ARISTARCHUS

AND THE

NUMERUS VERSUUM

OF HOMER

In accordance with the regulations for the degree of Doctor of Philosophy of the Australian National University, I hereby declare that the whole of this thesis is my own work, though with the benefit of such criticism and help from others as is acknowledged on pp. five below.

A thesis

submitted for the degree of

Doctor of Philosophy

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by

Michael John Apthorp

CANBERRA

September 1977
In accordance with the regulations for the degree of Doctor of Philosophy of the Australian National University, I hereby declare that the whole of this thesis is my own work, though with the benefit of such criticism and help from others as is acknowledged on pp. iv-v below.

M. J. Apthorp
SUMMARY

Preliminary Note: This Summary, like the Introduction, contains some deliberate oversimplification.

Bolling's demonstration that the *numerus versuum* of our Homeric vulgate, when shorn of the lines weakly attested by the post-Aristarchean tradition, is identical with that of Aristarchus has been strongly confirmed by recent papyrus publications. Some of these weakly-attested lines are demonstrably survivals from pre-Aristarchean times which have returned to the text, but the vast majority of the remainder probably entered the text for the first time in the post-Aristarchean period. In any case, the pre-Aristarchean survivals among the non-Aristarchean lines in our MSS. are themselves interpolations, since an examination of Aristarchus's *modus operandi* shows that he omitted only lines which were absent from the vast majority of his MSS., and such lines would have been spurious. In pre-Aristarchean times reciters' forgetfulness, copyists' slips and excision would have led to the omission of some lines from some of Aristarchus's MSS., but not enough MSS. to induce him to omit the lines. The possibility of very occasional exceptions cannot be completely excluded, but the chances that any given non-Aristarchean line in our MSS. is genuine must be virtually nil, *a fortiori* where the line cannot be shown to have been pre-Aristarchean.

A detailed consideration of various manuscript omissions which create difficulties for the hypothesis that Aristarchus did not omit genuine lines has left that hypothesis essentially unshaken.

There is widespread ignorance among Homerists about the significance of weak attestation by our MSS. This has had unfortunate consequences in certain discussions in the areas of formula-analysis, literary criticism and the Homeric Question. For example, the question of the authenticity of Od. 15. 113-19 is important for the question of the relationship between the Journey of Telemachus and the rest of the Odyssey, but the passage has usually been discussed with no appreciation of the significance, and sometimes with no apparent awareness of the existence, of the strong external evidence against it. If further discussions in these areas are to be soundly based, they must be based on a text which excludes the non-Aristarchean lines.
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I am deeply indebted to the Australian National University for the liberal Research Scholarship which I held from January 1971 to February 1974 and without which this thesis would never have been written.

I have benefited greatly from criticisms of large parts of this thesis in earlier forms by Dr. A.W. James, Dr. J.A.L. Lee, Dr. K.J. McKay and Mr. K.L. McKay. To the last-named, my Canberra supervisor, I am especially grateful, not only for his generosity with his time in discussing the subject-matter of the thesis with me, carefully scrutinizing my various drafts and making numerous valuable comments on them, but also for the generous way in which he helped me overcome the various practical difficulties caused by my move from Canberra to Adelaide in 1974. However, for whatever faults remain, ranging from the careless to the outrageous, I must assume full responsibility, the more so since none of the four named has seen my final draft (which is radically different from the versions they have seen) and since not every criticism persuaded me to alter what I had written.

To Sir Denys Page I owe a special debt of gratitude. Over the years - first while I was a student at Jesus College, Cambridge, and later by correspondence - he has constantly encouraged and helped me in the pursuit of my Homeric studies. I am indebted both to him and to Dr. Ann Moffatt for drawing my attention to some important work on the Homeric papyri and Alexandrian scholarship as soon as they became aware that I was turning my attention to the problem of the numerus versuum: this timely assistance undoubtedly smoothed my path considerably. To Mr. R.W. Barnes, the Canberra Classics Department's resident bibliophile extraordinaire, I am indebted not only for his prodigious efforts in building up the University Library's Classics collection but also for his generous practical help of various kinds. Indeed, I deeply appreciate the friendship, encouragement and practical help which I received from the entire staff of the Department.

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This thesis was typed by Miss Kathleen McKee: for her skill, patience and perseverance in coping so successfully with such intractable material I am very grateful indeed.

Finally, I am deeply indebted to my wife for her positively Penelopean πολυτλημοσύνη during the long gestation of this thesis. μάλιστα δὲ τ' ἔχλυεν αὐτῇ.
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Nothing solid can be built except on three foundations: first, the history of the transmission of the Homeric poems from remotest antiquity to the present day ... .

BIBLIOGRAPHICAL PREFACE

I shall usually refer to editions of Homer and of the Homeric scholia by simply naming their editors: for details see Sections A and B of the Bibliography at the end of this thesis. References to Allen's edition are to his editio maior except where otherwise stated. When referring to the edition of Od. 1-12 by Merry and Riddell, I have said simply "Merry" when the section of the commentary cited was written entirely by him (see p. v of his Preface).

In addition, the following abbreviations will be used:-


Companion: see s.v. "Wace" in Bibliography, Section D.
L-S-J: see s.v. "Liddell" in Bibliography, Section D.
Pack: see s.v. in Bibliography, Section C.
Pfeiffer: see s.v. in Bibliography, Section D.
RE: see s.v. "Wissowa" in Bibliography, Section D.

Wecklein: see s.v. in Bibliography, Section D.
West: see s.v. in Bibliography, Section C.

The abbreviations which I have used in referring to periodicals etc. are the standard ones: a list will be found in e.g. L'Année Philologique 45 (1974) XV-XXXII, though I have followed the English convention of writing P for the French Ph for periodicals with English titles (e.g. AJP, not AJPh).

When referring merely to part of an article rather than the article as a whole, I have refrained from giving its title in my Notes, but this information may always be found in the Bibliography.

Each Chapter is, basically, bibliographically independent. Except for the abbreviations etc. already mentioned, a work, when cited for
the first time within any given Chapter, is cited in full, and subsequent references to that work refer back to the note in which it was first cited within that Chapter. All references of the type "above, n. 15" refer back to Note 15 in the same Chapter except where otherwise stated.

