*, although the continuous numbering of XI–XXI was retained, with a lack of separation between XVII and XVIII in some of the fifteenth century MSS.

In view of all this, we must conclude that any new MS of XI–XXI can only be a new MS in so far as it brings us closer to this sole fifth(?) century codex. It cannot, in a sense, bring us much closer to Tacitus' autograph manuscript, for we have no way of determining how good was this manuscript in the fifth century. It, like other manuscripts of

[25] Oliver (above, note 10) 258–61 makes this point, but he thinks of the "consolidated edition" in the form of one codex, quite permissibly interpreting Jerome's *volumina* in the phrase *triginta voluminibus* as the equivalent of *libri*. In his admirable article Oliver is not concerned with the dating of the losses, and he is as cautious with his hypotheses as I also wish to be here. Except for the nature of the losses, I should agree with him on a single codex, for that would be preferable for the "consolidated edition." It should be repeated, however, that he wishes to place in the fourth century, or even in the third century, this codex (hyparchetype) that originally contained both the major works of Tacitus. (On the relationship between *volumen* and *liber*, cf. C. Wendel, *Die griechisch-römische Buchbeschreibung verglichen mit der des Vorderen Orients* [Halle 1949] 46–59.)

[26] J. W. Thompson, *Ancient Libraries* (Berkeley 1940) 58 f.; F. G. Kenyon, *Books and Readers in Ancient Greece and Rome*² (Oxford 1951) 112, 116 f.; cf. Roberts (above, note 18) 201–4. The fullest discussion of the transfer from rolls to codices is to be found in F. Wieacker, *Textstufen klassischer Juristen: Abhandlungen der Akademie der Wissenschaften in Göttingen*, phil.-hist. Klasse, 3. Folge Nr. 45 (1960) 93–138.

[27] Hall (above, note 20) 18 f.

that date, may, for example, have already contained interpolations and a corpus of variant readings.[28]

In one sense, all our efforts are confined to determining what happened to the manuscripts of Tacitus' major works between the fifth and the fifteenth centuries, and whether one of these fifteenth century manuscripts was in a tradition that paralleled but circumvented the tradition of the magisterial L 68.2 of the eleventh century. In a more valid sense, the period is rather, at its broadest, limited to the span between the mid-fifth and the mid-eleventh centuries. This constriction of time arises from the nature of the three inversions, described above in footnote 17. All three inversions can only have happened in a manuscript or manuscripts in the form of a codex, a circumstance that apparently was impossible before the mid-fifth century. All three inversions are present in L 68.2 of the eleventh century, but none of them occurred in L 68.2. Since they occurred in an earlier MS or MSS, we are generous in giving the mid-eleventh century as the *terminus ante quem*.[29]

All the MSS of this portion of Tacitus without exception have the first two inversions (in Book XIX), a proof that there was a single

[28] Hall (above, note 20) 59, 139. It is even more disturbing to realize that the author may well have been the source of some of the variant readings: G. Pasquali, *Storia della tradizione e critica del testo*[2] (Florence 1952) 395–465. Mendell (*YCS* 48 f.) of course from the start considered the possibility of variant readings in the archetype, a factor he subsequently employed to support his claims for Leidensis (*AJP* 75.262–69).

[29] Hence Dom Quentin (above, note 13) was sagacious when he undertook to specify that the MS in which the first two inversions occurred was similar to L 68.1. If we follow instead the hypothesis of Lowe that L 68.2 was copied directly from a Rustic Capital MS of the fifth century, the inversions must have occurred in that MS and the text of L 68.2 must have been copied from that MS by a scribe who failed to detect the inversions.

The perils of arithmetical inferences, however, cannot be ignored. While I have argued that the fragmentary state of *Annals* V operates in favor of a theory of the text in *volumina* at the time of the loss, those five chapters of *Annals* V contain about 70 (full) OCT lines. If the reader is willing to grant that the colophon and possibly part of *Annals* IV were written on the same folia, we find that we are dealing with folia not much different from those of the third inversion, where each folium (two modern pages) contained about 37 OCT lines. Yet these two folia must then have formed one single sheet containing a continuous text, and this cannot have been the surviving and innermost sheet of a gathering because the end of *Annals* IV is preserved, while it would have been lost if the rest of that gathering was lost. We must therefore reject the arithmetical inference that would lead us to think of this loss occurring in a codex rather than in a volumen.

tradition long enough for that event to occur in some codex. It can only have been after that date that there was the possibility of a division of the single tradition into two, one being the tradition of the Leidensis and the other being the tradition of L 68.2 and all the other MSS.[30]

The possibility of this double tradition will stand or fall on the individual reader's decision as to the history of the third inversion (in Book XX). The third inversion is either present in all MSS except Leidensis,[31] or the text of the MSS (Yalensis I and Yalensis II)[32] betrays the fact that the inversion had been corrected rather than that the MS had never had the third inversion at all. The eleventh-century L 68.2 has the third inversion, but the misadventure occurred in a predecessor of that MS. Only the text of Leidensis is in such a condition that Mendell can properly claim that it shows no sign of ever having had the third inversion at all.

Even before we make a detailed study of the third inversion, our study of the prehistory of the manuscript tradition makes us able to understand the awkwardness of Mendell's tentative stemma expounded in Book 344. He starts with the thirty-book edition (before the fourth century), and then allows for a common archetype, with the loss at both ends. That is followed by a copy that has the first two inversions, and that must be of the ninth century, if Mendell accepts Dom Quentin's theory. Then there has to follow a copy that lost XXI 23–26, because the Leidensis lacks these chapters and because the "Leidensis" tradition has to stem from this copy. The "Medicean"

30 It should be acknowledged that, although the several authorities have treated the ancestries of *Annals* I–VI and Books XI–XXI as different and distinct, I have here treated the problem in such a fashion as to indicate my opinion that the thirty book tradition in *volumina* must have originally produced a unified edition in codices/codex for all the then extant portions of the *Annals* and the *Histories*. Only R. P. Oliver in his article (above, note 10) has really approached a consideration of this question. It should also be of interest to note (Book 295) that the mutilations in *Annals* I–VI apparently took place before the date of the ninth century L 68.1.

