

few, and mostly obvious or unimportant compounds—such as, for example, *δωδεκαπήγυς* (Hdt.) (similar compounds are given), *φιλονία, -ουος* (Hdt., Plato); or some Aristophanic words like *φαλαρίς* (-ήρίς), *ναστός, ὄρχιλος, σέρφος, ὄψιμαθής* (Xen., Plato, Isocr.) should surely be included, the more so because—like *θορυβοποιῶ, ἀλιτενής, παράγραμμα, μικροψυχία* (or are Appian *B.C.* and the *Private Speeches* of Demosthenes now ruled out of school reading?)—it eases the problem of such Greek words in Cicero's correspondence as one can reasonably expect to find in a dictionary on this scale.

The Greek lettering, notably *ο, ρ, δ*, shows excessive contrast between thick and thin and thus gives an intermittent illusion of curvilinear slope to a basically upright fount. A new edition might well consider the advantage of a major change here in an otherwise excellent little work.

In the same format and at the same price, both nicely suited to the pocket, Mr. Handford gives us an entirely revised edition of the *Latin-English Dictionary*. Some prefatory pages deal clearly and not too technically with outstanding points in the history, pronunciation and accent, syllabic construction and prosody of the language. Spelling is touched on in two pages of notes, which also define the work as a 'substantially complete vocabulary of the most widely read authors of classical Latin down to A.D. 140'. The chief authors are listed, with dates. Expressly excluded are: (1) some words occurring only in surviving fragments of lost works; (2) most of the words found only in inscriptions, in technical writers (e.g. Cato, Varro, Seneca the Elder, Columella, Pliny the Elder, Quintilian), or Petronius; (3) a few rare technical terms occurring in certain authors (not listed) of the first two centuries A.D. 'Practically all' Plautine words of reasonable authenticity have now been included, on the basis of Lindsay's text. The result is a dictionary of

high quality and of commendably wide scope for its size. Whether all the material presented seems properly balanced will depend upon one's definition of a 'pocket' dictionary. A small but good dictionary such as this can be a valuable reference work for many outside the ranks of classical students; on the other hand, it is dangerous to suggest that classical students should use a small dictionary, inevitably lacking in its text such educational assets as names of authors, references, and quotations, beyond a relatively early stage in their studies—not, surely, extending to the reading of the whole of Plautus. Concentration on purely literary vocabulary is not easily maintained—one need instance only the inclusion of such technical words as *fistuca* (but not, of course, *fistucare*) as well as *festucula*, or *alica* (but not, e.g., *fertum*), *mal-leolus* 'fire-dart' (but not hammer-shaped 'slip' for planting), *factor*—and exhaustive treatment of one or a few writers reduces appeal to the non-classicist, who may with Julian seek 'more attention to the sciences'. Reasonably so, perhaps, when we are trying to lower barriers. Handford would doubtless welcome such wider appeal, as he would, one hopes, disagree with the suggestion found in the preface to the *Greek-English Dictionary* that it may 'lend occasional service to more advanced students when they are unable or disinclined' [*sic*] to use a more extensive work. If one questions some of the principles which Handford has followed it is only because from what he has given us one realizes his ability, by slight reorientation, to satisfy much wider needs.

The few misprints include *shērē* for *hērēs* (p. 152, head) and duplication of the entry *buccula*. The special symbols shown on p. 20 seem to have been neglected in the text.

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## CORRESPONDENCE

DR. PRETE writes:

In his review (*C. R.* 1956, pp. 128 ff.) of my edition of Terence, Dr. Skutsch offers what I consider to be unfair criticism. In his discussion of certain points, he betrays a lack of knowledge of current scholarship on Terentian problems. For the sake of brevity my remarks must be limited to parts of his

review, but I propose to publish in the near future a detailed reply to his objections. The nature of the subject lamentably makes it unavoidable that I speak of my own studies of Terence, but I would not have done that *si . . . non lacessisset prior*.

The first of my Terentian studies was an examination of the Bembo manuscript (Vat.