In referring to papyri and other uncial fragments I shall normally adopt the following system: first, the abbreviation "Pap."; next, the number of the papyrus in the Allen-Collart-Mette list of Homeric papyri, viz.: Iliad: Nos. 1-122: Allen's edition; Nos. 123-315: P. Collart, RPh 6 (1932) 323-8, RPh 7 (1933) 61; Nos. 316-66: Collart, RPh 13 (1939) 289-91; Nos. 367-72: Collart on p. 57 of P. Mazon's Introduction à l'Iliade (Paris 1942), where he also reprints his earlier lists (pp. 47-57); Nos. 373-453: H.J. Mette, RPh 29 (1955) 193-9, with a few additions in Lustrum 1 (1956) 9 n. 1; Odyssey: Nos. 1-30: Allen's edition; Nos. 31-104: Collart, RPh 13 (1939) 294-7; Nos. 105-36: Mette, RPh 29 (1955) 200-202; I shall then give the number of the papyrus in the second edition (N.B.) of Pack's inventory. Collart in his 1932 article followed Allen's numbering of the Iliad papyri as far as No. 104 and thereafter diverged from him, but I shall have no occasion to refer to the papyri which Collart numbers 105-22, so that there should be no confusion. II. Pap. 1, the "Ambrosianus Pictus", and II. Pap. 9, the "Syriac Palimpsest", have no Pack numbers because they fall outside the scope of his inventory. When referring to papyri published too recently to be included in Pack's second edition I shall give a detailed reference to the collection in which the papyrus in question has appeared.
Preliminary Note: So that the main issues will stand out in bold relief, this Introduction contains some deliberate oversimplification: certain statements made here need to be qualified, and I do in fact qualify them in the body of the thesis. For the same reason I have left this Introduction completely free of footnotes and as free as possible of bibliographical details, which will be found in profusion in the body of the thesis and in the Bibliography at the end.

In 1925 Bolling published his External Evidence for Interpolation in Homer. In his Preface he explained what had led him to the study of this question: "This book has grown out of my own experience of a need. I desired to study the Homeric language with a view to determine what variations - if any - exist in different parts of the poems. For that purpose it seemed important to ascertain just which lines have a prima facie claim to pass as Homeric. The question, as I soon realized, could be answered for no single line without the evidence for all." I have had a similar experience myself. I desired to study the structural relationship between the Journey of Telemachus and the rest of the Odyssey. In the early stages of my investigations I came to realize that there were certain lines relevant to my topic which were omitted by a substantial proportion of our MSS. and/or papyri. A conspicuous example (to which we shall return in Chapter VI) is Od. 15. 113-19, the lines, repeated from Book 4, in which Menelaus announces his intention of giving Telemachus a very special mixing-bowl. They are omitted by the two extant papyri (saec. i-ii p.C. and saec. iii-iv p.C.) and by various mediaeval MSS. How are we to interpret this fact? Is the weakness in the attestation due to Alexandrian excision, as Van der Valk thought, or does it indicate that the passage is an interpolation, as Bolling thought? I soon discovered that there were numerous other lines present in the Wolfian printed vulgate of Homer but omitted by the majority or a substantial minority of our MSS. and/or papyri, and that the question of their authenticity was sometimes relevant to wider questions of authorship and structure. Like Bolling, I soon realized that the question could be answered for no single line without the evidence for all, and began to embark on a full-scale study of the problem.
I saw that these lines omitted by many of our MSS. were nearly all repeated lines, or at any rate highly formulaic, and not essential to their contexts; I knew that Milman Parry had shown Homer's great tolerance of repetitions, but that the Alexandrian critics tended to object to lines on the grounds that they were περιττοὶς or ἄλλοτεν μετευθηνεμόνοις; was one to assume, then, that these lines supported by only a segment of the post-Aristarchean tradition were genuine lines misguidedly excised by the Alexandrians? This turned out to be (roughly) the position of the most learned and prolific of contemporary Homeric textual critics, Van der Valk; and I confess that initially my instinctive sympathies were with this position. However, it was not long before I discovered that Van der Valk, for all his phenomenal learning, was - though I hesitate to say this of one so courteous in his disagreements with other scholars (RTSI I p. XI, II 644) - utterly lacking in sound judgment on this important matter, and that these weakly-attested lines really were interpolations. That had been the position of Bolling; and a careful and dispassionate study of the evidence showed conclusively that he was right.

Bolling's greatest achievement was his earliest one: the absolutely incontrovertible demonstration that the numerus versuum of our mediaeval vulgate, when shorn of these weakly-attested lines, is identical with the numerus versuum of Aristarchus. Through his careful collection and presentation of the papyrus evidence one could watch these interpolations growing: where a line was weakly attested by the mediaeval MSS. and papyrus evidence was available, then that line was almost invariably absent from the papyrus or papyri. Papyri published since 1925 have served only to confirm Bolling's position, sometimes dramatically.

But where do these additional lines come from? Did they all enter the text for the first time in the post-Aristarchean era? No; a few are demonstrably survivals from pre-Aristarchean times which have re-entered the text (in most cases via the scholia). But how many of the remainder belong to this class? Only a few, Bolling assumed in 1925, though he later became less sure of this. I shall argue that his early instinct on this point was sound. But of course even so it will still be possible to argue in any given case that a line added to the Aristarchean numerus versuum may be one of these undetected pre-Aristarchean survivals. We are therefore faced with the question:
what was Aristarchus's *modus operandi* when establishing his *numerus versuum*? Of course he *athetized* numerous lines - i.e. marked them with a condemnatory obelus but left them in his text - but what lines did he actually *omit*? The evidence shows conclusively that he omitted only lines which were absent from the vast majority of his MSS. This point Van der Valk refuses to accept, but he has never (as far as I can see) even tried to come to terms with the evidence for the extreme conservatism of Aristarchus in this respect as presented in Bolling's *External Evidence* and earlier by Ludwich. Actually Bolling did not so much *argue* for this conservatism as take it for granted, with only an occasional polemical thrust when discussing particular passages (see e.g. his p. 121); this is probably because he was more concerned in his book with the much more controversial and less obvious hypothesis of the conservatism of Zenodotus. I have therefore taken the trouble to *argue* explicitly for this Aristarchean conservatism in my thesis, going into greater detail than Ludwich did. But nearly all the evidence for it is assembled in Parts III and IV of Bolling's book, and enough of it had been adduced by Ludwich to prove the point, so that I find it difficult to understand how anyone who has read and digested these works can seriously doubt it.

Aristarchus, then, omitted only lines which he found very weakly attested. But can we automatically conclude that all such lines were interpolations? Not automatically, because Bolling's firm conviction that all Aristarchus's MSS. were descended in a purely written tradition from a single sixth-century MS. is open to question. But I shall argue that, even when due allowance has been made for the operation of oral transmission (and thus omission through a reciter's forgetfulness) in conjunction with the written transmission, the evidence points to the conclusion that lines so weakly attested that they were omitted from Aristarchus's edition would have been spurious.