31 The MSS of Group I do not enter into consideration here because they have a lacuna that spans the third inversion, although part of the inversion is preserved in an excerpt at the end (*YCS* 54, 56; Book 329).

32 In both cases there are imperfections in the corrections, but they are not the same imperfections in both cases. We have no way of knowing whether it was the actual scribes of those MSS who made the corrections, or whether it was their predecessors. The matter is more fully expounded later in the present paper.

tradition, which includes all MSS except the Leidensis, also has to stem from the copy that lost XXI 23–26. Then, according to Mendell, the third inversion (transposition) occurred in the "Medicean" tradition.[33]

After the third inversion had occurred, some of the MSS, as L 68.2 (Group III), recovered XXI 23–26,[34] some of them (Group I) further lost XXI 13–23, and some of them (Group II) continued to end at XXI 23, just like Leidensis. It is consequently very difficult to design a stemma that will allow Leidensis to have the first two inversions and yet never to have had the third inversion and that will at the same time account for Leidensis' having the same point of conclusion as Group II. I should add that this stemma seems to assume an unwarranted amount of activity in copying MSS of Tacitus before the fifteenth century.[35]

A further complication arises from the fact that in Sijthoff-Leidensis (p. XIV) Mendell now has concluded that we should place the archetype of L 68.2 and Leidensis in the period between the fourth and the eighth centuries. He has no space to present a new stemma, nor does he refer to the problem. While Mendell's new view would remove the troublesome losses and recovery, this view makes even more difficult the question of why Leidensis ends with the *potiorem* of Group II.

All general considerations tend to weigh in the balance against the

[33] Although Mendell (Book 239) seems to agree that the archetype of L 68.2 and the other MSS was the lost portion of L 68.1, he states in Book 342 that "the split in the tradition ("Leidensis" vs. "Medicean") probably occurred well before the date of 68.2."

[34] Recovery of XXI 23–26 would inevitably imply that this tradition had access to a MS older than the MS from which the "Leidensis tradition" descends. Yet it then used that older MS only to add XXI 23–26! This possibility must not, however, be dismissed as outrageous. Budensis 9, although apparently of Group I, has the ending appropriate to Group II (*YCS* 56, Book 300; although XXI 13–23 were added by a later hand: *YCS* 69); three MSS of Group III (formerly Group IV) have no lacuna in Book XX, but Laurentianus 68.5 and Urbinas Latinus 412 have the two excerpts (and Laurentianus 63.24 has one of the two excerpts) from that lacuna that appear at the end of the MSS of Group I (*YCS* 56 n. 13; Book 306 f. 313 f.).

[35] Apparently Tacitus aroused only slender and scattered interest until Boccaccio and the fifteenth century: E. Cornelius, *Quomodo Tacitus, historiarum scriptor, in hominum memoria versatus sit usque ad renascentes literas saeculis XIV. et XV.* (Wetzlar 1888) 36–43; F. Haverfield, "Tacitus during the late Roman Period and the Middle Ages," *JRS* 6 (1916) 196–201; M. F. Tenney, "Tacitus through the Centuries to the Age of Printing," *Univ. of Colorado Studies* 22 (1934–35) 341–63; Book 225–38.

independence of Leidensis from the tradition of L 68.2, and I have added some further points in my Appendix II. We are left, then, with just one large distinguishing characteristic, the third inversion.[36]

Leidensis entirely lacks the third inversion, presenting the text in the order in which it is given in modern editions, and it also lacks the clear clues by which we have been able to prove that the third inversion was corrected in the case of the Yale MSS (I and II) and that the Yale MSS were not descended from a tradition that never had the third inversion. Mendell is justified in emphasizing the astounding fact that Leidensis presents the text here as it appears in modern editions, with the exception of the last word of *Hist.* 4.53 (XX 53).[37]

If Mendell has demonstrated that the predecessors of Leidensis never had the third inversion, we have in Leidensis a tradition different from that of L 68.2. If we have any hint that the ancestors of Leidensis had the third inversion, and if we can find any sign of a corrected third inversion in Leidensis, then we do not have a tradition much different from that of L 68.2. I believe the second alternative to be the true one, and that it is demonstrated by one word in Leidensis, the last word of *Hist.* 4.53.[38]

[36] Mendell (*AJP* 75.253, Book 336) rightly emphasizes also that only Leidensis lacks four unnecessary words that the other MSS have at the end of Book XX: *neque uos impunitos patiantur.* (Some MSS have notes on these words.) The other MSS also have those words in their proper position in XX 77. I can offer no subtle explanation of the absence of those words at the end of XX in Leidensis, for it is easy but undemonstrable to state that a scribe omitted them as nonsensical at that point, while their absence in Leidensis alone also does not constitute strong proof. I have proposed a possible theory, at the end of my Appendix III, as to the manner in which the curious error originated.