Lat. 3226). The results appear in the volume *Il codice Bembino di Terenzio*, Città del Vaticano, 1950 (Studi e Testi, 153). I approached and I believe I have solved the difficult problem of the correctors of the *Bembinus* (A) by a careful study of the scripts of the various hands and reached conclusions opposed to those of Umpfenbach, Hauler, and Kauer. What are we to say of the fact that Umpfenbach assigned the same kind of script to the 15th century, Hauler to the 9th, and Kauer to the 5th? Kauer in his enthusiasm for Iovialis, whom he discovered, went so far as to attribute to him corrections which in fact belong to different centuries (cf. *Il codice Bembino*, p. 37).<sup>1</sup> As for the content of the corrections in A, I have shown that some are independent of the Calliopian *recensio* ( $\omega$ ). These conclusions shed new light on the history of the text of Terence, as I pointed out in my article in *S.I.F.C.*, n.s. xxv (1951), 111–34.<sup>2</sup> Skutsch seems to know nothing of publications which are the foundation of the introduction to my edition. He asks that my work be checked by an expert. For the convenience of those who wish to do so the most pertinent *folia* of A are reproduced in *Il codice Bembino*. There are, moreover, numerous reviews of my publications which might have supplied him with information.<sup>3</sup>

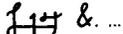
Instead of handling these fundamental issues he has confined his criticism to culling certain inaccuracies and misprints from my book and discussing them thoroughly. Even

<sup>1</sup> Kauer seems to have realized his mistake, for subsequently he distinguished between *Jov.* and *Jov*<sup>1</sup>. *Jov*<sup>2</sup>, however, does not appear in his edition. (Cf. J. F. Mountford, *The Scholia Bembina*, London, 1934, p. 118, n. 3.)

<sup>2</sup> The article was prepared at the request of the late Giorgio Pasquali who intended to include it in a revised edition of his *Storia della tradizione e critica del testo*. He was unable, however, to revise his work and his second edition is merely a reprint of the first with some general chapters added at the end.

<sup>3</sup> I quote two of the most important: A. Amatucci, *G.I.F.* lv (1951), 277–9 and A. Pratesi, *Doxa* iv (1951), 277–9. Perhaps Skutsch will be interested to know that Pratesi in his edition of Terence (Roma 1952) accepts completely the results of my research. He writes in his introduction: 'il testo della presente edizione si fonda in prevalenza sulla collazione dei manoscritti eseguita dall' Umpfenbach e dal Kauer. Non così per il Bembino, di cui una revisione totale era imposta dai risultati della recente indagine di Sesto Prete.'

so he is often superficial, misleading, and wrong. For example:

1. On p. 13 of the volume will be found the famous sign  & ...

I have read this as *J<ohannis> P<andon>j et <amicorum>*.<sup>4</sup> Skutsch writes: 'the discovery of the alleged signature of Porcellius . . . does not inspire much confidence'. My interpretation is based on the fact that it was the custom of owners of codices in the Renaissance to inscribe on the first folios of their manuscripts their own name, together with the phrase 'et amicorum'<sup>5</sup> and above all on the fact that the lines preceding the sign are *not* in Bembo's hand, as was pointed out not only by Hauler in 1889 but by Sabbadini in 1932; that the lines are those of Porcellius had been already suggested by the famous Gaetano Marini, primus custos of the Vatican Library in the time of Napoleon, as Dr. Skutsch might have learnt from my edition, p. 415.

2. Skutsch complains (p. 131) that I fail to note a fragment of the Bembo manuscript containing the initial portion of the last verses of *Andria*. I have discussed this fragment in an article 'Due frammenti Vaticani' (*Romance Philology*, 1955, pp. 260–3) and expressed my judgement on its value to the editor of Terence.

3. As for Skutsch's views on the Bononiensis (b), I once shared them myself, as he might have learned from my article 'Un nuovo codice di Terenzio' (*Rendic. Accad. Sc. Bologna, Sc. Mor.* Ser. v, iii, 1949–50, pp. 208–29). In consequence of a complete collation of the manuscript, however, I changed my opinion, and for a good reason. Many readings proper to A are found also in Bononiensis (b) together with a number of excellent conjectures later proposed by famous humanists. This latter is not unusual in manuscripts of the 14th century, but in (b) the number of such instances is extraordinary. For example:

<sup>4</sup> In *Il codice Bembino* (p. 13, n. 2) I noted (without realizing that Sabbadini had already done so) that it was possible that the sign indicates the price of the manuscript. In my edition of Terence I quote Sabbadini (*Le scoperte dei codici latini e greci ne' secoli xiv e xv*, Firenze, 1904, i. 146, n. 33): 'nella sigla L 14 et . . . che segue, io vedrei significato il prezzo di acquisto: Libris 14, con la cifra dei soldi cancellata.' Skutsch suggests this interpretation and adds 'if I am not mistaken' as if he were proposing something new, instead of reproducing a solution rejected in my footnote.