It will be noticed that my main purpose in investigating the pre-Aristarchean tradition is limited: *it is not* to find out which of the lines which Aristarchus *included* in his edition (with or without an obelus) but which the scholia tell us were absent from some of his sources are genuine and which spurious; rather, it is to inquire whether any of the lines which Aristarchus *completely omitted* from his edition but which were present in some of his sources are genuine.
However, my investigations into the pre-Aristarchean transmission will incidentally lead to important subsidiary conclusions which go beyond the immediate aim of my inquiry. My emphasis on the possibility of accidental omission in oral transmission implies that one cannot jump as quickly or as confidently as Bolling does to the conclusion that an Aristarchean line is spurious when a scholium tells us simply ἐν τῶν οὐκ ἦν ὁ στίχος. I shall also show that Bolling's outright rejection of any possibility of deliberate pre-Aristarchean excision is unwarranted (although I shall also argue that this would not lead to omission by Aristarchus). On the other hand, the numerous lines absent from all our MSS. which we know to have been pre-Aristarchean but absent from Aristarchus's edition - some cited by the scholia, many more present in extant Ptolemaic papyri - stand condemned as interpolations alongside the weakly-attested lines of the mediaeval MSS. My consciousness of the importance of these subsidiary conclusions is one reason why I have investigated the pre-Aristarchean tradition so fully. Another reason is that, of my two aims, viz. to show (1) that only a few of the weakly-attested lines in our MSS. are likely to be pre-Aristarchean and (2) that Aristarchus did not omit genuine lines, the latter is more important for our verdict on the weakly-attested lines in our MSS., (a) because if I can succeed in establishing (2) I shall have built a second, insurmountable, wall capable of preventing any of these lines which may have succeeded in leaping back over the lower first wall into pre-Aristarchean terrain from reaching the Eden of authenticity which lies behind that second wall, and (b) because I believe the evidence for (2) is actually considerably stronger than the evidence for (1). Paradoxically, however, there may at first sight appear to be more obstacles to (2) than to (1) - anomalies, real or apparent, which must be satisfactorily accounted for if the hypothesis is to be vindicated. Many of these anomalies were adequately accounted for by Bolling himself, but some were not. My desire to account for these anomalies, especially those neglected by Bolling, constitutes a further reason why I have devoted so much attention to the pre-Aristarchean tradition.

However, my main focus remains on the weakly-attested lines in the post-Aristarchean tradition. My reasons for choosing to focus on them are two. First, it is against these lines that the evidence for interpolation is strongest of all. Secondly, in spite of the strength
of this evidence, there prevails among Homeric scholars in general an almost total ignorance about the whole matter. This last point needs elaborating.

It has often been remarked upon recently that Homeric studies tend to be pursued in rigidly separated compartments; scholars working in one compartment tend to ignore what is done in others. Thus it has rightly been said that in Germany both Analysts and Unitarians have until very recently almost totally ignored the work of the Parry-Lord school, and that the Parryites, for their part, tend to write off the work of the Analysts as an outdated eccentricity to an extent which is unjustified. One of the main purposes of this thesis is to argue that there is another yawning chasm which needs to be bridged— that between specialist textual criticism on the *numerus versuum* and the other areas of Homeric studies, especially research into the genesis of the poems, literary criticism and formula-analysis. Now I would concede that the exclusion from the Oxford Text of the lines weakly attested by the post-Aristarchean tradition (there are some 76 such interpolated lines in Allen's *Iliad* and some 93 in his *Odyssey*) will not normally make a very great difference to rather general studies in formula-frequency and formulaic patterns and sequences, but it will make a difference, and any study, if it is to be solidly based, must exclude these lines. However, for a very close and detailed literary study of a particular passage—and such studies are becoming increasingly frequent—the false assumption that an interpolated line is genuine can have crucial consequences. The same applies—perhaps surprisingly, on the face of it—to certain discussions of some crucial aspects of the Homeric Question: e.g. Is the Journey of Telemachus an integral part of the *Odyssey*? Did "Homer" dictate his poems to an amanuensis, write them out himself or secure their preservation merely by teaching them, entirely orally, to disciples? Illustrations of all this will be given in Chapter VI. Here I may merely remark that one of the saddest aspects of such ignorance about the *numerus versuum* is the way the Herculean labours of generations of scholars are going to waste. The publication of the Venetus A by De Villoison in 1788 and the enormous increase in our understanding of Aristarchus's *modus operandi* which this MS., and the painstaking study of it by scholars such as Lehrs and Ludwich, provided; the publication of Homeric papyri, both pre- and post-Aristarchean, by scholars of every nationality, the laborious
analyses of their readings by Bolling and his disciples (on the latter see West 16 n. 14) and Bolling’s brilliant interpretation of this evidence; the great increase in our knowledge of the Homeric MSS. which we owe to Allen – all this is going to waste as scholars still persist in accepting the Wolfian printed vulgate as equivalent to "Homer".

What are the reasons for this widespread ignorance? Perhaps the fact that each new hypothesis put forward by Bolling was less convincing than its predecessor has something to do with it. For example, his view on the relation of our vulgate to the edition of Aristarchus has won widespread acceptance among those who have taken the trouble to consider the matter (e.g. Gilbert Murray, The Rise of the Greek Epic [4th ed. Oxford 1934] 292f.; P. Chantraine on pp. 33f. of P. Mazon's Introduction à l'Iliade [Paris 1942]; R. Merkelbach, Gnomon 27 [1955] 269-75; West 16f.), although the strength of Bolling’s case has not usually been fully appreciated. His view (EE, ALI) that Zenodotus literally never omitted a line without manuscript evidence against it has been widely rejected or doubted. His final view that neither Zenodotus nor Aristophanes nor Aristarchus ever even athetized a line without manuscript evidence against it (see his ALI and AJP 70 [1949] 373f.) has not, as far as I know, been accepted by anyone: D.L. Page is sympathetic, but does not accept it without serious qualification (Homeric Odyssey p. 100 n. 17). Another reason for the prevailing ignorance must surely be the superficial attractiveness of Van der Valk's view which initially seduced me too, as explained above. But the main reason is probably the ignorance and irresponsibility of Homer's editors. This applies particularly, but by no means exclusively, to Allen's Oxford Text. When Allen was not writing bad books on Homer seething with violent anti-German emotion (the latter very properly censured by Gilbert Murray, CR 39 [1925] 73), he was seeking out, collating and classifying the alarmingly numerous Homeric MSS.: this was his life's main work. Yet when it came to printing the text, all these decades of toil came to nothing as far as the numerus versuum was concerned: he slavishly followed the Wolfian vulgate, except that he omitted a very few lines found in no MS. but culled from quotations. All the other weakly-attested Wolfian lines he printed in his text, without so much as bracketing a single one. Of course he gave the evidence against these lines in his
apparatus, where he also cited the numerous weakly-attested lines in our MSS. which Wolf had not accepted into his text. But the result of his printing in his text, unbracketed, the 169 or so Wolfian lines weakly attested by the post-Aristarchean manuscript tradition has been that most readers accept the resulting text unquestioningly as "Homer". We urgently need a revision of the O.C.T. Homer (or, failing that, a completely new edition) which relegates all these interpolations to the apparatus (mere bracketing should be applied only in more doubtful cases). When such an edition appears, scholars, future translators and students will all benefit considerably. Meanwhile, when reading Allen's Homer we must constantly keep one eye on his apparatus.