[37] *AJP* 72.343 f., *AJP* 75.251–53, Book 329 f. To me the most striking point is that Leidensis gives *ferunt* in the phrase *sermone orasse ferunt* in the first sentence of XX 52 (*Hist.* 4.52). Although *ferunt* occurred in L 68.2, it is noted by Halm in the old Teubner (on *Hist.* 4.46) that it was Madvig who first observed that there was the proper location of the word; cf. H. Goelzer, for a fuller discussion of Madvig's work, in his edition of the *Histories* published by the Librairie Hachette (Paris 1920) 2.273. Older editions used *dicebatur* after *orasse.*

[38] Not to keep secrets from the reader, the word is *dicebatur,* which all the MSS, including Leidensis, read in place of *credebatur* at XX 53, immediately after the end of the third inversion. The *credebatur* is normally printed in modern texts on Doederlein's theory that the word was split into *crede* and *batur* by the occurrence of the third inversion, so that the *dicebatur* is merely an expansion of the *batur* while the fragmentary *crede* is preserved at a distance from its *batur* in some MSS (notably L 68.2). Heubner (Appendix I) is also convinced that *credebatur* is the correct reading in this passage, and that the presence of *dicebatur* in Leidensis serves to connect it with the tradition of L 68.2. Koestermann now prints *dicebatur* in the Teubner edition.

The question thus becomes: did the third inversion occur in the same manuscript as the first two inversions, or should we follow Mendell and claim that it occurred in a later manuscript which is the ancestor of all the MSS but Leidensis? Leidensis, in the case of the second alternative, would be descended from the tradition that had the first two inversions, which would still make it similar to the other tradition, but it would be unique in that it would be descended from the archetype that had the first two inversions but by way of a tradition that never suffered the third inversion.

Mendell (*AJP* 75.252) finds significance in the difference between the length of the pages in the third inversion and the length of the pages of the other two inversions.[39] I should point out that the length of the page in the second inversion is as much greater than the length of the page in the first inversion as the length of the page in the first inversion is greater than the length of the page in the third inversion.[40] We should also note that there is no variation in the length of the pages within each individual inversion.

My Appendix III appears to me to make it unreasonable to decide that the third inversion occurred in a different MS than did the first

[39] The differences in the numbers of modern lines may not be important, for Dom Quentin (above, note 13) also remarked that the folia of L 68.1, selected at random, do not give identical numbers of lines when their contents are matched against Halm's edition. L 68.1 itself, of course, has 24 lines to the page through Book III, and 25 lines thereafter.

[40] We are fortunate in having a parallel variation in the length of pages in the transpositions in the archetype of part of Cicero's correspondence, in the second book of the letters to Quintus, and in the fourth book of the letters to Atticus. The problem and its correction are expounded perhaps most conveniently in the Tyrrell and Purser edition (II[2] 276 f.). The differences in the relative page-lengths of the three inversions in Tacitus are less disturbing when we observe that there are similar differences in the page-lengths in Cicero, without producing general dismay. (L 68.2 itself does not always have the same number of lines on its double-columned pages.)

Another parallel is to be found in Livy, where some deviations must be admitted in estimating the numbers of lines involved in the columns in the lacunae in Books XXVI and XXVII and in the transposition (inversion) in Books XXVIII–XXIX (OCT ed. of Conway-Johnson, IV praef. vii–ix, xxi). In the apparatus criticus on XXVIII 22.14 it is observed that the transposition is in the correct order in some fifteenth century manuscripts, to which should be added, M. E. Agnew, "The Affiliations of the Spencer Collection's Corvinus Livy," *Bulletin of the N.Y. Public Library* 42 (1938) 324–6.

Even in poetry there is difficulty in calculating precisely the number of lines on pages transposed in an archetype: W. M. Lindsay, *The Ancient Editions of Martial* (Oxford 1903) Preface (Addendum), 5–12.

two inversions. The method by which all three inversions occurred is essentially the same, although not identical, since in the first two inversions it was the inside sheet of a gathering that was reversed whereas in the third inversion it was the next to the inside sheet that was reversed. While I can offer no proper explanation for the cause of the difference of the amounts on the pages in each of the three inversions, I am not sure that the dissimilarities are significant.[41]

Yet, even if the above paragraph is correct, Mendell is justified in demanding an accounting for the absence of the third inversion in Leidensis. In order to do this, we must review summarily what happened in the case of Yalensis I and Yalensis II, a matter that is discussed elsewhere in full.[42]

Let A represent the text of Book XX up through XX 46 *pecunia*, B represent XX 46 *tanta* through XX 52 *orasse*, C represent XX 52 *ferunt* through XX 53 *defuisse crede*, and D represent the remainder of Book XX. The proper order is A B C D, the inverted order is A C B D. All that the scribes of Yalensis I and Yalensis II (or their predecessors) had to do was to reverse the order of C and B, a procedure that was not easy (1) because B is three times the length of C and (2) because whatever MSS they used did not keep words in exactly the same positions on the pages as they had held in the MS in which the third inversion had originally taken place. The difficulty for the scribes was in knowing where A ended, where C and B began and ended, and where D began.[43] This difficulty caused the scribes who were responsible for the re-inversions to carry some words far from their proper positions by crediting them to the wrong units before

41 It should be noted that I do not attempt to deal with the theories propounded in 1927 by L.-A. Constans, "Remarques sur la 'préhistoire' du texte de Tacite," *Mémoires présentés par divers savants a l'Académie des Inscriptions et Belles-Lettres de l'Institut de France*, Tome XIII, IIe Partie (1933) 507–20. In particular, on pp. 518–20 he claimed that the variations in the amount of text involved in the three inversions would reflect a volumen rather than a codex, and that the amount of material on a page in the third inversion would be very small if written on both recto and verso. Hence there was writing on just one side, a fact indicating a volumen, with his additional suggestion that this papyrus volumen was cut up into leaves, perhaps to form a codex.

42 For Yalensis I see Allen *YCS* 33–36; for Yalensis I and Yalensis II see *YCS* 65 f., *AJP* 72.343 f., *AJP* 75.252, Book 329.

43 Mendell (Book 329) calls attention to the fact that nearly all MSS indicate the problem, but not at the correct junctures. Thus the scribe did not have to discover the displacement, but just its exact span and location.

re-inversion and also to lose some words (Book 329), doubtless because they made nonsense in their new and incorrect positions after the re-inversion. These tell-tale misplacements and/or losses do not occur in Leidensis, and we are consequently without that assistance in determining whether we are concerned with a corrected inversion.