<sup>5</sup> G. D. Hobson, 'Et amicorum', *The Library*, iv (1949), 87–99.

*Eun.* 268 nimirum homines Ab (nimum h. *cell.*); 370 tu *om.* Ab; 377 duc *om.* Ab; 384 despectam Ab, schol. D (*despicatam cell.*); 402 gestare ACb (*gestire cell.*); 454 voce visa sum modo Ab (*vi. sum vo. mo. DGL, vi. sum mo. vo. EF*); 460 non moror ADb (*num mo. cell.*); 513 fecisse se *codd. (praet. Ab, om. se)*. I quote some more instances, taken at random: *Heaut.* 779 spondeo Ab (*despondeo cell.*); *Eun.* 67 mehercule Ab (*hercule cell.*); *Phorm.* 98 ancillulam Ab (*aniculam cell.*); only Ab indicate a new scene after *Hec.* 815, and in *Eun.* 644 Ab alone give the correct distribution of roles.

4. Skutsch objects to the designation of the second corrector of the *Riccardianus* as E<sup>2</sup> instead of E\* or (E). E\* would be misleading because D\* is used to indicate the hand, a different hand, supplying the *periocha* in D. Skutsch realizes that the situation in E is not the same as in D but insists that 'he (Prete) ought to have given an explanation'. The explanation is given on p. 56, where the corrector *recens* is identified as E<sup>2</sup>. The symbol recommended by Skutsch and used by Umpfenbach would also be misleading, for to indicate the hand supplying the *periocha* as E\* or (E) and the corrector as E<sup>2</sup> would give the impression that these symbols refer to different persons, whereas they are in fact the same. Instead of limiting his remarks to the 'slightly misleading symbol E<sup>2</sup>' he might have informed his readers about my collation of E which has yielded a considerable amount of new and, I hope, interesting information, as the apparatus testifies. E is, to my knowledge, the only manuscript which indicates the division into acts (cf. *Gnomon*, xxvii [1955], 93).

5. Skutsch asserts (p. 132) that my apparatus is compiled from those of Kauer and Marouzeau. Evidently he does not know my review of Marouzeau (*G.I.F.* ii [1949], 276–80). It would seem, however, that his own review is based solely on Kauer and Marouzeau. My apparatus for A, G, E, and b is based exclusively on personal research; I have also collated at first hand<sup>1</sup> much of D, C, and F as well as other manuscripts of Paris and Vienna by microfilm.

Some of Skutsch's objections to details are without foundation. Thus, at *And.* 927, I note the line division of the manuscripts. Skutsch

finds this surprising and asks 'by what divine afflatus either of these scholars [= Kauer, Mar., and P.] knows the line division of those manuscripts that have no line division'. Skutsch must know that A indicates the line division throughout; D, P, and F do so sometimes, to mention only the principal codices. It is not too much to assume then, that when mention of line division appears in the apparatus the reader will understand that reference is made only to those manuscripts which have it. On p. 131 Skutsch twice observes that where I have ascribed a reading to the editors (*editt.*) these readings are not those of Bentley. 'It is nowhere stated', he writes, 'that *editt.* means editors of the nineteenth and twentieth centuries' and, shortly below, 'the sentence *editt. . . mutant* is wholly redundant and misleading as well since *editt.* in the apparatus means, or ought to mean, all editors'. Now in the 16th century alone there were 193 editions of Terence, 40 in the 15th, 104 in the 17th, and 58 in the period between 1700 and 1799.<sup>2</sup> Does Skutsch seriously propose that modern editors collate these 395 editions?

7. I do not agree with Skutsch that the collation of v.b. or b.c. is useless. It was long thought by scholars (cf. edition p. 416) that b.c. was the *Bembinus*. In my earlier publications I made no mention of this manuscript and this omission was criticized by certain scholars (cf. *Doxa*, iv [1951], 84). It was in answer to these criticisms that I offered the collation of b.c. Skutsch would not have considered this 'foolish' if he were acquainted with the problems involved. Nor do I agree that the identification of v.c. with the *Basilicanus* (B) is 'manifestly wrong'. I collated B in part<sup>3</sup> and concluded that its readings are the same as those designated as v.c. in the edition of B. Filologo.