I shall now explain the aim and scope of each of my six Chapters, their relation to the progress of my argument, and the way in which I believe each advances our knowledge. But first a general word on this last point:- Van der Valk, being a generation younger than Bolling, is the most recent writer to treat the question of the numerus versuum in detail, and because of this there is understandably some evidence that he is being relied on as an authority by scholars in other Homeric fields who have presumably not made a detailed study of the subject themselves: see below, p. 198 (Gunn) and p. 205 (Heubeck): Τὴν γὰρ ἄλλην μᾶλλον ἔκλεισαν' ἀνθρωπολ. Ἡ τῆς ἀκουόντεσσο νεωτάτη ἁμφιέλληται. This tendency, though natural enough (we all have to rely on experts in other fields), I can, of course, given my own position, only regard as disturbing. For this reason I believe there would be value in treating the subject afresh even if it were only to result in a rehabilitation of Bolling's views on the weakly-attested lines of our MSS. against those of Van der Valk. However, I have already given some indication of the fact that my position, while coming close to Bolling's, does not entirely coincide with it. Further, even where I agree with Bolling's general principles I shall often have occasion to disagree with him on their application to individual lines. Finally, the publication of new papyri has often changed the picture since Bolling uttered his last word on a given line; and recent literature (apart from Van der Valk) has had to be taken into account.

In Chapter I, "Aristarchus and the numerus versuum", I shall summarize the arguments used by Bolling to prove that the numerus versuum of Aristarchus is the same as that of the post-Aristarchean
vulgate when shorn of its weakly-attested lines. I use the word "prove" advisedly: as I have said above, the strength of Bolling's case has not usually been fully appreciated (e.g. West 16 "It cannot be proved, but seems extremely probable, that Aristarchus was in some way responsible for the obsolescence of the eccentric texts"), so that, while a detailed reproduction of the material collected and arguments used by Bolling is presumably unnecessary, there would seem to be some value in an insistence on the cogency of those arguments. Secondly, I shall argue that the case for Bolling's position is actually even stronger than he made out: this is partly because he underplayed his hand in various ways, acknowledging anomalies where there were none, or (in some cases) were arguably none, and partly because the publication of new papyri has confirmed Bolling's general position not only by supporting his rejection of lines which were already weakly attested by the MSS. but also, in two cases, by spectacularly removing what really did seem to be anomalies at the time when he wrote.

Of course not every omission of a line by any post-Aristarchean MS. or MSS. is significant in the sense of indicative of interpolation: some allowance must be made for scribal carelessness. But how much? This is a preliminary issue which needs settling before the investigation can legitimately proceed, and I shall consider it in Chapter II, "Non-significant omissions in our MSS.". I shall argue that Bolling did not make enough allowance for scribal carelessness. Further, I shall consider whether we should allow for another kind of non-significant omission: is it possible that occasionally some post-Aristarchean critic, bred in the Alexandrian tradition but lacking the caution of Aristarchus, excised a genuine line on the basis of the internal evidence alone? Van der Valk believes that this did sometimes happen, Bolling that it did not: there would seem, therefore, to be some point in assessing the probabilities here, even though the importance of the question is minor, in that such excisions would have no greater effect on our MSS. than purely accidental omissions. I shall argue that the evidence provides no firm support for the hypothesis of occasional post-Aristarchean excision, but that it cannot be completely excluded. (The hypothesis of bowdlerizing, both pre- and post-Aristarchean, will be dealt with separately, in Chapter IV.)

In Chapter III, "Pre-Aristarchean survivals among the non-Aristarchean lines in our MSS.", I shall try to assess how many of the
lines weakly attested by the post-Aristarchean tradition, over and above those known from the extant scholia and pre-Aristarchean papyri, are likely to be survivals from pre-Aristarchean times which have re-entered the text. I shall argue that only a few are likely to come into this category. This was also Bolling's position in 1925, but the doubts which he expressed about this position in 1944, as well as his failure on both occasions to consider the question in sufficient depth, have made it necessary to return to the question.

In Chapter IV, "Did Aristarchus omit genuine lines?", I shall first argue that Aristarchus omitted only those lines which were absent from the vast majority of his MSS. Bolling took this for granted (e.g. EE 118f., 121); Ludwich had already argued the point in some detail. However, Van der Valk's rejection of this position has necessitated a return to the question. I shall argue that the case for Aristarchus's conservatism is even stronger than either Bolling or Ludwich realized - partly because of the publication of pre-Aristarchean papyri since Ludwich's time (II. 23. 92), partly because (I believe) Ludwich and Bolling did not always interpret the scholia correctly. However, we cannot simply conclude, without further ado, that Aristarchus never omitted genuine lines merely because he only omitted lines which he found very weakly attested: we must first ask whether accidental omission through forgetfulness in oral transmission or in the mind of a dictating reciter or, for that matter, in the copying of an influential MS. may not sometimes have led to a situation in which a genuine line was absent from the vast majority of Aristarchus's MSS., and the same question may be asked about pre-Aristarchean excision. In 1925 Bolling expressed his belief that all Aristarchus's MSS. were descended in a purely written tradition from a single MS. "not earlier than the sixth century" (EE 33f.), whereas in 1930 and 1932 Milman Parry argued that the textual divergences known to us reflect divergent oral versions of the poems. Bolling later stated his position even more emphatically with no concessions to Parry, and, more recently, West seems to have operated on the assumption that weak pre-Aristarchean attestation automatically (barring scribal errors) implies spuriousness. However, this cannot simply be taken for granted if (as I shall argue) it is not unlikely that many of the textual divergences known to us stem from slightly differing dictations of the poems. In effect Aly raised the same objection as long ago as 1926 in his review of Bolling's
EE (Philologische Wochenschrift 46 [1926] 1188f.), and in 1971 Rittoo argued that a particular non-Aristarchean line was genuine while conceding that it would have been absent from the majority of Aristarchus's MSS., attributing the latter fact to the activity of the rhapsodes (see below, pp. 160-64). Clearly, then, the whole question of oral transmission and its effects on the written transmission needs considering. Further, Bolling's assumption that there was no pre-Aristarchean excision on the basis of the internal evidence alone, either Alexandrian or pre-Alexandrian, either from typically Alexandrian motives or by way of bowdlerizing, needs to be challenged: I shall argue that in all probability some bowdlerizing did take place. However, I shall argue that even after allowing for omissions due to reciters' forgetfulness, scribal carelessness and excision, the chances that any given non-Aristarchean line known to us is genuine are virtually nil.