All that remains to us is one word. It will be recalled that above I designated the end of C as *defuisse crede*. It is usually assumed that actually *credebatur* was split into *crede* and *batur* between the end of C and the beginning of D, just as the inverted section of the first inversion was placed between *Satur* (actually *satium*) and *nino* because the word was divided between the end of one page and the beginning of the next. The *crede* remained *crede* in the MSS of Group III (including L 68.2) in spite of its lack of meaning, but it was "improved" to *creditum quo* in the group of MSS termed "Genoan."[44] The hypothetical *batur* appears in all MSS, including L 68.2 and Leidensis, as *dicebatur*. In no MS do we find *credebatur*, although that is the usual reading of the modern text in order to account for *crede*.

It is this word *dicebatur* that urged Mendell to the hypothesis (*AJP* 75.253, repeated in different words in Book 327–30):

> Leidensis has none of the confusions caused by restoration. It reads *dicebatur* and not *credebatur*. It therefore seems probable that this was the reading of the archetype, that M, or more probably an ancestor of M, perceiving the need of a verb after *defuisse*,[45] started to write *credebatur*, saw his mistake when he had written *crede* but forgot to expunctuate the five letters.... The breaks then actually came between *pecunia* and *tanta*, between *orasse* and *ferunt*, and between *defuisse* and *dicebatur*.[46]

[44] Since Yalensis I largely belongs to this group of MSS termed "Genoan" from annotations referring to *Genua*, it may be of interest that there has been a dispute as to whether the city is Genoa or Geneva: (in favor of Geneva) H. Goelzer, "Du nouveau sur le texte de Tacite: Le *Vaticanus 1958*," *Bull. de l'Assoc. Guill. Budé* (1925), no. 8, 29, n.; F. Grat, "Nouvelles recherches sur Tacite," *Mélanges d'archéologie et d'histoire de l'École française de Rome* 42 (1925) 47 f.; (in favor of Genoa) L.-A. Constans, rev. of F. Grat in *REL* 4 (1926) 263; Quentin (above, note 13) 168 f.

[45] Perhaps the reader should here be reminded of the uncorrected inversion of L 68.2. The word *dicebatur*, which is the beginning of D, is in L 68.2 preceded by the *sermone orasse* of XX 52 and not by the *magnificentiae defuisse crede* of XX 53. The scribe of L 68.2 (and its predecessors) copied the strange jumble of "et prioris templi magnificentiae defuisse crede tanta uis hominum retinenda erat." It is no wonder that some MSS "improved" *crede* to *creditum quo*.

[46] It must be that Mr. Mendell wishes to retract *AJP* 72.344:

By this statement Mendell rejects the idea that the break at the time of rebinding came between *crede* and *batur*, with the *batur* subsequently amplified into *dicebatur*. He insists that *dicebatur* was the original reading, that the *crede* was a slip of the pen that has been perpetuated by subsequent scribes,[47] and that Leidensis preserves the original reading.

The existence of *crede* is worthy of some thought, for it could have come into existence either before or after the inversion took place. In the first instance, *defuisse crede* (the *crede* being of course superfluous but not expunctuated) would have been written at the very end of the bottom line of one page and *dicebatur* would have been written at the very beginning of the first line of the next page, and then the third inversion would have occurred after the writing of the MS was completed, and when it was being bound or rebound. In the second instance, if the *crede* came into existence after the third inversion took place, the scribe found no verb at all after *defuisse*, according to Mendell's hypothesis, and the scribe consequently began to supply one. Then the scribe, seeing that there was no verb in the MS from which he was copying, stopped after he had written the *crede*, left that word incomplete, and did not expunctuate it before he went on with his copying.

I find it to be too much of a coincidence that this scribe of the second instance should have taken this action at precisely one of the junctures of the inversion, and that later scribes should so scrupulously have retained the *crede* which ought to have been expunctuated; although I agree that someone was not so scrupulous with *dicebatur*, if we (and not Mendell) maintain that it is an expansion of *batur*.

By his stemma in Book 344 Mendell is debarred from the simple statement that the *crede* was first inserted by the scribe of L 68.2 itself,

> Leidensis does not show the transposition nor any of these signs of correction. It reads exactly like the text of today with one exception: it has *dicebatur* instead of *credebatur* which indicates a tradition different from all other manuscripts, a tradition which read *dicebatur* instead of *credebatur* and could not therefore have given rise to the *crede* of our manuscripts or of the *creditum quo* which developed from it.

All MSS have *dicebatur*, and no MS has *credebatur* (*AJP* 72.343).

[47] It bears repeating that this inversion did not first take place in L 68.2, that the breaks in that MS among A C B and D occur in the middle of pages, and that no one claims that it was the scribe of L 68.2 who amplified *batur* into *dicebatur*.

for he is unwilling to claim that all the MSS except Leidensis are descendants of L 68.2, a point on which I should concur, in the present state of our knowledge.

Certainly one of two alternatives is true—either *dicebatur* is the original reading, or the presence of a factitious *dicebatur* in Leidensis is evidence that in Leidensis or in one of its ancestors an extremely intelligent scribe was able to accomplish the re-inversion precisely and correctly.[48] And our entire estimate of the value of Leidensis will be eminently influenced by our choice of one of these two alternatives.[49] It is to be remembered that L 68.2 and the other MSS also have *dicebatur*. What Mendell's argument strongly relies upon here is the fact that Leidensis does not have *crede* or *creditum quo*, but neither has Yalensis II (*AJP* 75.252; Book 329).