8. Admittedly there are misprints in my edition; the printing of a new critical apparatus presents many difficulties to a publisher who was doing his first work of this type. Some of the errors are obvious. Skutsch, I think, has devoted a disproportionate space to them. Thus at *Andria* 927 my apparatus

<sup>2</sup> I have taken these facts from the Bipontine edition of Terence (*Publii Terentii Africi comediae sex . . . studiis Societatis Bipontensis*, Biponti, 1, xi–xxvii); cf. L. W. Jones and C. R. Morey, *The Miniatures of the Manuscripts of Terence prior to the 13th Century*, Princeton, 1930–1, ii. 4.

<sup>3</sup> It was not easy to find it in the Vatican Library, since the editors, obviously working at second hand, refer to it as H 79. The correct signature is H 19.

<sup>1</sup> Since it has escaped Skutsch's attention I note here that in a miniature in C (f. 54<sup>v</sup>) I discovered, written in a very thin 14th-century script, the words 'vivo burgongne' which the late Prof. C. R. Morey considered very important for the history of the manuscript.

reads: is *in fine huius uersus habent codd.* (om. GV) *edit.*; *imit. uersus* 927 *pon. Linds. K. Mar.* Clearly, 927 should read 928. Skutsch complains that the impression is given that b is independent of the other Calliopian manuscripts and selects two places in which b appears in the apparatus to be isolated when, in fact, it agrees with the other manuscripts of the Calliopian family. In citing these errors he might have noted that in the apparatus to *Andria* 762 there is a lacuna before b (the letter  $\delta$  has been omitted). In view of what I have said, Skutsch's tone is hard to justify. It seems to betray a subjective attitude that is scarcely in keeping with the objectivity required of serious scholarship.

PROFESSOR SKUTSCH replies:

It will not do for Dr. Prete to lay the blame for his misprints, a small portion of the errors censured by me, at the door of his publishers. Knowing that they were inexperienced in this sort of work he ought to have read his proofs better.

Having shown that Dr. Prete's knowledge of Latin and of metre was totally inadequate to the task of editing Terence I proceeded to prove that his edition was lamentably inaccurate. The same inaccuracy is shown in his reply. Not with one word did I mention, either on p. 321 or elsewhere, the fragments of the Bembinus which he has since discussed in the *Journal of Romance Philology*. Nor did I call his collations of b.c. and v.c. 'foolish'. What I said was: 'If anyone should ever be so foolish as to collate hundreds of manuscripts of Terence in an attempt to get some order into the late transmission'. The difference, however, may not be obvious to somebody who does not distinguish between Bentley's and 394 other editions. The Librarian of the London Institute of Classical Studies says in connexion with the identification of v.c. with the Basilicanus, which I called 'manifestly wrong': 'I have checked the ten readings of v.c. given by Dr. Prete on p. 318, against a microfilm of the Basilicanus; three tally with the manuscript, seven do not'. I do not propose to argue other points with Dr. Prete and leave it confidently to future reviewers to confirm the charges which I have made; in the meantime compare E. Fraenkel, *S.I.F.C.* xxvii/xxviii (1956), 123 n. 2. But I am grateful to Dr. Prete for enabling me to correct a false impression concerning the alleged signature of Porcellius. L 14 is the obvious interpretation. My remark 'if I am not mistaken' was, as the editors of this journal know, substituted in the proof for 'as plain as a pikestaff'. In trying to find an expression of equal length,

at once more cautious and more polite, I overlooked that I might seem to be claiming for myself the solution which Dr. Prete in that very passage endeavours to replace.

PROFESSOR PAGE writes:

(1) Professor Davison tells us (*C. R.* n.s. vi [1956], 207) that he had to 'repress a shudder of repugnance' at the announcement of two more books on the *Odyssey*: which he nevertheless steeled himself to read and review; in what spirit, we can imagine; with what result, we shall see.

I spend no time over his exaggerations ('Professor Page is *almost always* polemical'); or his complacency (it is not for Professor Davison to inform me that my 'reputation as a scholar' is my 'own affair'); or his indignation *in vacuo* (I am 'at times unbecomingly rude to eminent scholars': this is false; or perhaps he will quote some examples verbatim, remembering that the standard of unbecoming rudeness is set by his 'academic troglodytes', 'numskulls', 'grubbing about', 'climbing on the Homeric band-wagon', and other such phrases in his few pages). These may be thought trivial matters: not so the following misrepresentations:

(1) Professor Davison asserts that I have set myself 'to answer a question which no longer has any real meaning, namely whether the *Odyssey* was composed by a single author or by a number of authors'; this is an 'obsolete conception', and 'the rest of us . . . have already assimilated the idea of an evolutionary "Homer"'. An author has the right to demand that a reviewer shall read his book less inattentively. What the analysis of the poem was designed to prove is that the *Odyssey*, in its present form, owes its coherence at certain points not to the poetical tradition but to a summary (and rather perfunctory) editorial process, acting on the 'evolutionary' material. The importance of the fact, if it is one, needs no stressing: the point was obvious to my listeners at the time, and has specially attracted the attention of readers, whether convinced or unconvinced. But I have evidently not made the point clear enough for Professor Davison to notice it: he actually thinks that I am trying to discover whether the *Odyssey* was composed by a single author 'in the sense in which Thackeray was the author of *The Newcomes*'.

(2) The error in the final paragraph of the review has already been noticed by others: 'Professor Page makes a great deal of play with the Pisistratean recension of the Homeric poems'—he does not; his reviewer has confused two very different things: (1) the evid-

ence (external, of course) for a 'Pisistratean recension', which I did not discuss, referring the reader to Merkelbach's admirable treatment of it; (2) the evidence (internal, of course) for a standard Athenian edition, dating from the sixth century or a little earlier, as established in infinite detail by Wackernagel's *Sprachliche Untersuchungen zu Homer*; that is what I was talking about, and nobody familiar with those topics can say that I have not made it plain enough. Professor Davison refers us to his article in *T.A.P.A.*, where the confusion is exhibited at full stretch. There he shows that he is unfamiliar with Wackernagel's great book, for he does not so much as mention it, and he makes a few remarks about Attic forms in Homer which he would not have made if he had read it. The 'Pisistratean recension' is a distinct and quite secondary problem: it is Wackernagel's work which justifies the claim of Cauet and Carpenter, that if there were no tradition about an Athenian edition we should be compelled by the internal evidence to invent one. To confuse these two matters is to display a fundamental misapprehension of one of the most important points in Homeric criticism.

(II) When Professor Davison does no more than misrepresent the entire contents of a book, nobody cares any longer. When he ends his review of my *Sappho and Alcaeus* by alleging that 'downright inaccuracies' are to be found in it, it is proper that downright falsehoods should be exposed. Three examples are given: (1) Ἀφροδίτη instead of Χάρις, an unimportant slip (and it required a great deal of ill will to call it anything else). (2)

'The misquotation of *P. Oxy.* 2294 at the top of p. 117': I hereby inform your readers that the quotation is absolutely correct. (3) 'The extraordinary slip by which *mollis* is taken with *flamma* at *Aen.* iv. 66, quoted on p. 29, n. 2': if the reader will turn to p. 29, n. 2, he will not find anything about *mollis* being taken with *flamma*; he will find the accurate statement that '*est mollis flamma medullas* has something in common with Sappho's *λεπτόν πῦρ*', and he would be right if he judged that this was the careful phrasing of one who knows (as Professor Davison apparently does not) that since remote antiquity it has been a matter for debate whether *mollis* agrees with *flamma* or with *medullas*.

In the light of these exposures, your readers will know what to think of anything else this reviewer may say.

PROFESSOR DAVISON replies with reference to (I):

Although I am very sorry that my review of *The Homeric Odyssey* has caused Professor Page so much distress, I cannot honestly say that I feel at all guilty. Anyone who compares what I actually wrote with Professor Page's version of it will see at once that almost all his grievances are figments of his own imagination; for the rest, I am well content that your readers should judge for themselves whether anything that I have written about Professor's Page's work exceeds, or even approaches, the severity of his own comments on the work of scholars with whom he disagrees (e.g. Professor Karl Reinhardt, *The Homeric Odyssey*, near the top of page 48).

## SUMMARIES OF PERIODICALS

### TRANSACTIONS OF THE AMERICAN PHILOLOGICAL ASSOCIATION

LXXXVI (1955)

J. A. Davison, *Peisistratus and Homer*: a thorough-going consideration of the evidence does not support the allegation that the text of the *Iliad* and *Odyssey* was first put together and reduced to writing in sixth-century Athens, but rather suggests that the version

which was adopted as the standard text for the Panathenaea was already in writing when imported for that purpose. D. W. Bradeen, *The Trittyes in Cleisthenes' Reforms*: the general purpose was to integrate the Eupatridae into the democracy by devising a system by which, since most of them belonged to city demes, they could have the maximum chance of serving as Prytaneis and also in the military organization. L.