I must offer a word of explanation about my discussion in this Chapter of the connection between the divergences of oral versions and the divergences within the written tradition. In the light of the various hypotheses on the mode of this connection which I shall put forward as possibilities without reaching a conclusion about which is the most probable, the reader may be tempted to compare me with the Epicurean philosopher listing various hypotheses simply in order that the truth may be mentioned among them (e.g. Lucretius 6. 703-11). However, the limited nature of my aims should be borne in mind: they are, first, to show that Milman Parry's hypothesis of the oral origin of many of the written divergences known to us remains plausible in spite of the difficulties which it seems, at first sight, to encounter; and, secondly, to show that a line omitted through a reciter's forgetfulness, in whatever circumstances, would be extremely unlikely to be so very weakly attested as to miss a place in Aristarchus's edition. Neither aim requires me to plump for one possible mode of oral-written influence rather than another, and our lack of evidence makes it difficult to decide between them.

In Chapter V I shall consider some omissions which create difficulties for the hypothesis that Aristarchus did not omit genuine lines. This will involve me in denying that, or questioning whether, some lines apparently omitted by Aristarchus really were, and arguing that some non-Aristarchean or possibly non-Aristarchean lines which
have been thought to be essential to the sense are not, and that some non-Aristarchean lines allegedly excised by Aristarchus on the basis of the internal evidence alone were in reality omitted by him primarily because of the overwhelmingly strong external evidence against them. Such a discussion, already made necessary by the way Bolling ignored or glossed over certain difficulties, has been rendered even more necessary by various contentions of Van der Valk and others in recent years. It would be impossible, within the scope of this thesis, to discuss every non-Aristarchean line which Van der Valk happens to regard as genuine, and indeed, as I shall argue at the outset of this Chapter, it is unnecessary to do so; but I have aimed to consider every line which seems to me to present a difficulty worth taking seriously (though some such lines will instead have been considered in earlier Chapters under different headings).

In Chapter VI, "Applications", I shall show the relevance of my conclusions to the areas of formula-analysis, literary criticism and the Homeric Question by giving examples of the unfortunate consequences which ignorance of the textual history of the poems or erroneous opinions on the subject or inadvertence can have on discussions in these other areas. My treatment aims to be illustrative rather than exhaustive, but even so I know of no similar continuous general discussion on anything like this scale - though Bolling gave a few examples in passing (EE 1 n. 1, 3).

Finally, there are a few other matters which need explaining.

It did not seem appropriate that a Ph.D. thesis should contain a primer on the interpretation of the Homeric scholia explaining in detail points which professional Homeric textual critics take for granted. I have, however, given such explanations wherever it was possible to do so in a few words, but there remain a few matters which may puzzle the newcomer to Homeric textual criticism. However, reference to the relevant sections of Pfeiffer or the Appendix to Monro's edition of Od. 13-24 should suffice to clarify at any rate most of these matters; Davison in the Companion, pp. 221-6, is also useful, though a bit too brief.

I need to define the sense in which I use the term "post-Aristarchean interpolation": by this I mean a line or passage which (a) is an interpolation in the text of Homer, (b) was absent from the
edition of Aristarchus, (c) has been added to the text in the period after Aristarchus and (d) probably entered the text for the first time during this period. The "probably" in (d) implies that when I use the term "post-Aristarchean interpolation" I do not mean to completely exclude the possibility that the interpolation is a re-interpolation - one which originated in pre-Aristarchean times, was excluded from Aristarchus's edition and then re-entered the text.

The term "concordance interpolation", which I shall often have occasion to use, may not be familiar to every reader: I can do no better than quote Bolling's brief definition, slightly expanded (Il. Ath. 3 n. 6): "a convenient term for cases in which a scribe [or reciter] makes a passage agree with a [similar but] longer passage that he remembers".

In his edition of the Odyssey Allen often cites families of MSS. rather than individual MSS. as offering a particular reading, and I shall often be obliged to follow him, though the reader should bear in mind throughout that such citations need not imply that all the members of a cited family contain the reading in question: it is clear from the lists of specimens of agreement within the various families given by Allen in "The Text of the Odyssey", Papers of the British School at Rome 5 (1910) 1-85, that he regards agreement among a substantial proportion of the members of a family as enough to entitle him to attribute a reading to the family itself.
CHAPTER I