The reader should also ponder whether *dicebatur* or *credebatur* better suits the syntactically involved passage at the end of XX 53, which of course relates to the rebuilding of the Capitolium:

> passimque iniectae fundamentis argenti [et] aurique stipes et metallorum primitiae, nullis fornacibus victae, sed ut gignuntur: praedixere haruspices, ne temeraretur opus saxo aurove in aliud destinato. altitudo aedibus adiecta: id solum religio adnuere et prioris templi magnificentiae defuisse credebatur.

[48] *AJP* 75.253:

> That the scribe of Leidensis should have recognized the transposition and attempted its correction would not be surprising. Several MSS note difficulties at the junctures and as we have seen two undertook to restore the text. But that the scribe of Leidensis could have ignored all the misleading corruptions which had crept in and could have hit upon the correct text which eluded all editors down to the last century is almost beyond belief. The alternative conclusion is that Leidensis alone of all our MSS derives from the tradition at a point antecedent to the mutilation, that is, before the eleventh century when M was written.

There are no "misleading corruptions" in L 68.2 in the sense of false readings. A scribe who wished to correct the inversion would have to decide what to do about *crede* and *dicebatur*, and he could pardonably have been confused by the notations that occur in L 68.2 at the junctures. A description of these notations, if Rostagno's photographic reproduction of the MS is not at hand, can be found in *AJP* 75.252, and in Allen *YCS* 32 f.

[49] The importance of this choice is shown by the fact that Mendell in *AJP* 75.253 says, on the strength of the absence of the third inversion, that Leidensis is the only MS to represent the tradition of the text at a stage earlier than that of L 68.2. He retreats somewhat from this extreme position on p. 270, but in the stemma on p. 344 of his book he makes it clear that Leidensis occupies a unique status.

It is my opinion that a MS that never had the third inversion would read *credebatur*.[50]

Although I believe I have demonstrated why we need not at present admit that Leidensis is descended from a progenitor of L 68.2, we are also left with several important but unanswered problems. In the matter of readings, all the MSS, but especially Leidensis, differ seriously from L 68.2 at some points. Are those differences the handiwork of scribes copying from L 68.2 (or from copies of L 68.2), or should we think of a twin of L 68.2? What, if anything, happened to the MSS of Tacitus between the eleventh and the fifteenth centuries? Yet in the fifteenth century we have three distinct groups of MSS, the third group possible of subdivision into Groups III and IV, and some of the manuscripts in contaminated traditions. How did all this happen? And did it all happen in just part of the fifteenth century? While I have found it highly improbable that Leidensis should emerge in the fifteenth century, independent of L 68.2 and of the other fifteenth century manuscripts as well, I likewise find it difficult without further investigation to subscribe to the statements that all the fifteenth-century manuscripts are descended from L 68.2 alone and that there were no other possible manuscript influences upon them. These, too, are problems worthy of attention.

APPENDIX I

Two Important Reactions to Leidensis

Mendell's theory of the value of Leidensis was accepted by E. Koestermann, who, with modifications, followed Mendell in his article, "Codex Leidensis BPL. 16. B—ein vom Mediceus II unabhängiger Textzeuge des Tacitus," *Philologus* 104 (1960) 92–115. Koestermann collated a "Fotokopie" of the MS, and in his moderate article devoted himself more to a consideration of readings than to the larger issues about the manuscript with which I am concerned. He incorporated many of the readings of Leidensis into the text

50 Employment of A. Gerber and A. Greef, *Lexicon Taciteum* I (Hildesheim, reprinted 1962), while not decisive for our problem, seems to indicate a Tacitean preference for *credebatur*, as is noted by R. H. Martin (above, note 7) 110 f. An examination of the words in *ThLL* shows that *credo* (4.1141) is more likely than *dico* (5.984 f.). R. Till cites E. Wölfflin, *Ausgewählte Schriften* (Leipzig 1933) 62, to support his use of *credebatur* at 4.53 in his edition of the *Histories* (Heidelberg 1963).

of his Teubner editions of the *Annals* (1960, 1965) and of the *Histories* (1961), with valuable discussions in the Prefaces.

The most detailed review of either edition, in this case of the *Histories*, is by H. Heubner, *Gnomon* 34 (1962) 159–63, and it deserves more attention than it has received. Heubner objects vigorously to Koestermann's use of Leidensis, and states that a reprint of Koestermann's 1957 edition is desirable. A principal point is that he distrusts Leidensis because it ends at *potiorem* in *Hist.* 5.23, along with Mendell's Group II of MSS. He believes that the *dicebatur* at the end of the third inversion is merely a scribal improvement on the *batur* that was left when the third inversion took place, just as *Saturnino* was split by the first inversion; and he is convinced that *credebatur* is required by the sense of the text. He further states that readings of Leidensis show dependence on L 68.2. In general, his approach is different from mine except that we agree on the crucial item that *dicebatur* shows that Leidensis has a corrected third inversion since the true reading is *credebatur*. Heubner is somewhat more vigorous in rejecting Leidensis than I should care to be at this time.

APPENDIX II

Our suspicions of the Leidensis are no little aroused by the fact that the MS is on paper[51] which is dated by its watermark to 1475–1481 (*AJP* 72.343; Book 307 f.), and by the further item that the MS does not seem to have been written in a hand appropriate to a professional scribe.[52]

We know that Agricola owned a copy of the *editio princeps* by Vindelinus de Spira (Wendelin von Speyer) and that he made notes in it. These notes came to the attention of later editors, who tended to attribute all of Agricola's readings to conjecture. This volume has now reappeared in the Stuttgart library.[53] The *editio princeps* would, however, have given Agricola a different text from what we have in his MS (cf. my Appendix IV). It does, however, end at *potiorem* in XXI 23, as does Leidensis.

[51] Mr. Mendell's descriptions in his book indicate that at least five of the other MSS are on paper.