ARISTARCHUS AND THE NUMERUS VERSUUM

The text of our extant minuscule MSS. and vulgate post-Aristarchean papyri is essentially dependent on the edition of Aristarchus for its numerus versuum. The correspondence between the numerus versuum of Aristarchus and that of the post-Aristarchean vulgate (minus those lines where there is a significant weakness in the attestation) has been irrefutably demonstrated by Bolling. The evidence is of four types. (In presenting the first two here I ignore, for the sake of simplicity, certain exceptions which will be dealt with later.) (1) The thousands of lines which we know from the scholia to have been present in Aristarchus's edition, including those which we know to have been weakly attested in the pre-Aristarchean tradition, are all present in the post-Aristarchean vulgate (though some have been omitted from a few MSS. through transcriptional error, normally induced by a homoiographon). (2) The lines which we know from the scholia (and similar sources) to have been absent from the edition of Aristarchus are either totally absent from this vulgate or else weakly attested by it - and the further back we go in time to Aristarchus the weaker their attestation, so that we can see that they are interpolations in the post-Aristarchean text. (3) The lines in whose post-Aristarchean attestation there is a significant weakness receive no mention in the Aristarchean scholia - unless it be a statement that the line was not present in the edition of Aristarchus. (4) The demise of the "wild" or "eccentric" papyri with their high proportion of extra lines and the takeover by our vulgate in the latter half of the second century B.C. coincides with the death of Aristarchus: the first extant papyrus which clearly represents our vulgate, II. Pap. 271 (Pack 991), was written in about the middle of the second century B.C., and Aristarchus fled from Alexandria to Cyprus in 144 B.C. This is not to deny the possibility that the Alexandrians had access to MSS. which were relatively free from interpolations such as those found in the "eccentric" papyri: the various fragments which together constitute the third-century B.C. II. Pap. 41 (Pack 699) may come from a MS. of this superior class: '73 lines of our vulgate are covered; there is only one additional line, II. 4. 69a, and there is a remarkable agreement
with Zenodotus in the omission of 4. 89 and the corresponding Zenodotean wording in 88, which may well represent the original text. A third-century B.C. papyrus with no additional lines at all is Pap. Sorbonne 4, containing II. 12. 228-38, 246-65: it is noteworthy that it omits the line 250a found in the "eccentric" Pap. 121 (Pack 894). However, it is not essential to assume that there were some relatively uninterpolated MSS. in order to make sense of the Alexandrians' editorial activity, since if different MSS. were interpolated in different ways collation would still reveal a constant nucleus of lines present in all MSS., some lines which were present in most MSS., some which were present in a substantial minority and some which were present in only a few. In any event, we clearly cannot deny that the numerus versuum of our vulgate (when purged of its post-Aristarchean interpolations) is descended from the edition of Aristarchus by supposing that it is descended instead from some hypothetical pre-Aristarchean ancient vulgate with which the edition of Aristarchus happened to agree. There was no such vulgate (unless we are prepared to apply the term so loosely as to allow numerous fluctuations in the numerus versuum). This is clear, first, from the obvious dominance of "eccentric" texts with a high proportion of plus-verses in early Ptolemaic Egypt: to regard this as a purely local phenomenon, as Van der Valk does (e.g. RTSI II 483), will surely strike most as special pleading; and it is also clear from the evidence of the scholia, which record numerous pre-Aristarchean divergences from the edition of Aristarchus, some of them substantial and some of them strongly-supported. Those in the latter category are lines which Aristarchus's conservatism led him to retain in his edition although they were absent from the majority of his MSS. This is graphically illustrated by the case of II. 23. 92, to be discussed in detail in Chapter IV: the scholia tell us that this line was absent from all Aristarchus's better MSS., and it is also absent from both our two extant pre-Aristarchean witnesses for the passage, viz. a quotation by Aeschines and a papyrus of the third century B.C., yet Aristarchus included it in his text, merely marking it with a marginal obelus, and so of course it is also present in the entire post-Aristarchean tradition, but to claim that it was part of a pre-Aristarchean vulgate would be to fly in the face of all the evidence. Further, it is not only lines like these (and the whole argument
against the existence of an ancient vulgate) but also the virtually unanimous testimony of the post-Aristarchean papyri (and the strong support they normally receive from the early minuscule MSS.) against the weakly-attested lines of our MSS. which rule out the assumption of Van der Valk (e.g. RTSI II 503ff.) that the numerus versuum of our MSS. is primarily descended from that of the "archaic vulgate" and was only partially influenced by Aristarchus and that most of the lines omitted by a substantial minority of our MSS., and some omitted by the majority, are descended through direct manuscript tradition from this pre-Aristarchean vulgate. Nor is it possible to deny the existence of a causal relationship between the numerus versuum of Aristarchus and that of the post-Aristarchean vulgate (in its earliest form) by supposing that our MSS. are descended from some other recension which just happened to largely coincide with that of Aristarchus: the demonstrable agreements between the numerus versuum of this vulgate and that of Aristarchus are far too extensive to make this possible, and this point is graphically illustrated by a comparison between the edition of Aristarchus and that of his predecessor Aristophanes: it would seem that the numerus versuum of Aristarchus was very similar to that of Aristophanes, but at every point (there are six of them) where we know from the scholia that Aristarchus's numerus versuum differed from that of Aristophanes, whether by including a line which Aristophanes had omitted or by omitting a line which Aristophanes had included, our MSS. and post-Aristarchean papyri unanimously follow the version of Aristarchus, not that of Aristophanes.

Bolling, then, assembled the evidence provided by the vulgate post-Aristarchean papyri and found that, when a line is absent from some of our minuscule MSS. and the omission is unlikely to be accidental, and papyrus evidence is available, then the line is almost invariably absent from the papyrus or papyri; and he argued that all weakly-attested lines whose omission was not accidental must have been absent from the edition of Aristarchus: "An attestation of one of them in a papyrus is of value only in giving a terminus ante quem for the interpolation" (EE 11). Consequently, "Whenever two [of our] manuscripts differ - bar 'surface corruptions' - by the presence or the absence of a passage ... the passage has not been excised in the manuscript that lacks it, but has been added in the manuscript that
contains it." I accept this conclusion with the reservation expressed earlier, although quite apart from that reservation I do not always agree with Bolling's interpretation of the evidence in individual cases, in that I would allow rather more latitude for the operation of "surface corruption" or accidental omission than he does.

Bolling's general position on the relationship between our vulgate and the edition of Aristarchus has been strongly confirmed by the publication of papyri since the time when he first arrived at it. This point was made by Bolling himself in 1944 (ALI 10f.), and since then the evidence in his favour has continued to mount. Some examples will be given later in this work, but two must be singled out for special mention here, as they are cases where the papyrus evidence dramatically clears up what has hitherto appeared as an anomaly:

(a) Ode 11. 525 was absent from Aristarchus's text but mentioned in his commentaries, according to Didymus (Schol. H) - Ἀρισταρχός οὗτος οἶδε τὸν στίχον, ἐνώα δὲ τῶν ύπομημάτων. This wording suggests that the interpolation of the line in our vulgate had already started when Didymus wrote, and this is confirmed by the note of Aristonicus, περιγραπτέον ὡς ἄρτεπη (Schol. H), i.e. "It should be deleted ..." (see Bolling, EE 49, 21f.). With this early start, it is not particularly surprising that the line is read by all our minuscule MSS. and by Pap. 10 (Pack 1100, saec ii p.C.), which was the only known papyrus covering the passage in 1925. However, the unanimity of the support for the line in the extant post-Aristarchean tradition was spectacularly broken in 1967, which saw the publication of Pap. Ant. 170 (saec. iii p.C.), which covers Ode 11. 502-39 and omits 525 - twice, as it happens, for after writing 528, which starts κεῖνον, the scribe was trapped by the occurrence of the same word at the beginning of 522 into repeating 523-6, again without 525.