[52] The handwriting is not that of Agricola (*AJP* 75.255). Wellesley (above, note 7) 302, is of the same opinion, although he declines to stress the question of the handwriting. I am inclined to agree with Mr. Mendell on this point, but see my Appendix IV for the contrary view. It might be added that it is perhaps of importance that Agricola's name does not appear at the end of this MS, although it occurs at the end of two Pliny MSS and at the end of the Stuttgart copy of the *editio princeps* of Tacitus.

[53] The peregrinations of both this volume and Leidensis have been elucidated by Hulshoff Pol in Sijthoff-Leidensis xviii f. (The book is in the Württembergische Landesbibliothek, Stuttgart.)

I do not see, on the other hand, why Mr. Mendell states (Book 330) that Agricola could not have used Puteolanus' edition, for that edition appeared about 1475–1480 (Book 351), and Agricola was in Italy then and did not die until 1485. The point is important since the edition of Puteolanus has the third inversion corrected according to the Genoan system, very like Yalensis I (Allen *YCS* 37; *AJP* 72.344; Book 329, 352). I doubt that Agricola did use the edition of Puteolanus,[54] certainly not throughout, because the second inversion is corrected in the edition and uncorrected in Leidensis, and because Puteolanus printed the section XXI 23–26 that is not in Leidensis.

APPENDIX III

Arithmetical Efforts

In *AJP* 75.252 Mendell found that there is a significant difference between the lengths of the pages involved in the first two inversions and the length of the pages involved in the third inversion. The issue is a basic one because, if all three inversions occurred in one manuscript, then Leidensis is presenting us with a corrected third inversion rather than with a text that never had the third inversion.

I have found that the lengths of the pages of the manuscript in which the third inversion occurred are as much shorter than the pages of the first inversion as the pages of the first inversion are shorter than the pages of the second inversion. My somewhat crude procedure was to try to count the number of (full) lines in Fisher's OCT edition. On this system each folium (two modern pages) in the first inversion contained about 41 lines, in the second inversion about 45 lines, in the third inversion about 37 lines. The system is only an approximation, but probably no other system would be more exact.

It should be remembered that L 68.1 has 24 (of its own) lines to the full page for *Annals* I–III, and 25 lines to the page beginning with IV, while L 68.2 in the *Histories* has 35 (of its own) lines to the page, except for two pages that have 34 lines and two pages that have 36 lines. One can only state that the predecessor of L 68.2 must have been less consistent, unless one wishes to conclude that all three inversions, although of the same type, occurred in three different manuscripts. As I have noted above in footnote

[54] It is of course true that in the fifteenth-century manuscripts were often copied from printed books: C. F. Bühler, *The Fifteenth-Century Book* (Philadelphia 1960) 16, 34–39. Since we know that Agricola worked with the *editio princeps*, it is only other considerations that prevent our stating that he could also have consulted Puteolanus' edition. Laurentianus 68.3 is a copy of the *editio princeps* (Book 370).

40, there is a similar inconsistency in the numbers of (modern) lines involved in the inversions in the texts of Livy and of Cicero's letters.

We may then, although always bearing in mind this distressing lack o uniformity, nevertheless consider it worthwhile to see if there is any clue to be derived from the study of the amount of text between the inversions, that is, between I and II and between II and III, also going on to investigate the amount of text between III and the point at which the MSS of Group I end, and the amount of text between the points at which the MSS of Group I and Group II end. There is little to be made of a study of the text between the endings of Group II and Group III, since the latter could well have been arrived at by a random fate.

I have worked with the idea of quaternions, i.e., what would appear in a modern text as sixteen numerical pages.[55] One must also remember that he is dealing in the first two inversions with the innermost sheet of a quaternion, with the result that his count will have to leave space for the uninverted portion of that quaternion (three folia). In the case of the third inversion we are dealing with the next to innermost sheet of a quaternion, with the result that there were two uninverted folia on either side of that inversion.

If we take the arbitrary figure of 41 (full) OCT lines to a folium (two modern pages), which is approximately the average length of the folia of the three inversions, we find that we have about 900 OCT lines between the actual first and second inversions. We can break this down into two quaternions of 328 lines each, plus 123 lines for the latter three folia of the quaternion in which the first inversion occurs, plus 123 lines for the first three folia of the quaternion in which the second inversion occurs.[56]

For the calculations between inversions II and III the figure of 40 OCT lines per folium works out more satisfactorily, and it is perhaps justified because the number of lines per folium is smallest in inversion III. We find that the space between inversions II and III is about 1160 lines. If we subtract 120 lines for the remainder of the quaternion of the second inversion, and also 80 lines for the first two folia of the quaternion of the third inversion, we are left with about 960 lines, which nicely divides into three quaternions of 320 lines each.

These computations seem strong enough to support the presumption that

[55] Koestermann, *Annals* 1965, p. xvii, states that the archetype was in quinions, without elaboration and in his exposition of the three inversions.

[56] Quentin (above, note 13) 176, thought in terms of three quaternions (i.e., two full quaternions and two half-quaternions), apparently (?) calculating from the middle of the first inversion to the middle of the second inversion. I have less than three full quaternions in amount because I have calculated from the end of the first inversion to the beginning of the second inversion.

all three inversions occurred in the same manuscript. Further computations are less rewarding but still worth reproducing since they tend in the direction of the same presumption.

If we are willing to continue to disregard the variations in the number of lines per folium in the three inversions and to use the arbitrary figure of 40–41 modern lines to the folium, we can profitably examine the amount of text between the point at which the third inversion ends and the point at which the MSS of Group I end, and then the amount of text between the endings of Group I and Group II. (I am, of course, disregarding the fact that there is a lacuna in the actual MSS of Group I.)

From the end of the third inversion to the conclusion of the MSS of Group I there are about 900 lines, while there are about 175 lines between the endings of Group I and Group II, and 50 lines between the ends of Group II and Group III.

If, for the sake of argument, we take 41 lines to the folium, we then leave 82 lines to take care of the last two folia of the quaternion after the third inversion, and we find that we have 162 lines left after taking into consideration two full quaternions at 328 lines each. These 162 lines are about half a quaternion, which would lead us to the possibility that the other half of the quaternion is to be found in the 175 lines between the endings of Group I and Group II. If we therefore add the 900 and the 175 for a total of 1075, and if we are further willing to take 41.5 lines as the average per folium, we then subtract 83 lines for the last two folia after the third inversion, and we can divide the remaining 992 by 332 with the result of three quaternions. (Quinternions do not seem convincing here, even if one computes with fewer lines per folium.)

This last paragraph is, however, disturbing for more than its cavalier arithmetic. It suggests, for instance, that the losses at the ends of Groups I and II indicate that those MSS are descended from the ancestor of L 68.2 rather than from L 68.2. It also suggests that those losses and all three inversions occurred in the same manuscript.

It should be noted that of course my computations would work as well for binions as for quaternions. If we follow this possibility, there would be a binion between the endings of Group I and Group II. The 175 lines here would produce folia of a content of about the size of the first two inversions and a little larger than the size of the third inversion. A binion would not be unusual as the conclusion of a manuscript, but it should be easy to lose half a quaternion just as simply, if this is the same MS as produced the three inversions in the process of a rebinding at an earlier date.

It is difficult to find anything of significance in the 50 OCT lines between the endings of Group II and Group III. This material was in a dangerous position, it is clear, whether we think of the preceding material as in binions or in quaternions. Nor would the last folium necessarily be filled up.

In his 1961 Teubner edition (pp. ix–xi) of the *Histories* Koestermann made a few arithmetical efforts. He examined the amounts of material involved in inversions I, II, and III, and he declined to risk an opinion on the significance of the fact that III showed a shorter page than I or II. He also counted lines between the endings of my Group I and Group II (*Hist.* 5.13 and 5.23), and he decided that those lines indicated four folia in the archetype, much as I have specified above.[57]

One further small matter of line-counting may be suggestive to some reader. It will be remembered that Leidensis alone lacks the words *neque uos impunitos patiantur* at the end of Book XX (Book 336), to which they were somehow pointlessly transferred from their proper place in chapter 77, where they also occur. If we count from that point in chapter 77 to the end of Book XX, we find that we have about 166 OCT lines, which would be four folia of the type we have been describing (cf. Koestermann, 1961, p. xi). It would appear possible to discover a mechanical problem of book-making as the solution to the problem of the occurrence of these four words at the end of Book XX. More important, we seem once more to be dealing with a MS in pages of a similar size.

We need not concern ourselves with a characteristic of the MSS of Group I, i.e., with the lacuna in Book XX and with the excerpts preserved from that lacuna. Mendell (*YCS* 56; Book 337 f.) has convincingly argued that part of the misadventure originated in a MS in quinions, a fact that I consider points to an intermediary MS rather than to the common archetype of all the MSS. The problem of the lacuna, however, should not be lightly dismissed, because the *toleremus* excerpt, as noted above, contains part of the third inversion in its inverted form; but the presence of the third inversion in the MSS of this Group suggests that the accident of the lacuna occurred at a later date than the date of the MS in which the third inversion occurred.

[57] In his reckoning on p. xi of *Hist.* (1961) Koestermann does not place much emphasis on the space between the words from 3.18 (XIX 18) which in L alone are pointlessly repeated in 3.23 (XIX 23), although he calculates that the interval indicates two folia of the archetype. In 3.18 the passage reads: *tam anceps prelii fortuna quamuis prospero fine equites equosque afflictauerat.* In 3.23 the proper *fortuna* of 3.23 has become *fortunam* instead, and it is followed by: *tam anceps prelii: quamuis prospero fine equites afflictauerat;* after which 3.23 continues correctly with *donec.* This interpolation could have occurred only in the copying from another MS because both passages in L occur in the middle of pages. It occurred because of the presence of the word *fortuna* in both places. The garbled interpolation could well, however, be of some importance for the provenance of L if it turns up elsewhere. It does not occur in the printed text of the *editio princeps* (see my footnote 59), and the words in 3.18 are in lines 5 and 4 from the bottom of the printed page.

APPENDIX IV

Dr. Elfrida Hulshoff Pol has done a great service by her searching study in her Addenda to Professor Mendell's Preface to the publication of the Leyden MS in the Scato de Vries-Lieftinck series. Her greatest achievement is a matter on which she does not dilate, the recovery of a volume for which previous inquiries at Stuttgart, including my own, had been futile—the copy of the *editio princeps* used by Agricola! She has consequently been able to display, in the plates at the end of the volume, the notations by three hands in the *editio princeps* that indicate the form of the third inversion as it stands in the MS in the correct form. She argues vigorously that this copy of the *editio princeps* precedes the MS, and that it had some marginal notations before Agricola had access to it. (At the outside, there is no more than a decade between the publication of the book and the copying of the manuscript.)

It is ironic that our complications are not readily and immediately resolved by the subscription at the end of the book, in red: *Rhodolphus Agricola recognovit.* We still cannot firmly decide whether the book was corrected from the MS or the MS was copied from the book.

Dr. Hulshoff Pol makes a good case for her claim that the final note is in Agricola's handwriting, although such proofs are never free from controversy,[58] and she further adds that, among the marginal annotations and readings by various hands *passim* in the book, Agricola's hand is found only infrequently.

The book, Dr. Hulshoff Pol concludes, did not belong just to Agricola or another single scholar, and consequently the annotations in it do not come from this MS. Put more strongly, the writers of the marginalia employed one or more manuscripts in their efforts. Agricola therefore had this book, partly corrected by others, partly corrected by himself, while he often added further changes when making his MS from this book, and also rejected some of the emendations he found in the book.