(b) There are various scholia on 11. 5. 808 stating or implying that Aristarchus did not read the line: Schol. T (Didymus) οὗ καθόλου δὲ εὑρέθη ἐν ταῖς Ἀριστάρχου, Schol. BCE364 οὗ καθόλου δὲ εὑρέθη παρὰ Ἀριστάρχου, Schol. A τοῦτον τὸν στίχον οὐχ εὑρήκατι καθόλου φασέν ἐν ταῖς Ἀριστάρχου, Schol. Αα ὁδὸς ὁ στίχος οὐδαμῶς εὑρέσκεται ἐν τῷ Ἀριστάρχου, and Schol. A (Aristonicus) on 807, δὴ ὡκλὴ περετστιγμένη, δὲ τὰ ζηνόδοτοι ὑποτάσσει τούτῳ στίχον ἰδέδες ..." [808]. However, the line is present in all but two of our minuscules, the exceptions being L9 (1452 A.D.) and Allen's "independent MS." V16 (saec. xii),
and Bolling took these omissions to be merely accidental. Further, there is an A scholium (Aristonicus) on ll. 4. 390, ὁ ἀστερίσκος, ὅτι ἐνταῦθα ὡς τετακταν, ἐν ὡς τῷ τῆς Ἀθηνῶς πρὸς τὸν Διομήδη λόγῳ [ll. 5. 808] οὕκετι, which would normally imply that Aristarchus read ll. 5. 808 but marked it with an obelus and asterisk - though this would of course contradict the evidence of the other scholia. Hence Bolling concluded (1) that the original form of the Didymean scholium on the omission was Schol. A's ... ὃν ἐνρῆσαν καθόλου φασόν ..., and (2) that "the sentence must have been the introduction to a note correcting the belief that the line was non-Aristarchean. There is no reason to doubt that the line stood in the second edition of Aristarchus (at least) and was there athetized." Now when Bolling wrote this in 1916, and when he returned to the matter in 1925, no papyri covering the passage had been published, but in 1950 (ll. Ath. 4 n. 10) he was able to report the evidence of a single papyrus, Pap. 295 (Pack 755, saec. ii-iii p.C.), which contains the line. This seemed to lend support to his view that the line was present in the second edition of Aristarchus. However, we now have two more papyri covering the passage, and both omit the line: Pap. 408 (Pack 765, saec. ii p.C., published in 1947 but understandably unknown to Bolling in 1950) and Pap. 400-401 (Pack 736, saec. iii-iv p.C., published in 1954). These two papyri completely change the picture: the five scholia asserting the absence of the passage from the text of Aristarchus can now be seen to have been supported by the early post-Aristarchean tradition, and it is arguably most natural to suppose that the omissions by the two mediaeval MSS. are directly descended from this tradition, especially as the homoiographic temptations to accidental omission are so slight. If we still wish to accept Bolling's tentative 1916 suggestion of a difference between the two editions of Aristarchus at this point, we shall have to modify it by supposing that the line was present in his first edition and absent from his second, because we happen to know that it is the second edition on which our numerus versuum is based. In this case we could assume that the scholium of Aristonicus on ll. 4. 390 quoted above stems from the ὑπομνήματα on Aristarchus's first edition. But I think a better explanation of the situation can be given. Although Aristarchus's simple asterisk (meaning, basically, "I judge this line to be genuine here but spurious in another place") normally implied that the counterpart of the asterisked line was contained in his text
with obelus and asterisk, I think it probable that on this occasion he used it merely to foreshadow his disagreement with Zenodotus and that its only corresponding sign was the διπλή ξερωστιγμόνη (signifying disagreement with Zenodotus) attached to II. 5. 807. The fact that Aristarchus's distinguished predecessor had included II. 5. 808 in his text would suffice to explain why Aristarchus thought the parallel line II. 4. 390 worthy of an asterisk. This enables us to take the five scholia asserting the absence of the line from the text of Aristarchus - including the two which say ἐν ταῖς Ἀριστάρχου (i.e. in both his editions) - at their face value. The net result is paradoxical: while the special pleading in which Bolling felt forced to engage in order to harmonize the evidence of the MSS. with that of the scholia has been revealed by the two most recently published papyri to be unnecessary and misguided, this very fact has strikingly vindicated his general position on the relationship between the numerus versuum of Aristarchus and that of the post-Aristarchean tradition.

Further, in considering the evidence for the Odyssey Bolling has made his general position appear less strong than it actually is. On his list, IIb, of "probable interpolations" in the Odyssey (AJP 37 [1916] 453), compiled on the basis of slight weaknesses in the manuscript attestation, he remarks (EE 11) that "ten of the lines in it are found in papyri while but one (φ 308) is omitted". He also lists (EE 13) as "known to be non-Aristarchean" five lines present in all our MSS. and not omitted by any of the papyri published by the time he wrote. For Bolling's explanation of these apparent anomalies see AJP 37 (1916) 457 and EE 13f. His main argument - that "the papyri of the Odyssey are not extensive in the early periods" (EE 13) - is, as far as it goes, just, and the fact which it states remains true today; and it received striking confirmation when (as we have just seen) a papyrus turned up which omitted one of the lines in his EE 13 list, Od. 11. 525. However, there are two ways in which Bolling has underplayed his hand:-

(1) Of the 'ten ... lines ... found in papyri" within his 1916 list of "probable interpolations", most should in fact be regarded either as probably genuine (and their omission as probably accidental) or as at least as likely to be genuine as interpolated. Bolling's own 1925 notes on Od. 14. 451 (EE 213; note also that both MSS. which
omit the line are as late as saec. xv), 21. 189 (EE 215; also, the single MS. which omits the line is again saec. xv), 21. 381 (ibid.) and 22. 37 (EE 215f.) imply a reversal of his earlier judgment on four of the lines in his list, and his failure to point this out at EE 11-13 is misleading.

(2) What of his five lines found in all our MSS. but "known to be non-Aristarchean"?

(a) In Chapter IV I shall argue that one of them, Od. 4. 511, was Aristarchean; since Bolling wrote, the line has turned out to be present in Pap. Ant. 168 (saec. iv-v p.C.), a fact which favours my view rather than Bolling's.