Dr. Hulshoff Pol distinguishes two further hands, in addition to Agricola's, in the annotations demarcating the accurate correction of the third inversion in the book. Agricola's was the second of these three hands, and the most extensively represented. This joint endeavor is the source of the absence of the third inversion in Leidensis.

Efforts to solve the problem by the use of readings are equally mystifying.

[58] In connection with a MS of Tacitus' minor works: B. L. Ullman, "Pontano's Handwriting and the Leiden Manuscript of Tacitus and Suetonius," *Italia Medioevale e Umanistica* 2 (1959), on pp. 326–35 discusses the styles of handwriting used by the single humanist. Cf. my footnote 52.

Dr. Hulshoff Pol believes that many of the extraordinary readings of Leidensis in fact come from this book (including its marginalia and corrections). Yet she adds that readings also show that Agricola's MS had several sources, other MSS in addition to the book. Some of Agricola's readings show that he had access to a MS of my Group I; and it is more disturbing that at least one of Agricola's readings perhaps shows a relationship to my Group III, for he then inexplicably failed to add chapters 23–26 to his own MS after *potiorem*.[59]

Before we continue to discuss the work of Dr. Hulshoff Pol, we may pause to note the contentions of Wellesley (above, note 7) 309–20 that, where the readings of the corrections of the *editio princeps* of Stuttgart are found also in L and in no other MS, the correctors were using L (or a kinsman of L); and that L did not use the corrected *editio princeps*, from which it differs at many points. The solution of the problem of the status of L and the other recentiores, as Wellesley comments, will require more work and thought.

It is imperative here to remind the reader once more of a point not discussed by Dr. Hulshoff Pol, the fact that the third inversion had also been corrected imperfectly three times, by the Puteolanus edition and by Yalensis I and Yalensis II. We can fairly presume that these corrections antedate the Agricola MS, although not by much, and they could perhaps better be described as early contemporaries of the Agricola MS.

Dr. Hulshoff Pol also does not remark on the surprising fact that Agricola, if he had the *editio princeps* earlier than the MS, failed to include the two minor works of Tacitus that are printed in that edition. It is, then, perhaps not entirely idle to wonder why he did not likewise have the edition of Puteolanus, which prints the longer text to XXI 26 as well as all three minor works.

I find it disturbing that the marginalia and corrections in the *editio princeps* proceed with such certainty, without any sign of experimentation even in

[59] She makes no use of what I should regard as a better touchstone, the garbled interpolation of a passage from *Hist.* 3.18 in 3.23, noted above in my footnote 57. That interpolation, if found elsewhere than in L, could lead to a new subgrouping of MSS. The interpolation in *Hist.* 3.23 does not occur in the printed text of the *editio princeps.* It makes no great difference if these words are copied by hand into the Stuttgart copy (I do not know if they are), since we should then merely become involved in a question of hands. Nor do the proper words in *Hist.* 3.18 (on the fifth and fourth lines from the bottom of the printed page) occur at such a point of the printed text of the *editio princeps* that the scribe could have made the mistake himself in copying. Nor do the words occur in L either in *Hist.* 3.18 or in 3.23 in such places that the scribe could have confused himself from one page to the next (140^v, 142^r).

Less significant is the interpolation that occurs in Leidensis at the beginning of *Hist.* 1.43 for Koestermann's apparatus there notes that Rhenanus had deleted it.

the case of the third inversion.[60] To me the implication of this fact would be of the correction of this copy of the *editio princeps* from another work. In that case we revert to Mendell's question, but now we are asking why another MS lacked the third inversion.

The new issue is going to arise from Dr. Hulshoff Pol's identification of Agricola's handwriting, a point on which not everyone will agree (cf. Wellesley [above, note 7] 302). It will also be possible to contend that Agricola had the MS before he had the book, even if we grant that the MS is in his writing, for it will be excessively difficult to demonstrate to universal satisfaction which hands in the book are earlier than Agricola's, and which hands are later. It is of the greatest importance that the texts of the MS and of the book are not identical.

I see no short way to answer these questions. There seems still to be so much authority in the manuscript Leidensis that it is best to treat it in the standard manner, as I have above. The full discussion is also necessary because we must devote thought to the whole history of the text of the major works of Tacitus, searching for help to explain all the MSS.

One point that is already being obscured in the prefatory remarks by Mendell and Dr. Hulshoff Pol, for example, is the important fact that the existence of the third inversion was approximately demarcated in L 68.2, and corrections attempted in two Yale MSS and in the edition of Puteolanus. Hence the striking thing about the notations in the *editio princeps* is not that the third inversion was corrected at all, but that it was corrected absolutely.

Another point is that Hulshoff Pol's demonstration of resemblances between L and the *editio princeps* is less impressive if we remember that the *editio princeps* also came from a MS, possibly similar to Venetus 381[61] but not containing the two minor works that appear in the *editio princeps*. It would thus be possible for all these resemblances, except Agricola's notations, to be to a MS rather than to the *editio princeps*.

[60] The *dicebatur* is neatly crossed out where it follows *orasse* in the inverted text of the *editio princeps*. The *crede* is also firmly crossed out after *defuisse* and *dicebatur* is there supplied in the margin. It is pertinent to ask: why didn't the corrector let the *dicebatur* stand where it properly precedes *Audita*? Then he would only have had to mark the division before *dicebatur* instead of after it. A possible answer to this question carries the implication that the book was corrected from the MS. This answer notes that in L *defuisse dicebatur* comes at the end of 174^v and *audita* is the catchword at the bottom while *AUDita* is at the top of 175^r.

[61] Suggested by Book 325. Wellesley (above, note 7) 306–8 argues persuasively that Spira printed from a now lost ancestor of Vindobonensis 49.