(b) I regard it as highly probable that another line in Bolling's list, Od. 10. 243, was also read by Aristarchus. The subject-matter is Circe's feeding of the companions of Odysseus whom she has turned into pigs:

\[ \text{τοῦσι δὲ Κύρη} \]
\[ \text{πάρ ὧν ἐκλυόν μᾶλλον τ᾽ ἐβάλεν καρπάν τε κρανεῖς} \]
\[ \text{ἐδομεναί, οἷα σὺς χαμαλευνάδες οἷν ἔδοουν.} \]

Schol. HQV (Didymus) on line 242 (sic) says: 'Ἀρισταρχὸς οὐκ οἶδε τὸν στίχον. ὃ δὲ Καλλιστρατὸς ἀντὶ αὐτοῦ γράφει "παντὸς μὴν [παντοῖς coni. R. Schmidt] ὕλης ἐτίθαι μεληθέα καρπάν". In its present form this scholiom obviously cannot be right, for a text which simply omitted 242 would be arrant nonsense. Hence the emendation proposed by M. Schmidt and followed by A. Ludwich, 'Ἀρισταρχὸς οὖτως οἶδε τὸν στίχον, has great plausibility. However, Van der Valk (RTSI II 487f.), while acknowledging that this emendation is palaeographically plausible, argues that it is unlikely to be right because the paradosis is supported by the scholium on Od. 11. 525 (quoted above), where the same phrase οὖν οἶδε appears. This argument, however, can carry little weight, since Didymus is fond of using the term οὖτως in connection with Aristarchus's readings, as Van der Valk is aware (e.g. TCQ 167-70), and the phrase οὖτως οἶδε(ν) occurs elsewhere in the scholia in the same sense as here - Schol. A on 11. 9. 426 (bis) and 10. 252. Further, Van der Valk is still left with the difficulty of the nonsensical text which results if line 242 is omitted, and is forced to adopt the old expedient of supposing that at the end of 241 Aristarchus read τοῦσι δὲ δῶκε or δῶκε δὲ Κύρη although there is no
mention of this in the scholia and no trace either of such a reading or of the omission of 242 in our MSS. Bolling's solution (EE 210) was to move the scholium one line down and make 243 the line omitted—a view in which he had been anticipated by Meierheim25 and Kirchhoff.26 Then of course the version of Callistratus becomes a substitute for 243, not 242; and "in either form the plus verse is a needless gloss" (Bolling). But even if Callistratus's version is spurious, it is extraordinarily inept and banal if it follows 242, whereas as a substitute for 242 it gives good enough sense. A disadvantage which Bolling's position shares with Van der Valk's is the unanimous testimony of our MSS, in favour of both 242 and 243, whereas with the Schmidt-Ludwich emendation this is precisely what we should expect. There was no papyrus covering Od. 10. 242f. in 1925, but we now have Pap. 115 (Pack 1086, = Pap. Berol. 11754, saec. vi p.C.), which Mette27 lists as containing 10. 217-68: it presumably reads both 242 and 243, as Mette, on the authority of Schubart, normally records the noteworthy variants of the unpublished Berlin papyri in his list (see e.g. his notes on II. Pap. 449, which include a mention of the omission of II. 21. 480), and of Od. Pap. 115 he says merely, "Schräger Strich am Wortende." Probably, then, Aristarchus read both 242 and 243.

(c) There is in fact no adequate evidence in support of the contention of Bolling (EE 209f.) and others28 that Od. 10. 189 was not read by Aristarchus. What is the evidence? First, an Η scholium on the line: Καλλιστρατός ḿπούν ὡς ὑπὸ τυνος ὁ στίχος προτέτακτος ἀγνοοῦντος τὸ Ὀμπρυκόν ἔθος, ὡς θέλει ἂρξεθαί ἀπὸ τοῦ γάρ. Callistratus is of course entitled to his opinion—a reasonable enough opinion in the light of the awkward double vocative (though one might compare Od. 11. 473f., 617f.); but this need not imply that he found any external evidence against the line. Secondly, Bolling claims that "the scholiast on Euripides, Phoen. 886 ... and Aristonicus (Schol. A) at P 221, both cite x 190 to illustrate Homer's way of beginning a speech with a γάρ-clause" (my emphasis).29 I have underlined "speech" because for it one must substitute "thought-sequence"—a difference which is crucial to the point at issue. II. 17. 220f. run: κέκλυτε, μυρία φίλα περιτιττών ἐπικούρων· ὦ γάρ ἔγω ..., and Aristonicus comments on 221: <ἡ δικλη> δτι ἀπὸ τοῦ γάρ ἱρθονι, ὡς καὶ ἐκ' ἐκείνου ὡ φίλοι, ὦ γάρ τ' ἄδειν δὴ τις ἐφ' [Od. 10. 190]. The point which Aristarchus was making here was that the γάρ in II. 17. 221 does not
refer back to the preceding line but is part of a new beginning; and, especially as the preceding line, like Od. 10. 189, consists of κέκλυτε + a vocative, it is arguably most natural to interpret Aristonicus's scholium on ll. 17. 221 as follows: "γάρ here does not refer back to line 220 any more than γάρ in Od. 10. 190 refers back to line 189: in both places it is part of a new beginning." If this is right, then of course Aristarchus read Od. 10. 189. But what about the Euripides scholium mentioned by Bolling? It runs: έξος ποιητικάν το ἀπὸ τοῦ γάρ ἄρχεσθαι. "Ομηρος: "ἐδείκτε, οὐ γάρ τ' ἔδωκεν δὴν τοῦ" [Od. 10. 190] ... But the Euripides line being commented on, Phoenissae 886, comes in the middle of a long speech by Teiresias, so that the scholium's ἄρχεσθαι must, again, refer to the beginning of a thought-sequence rather than of a speech. It follows that the scholium need not imply a text of the Odyssey which lacked 10. 189. We still have no papyrus covering the passage; future papyrus publications may change the picture, but the evidence available to us at present — including the unanimous manuscript attestation of the line — suggests that Aristarchus probably read the line.

(d) Od. 11. 525 is indeed non-Aristarchean, but, as we have seen, its omission by a recently-published papyrus means that its status is no longer anomalous.

(e) That leaves Od. 8. 142 as the only clearly non-Aristarchean line in the Odyssey attested by the entire extant post-Aristarchean tradition. There are still no papyri covering the passage, but the unanimity of the later witness for the line is understandable in the light of the wording of the scholia, which seems to indicate that the interpolation had already made considerable headway by the time of Didymus.

We come finally to the problem posed by the fact that the intra-linear readings (i.e. readings not affecting the numerus versuum) known to be Aristarchean have not had a great deal of influence on our vulgate — probably many of those that do appear in the minuscule MSS. have been taken over from marginal scholia. How, then, are we to account for the influence of Aristarchus's edition on our numerus versuum? First, we should not be misled by the term "edition" conventionally used of Aristarchus's διάρθωσις or ἐκδοσις into supposing that in about 144 B.C. the Museum suddenly flooded the market with hundreds of verbatim copies of this text — as West (16) has said,
"there was no Alexandrian University Press". There may well have been merely the original, which Aristarchus kept for his own use; a single apograph for his pupils and other authorized persons to consult, presumably deposited in the Library of the Museum; and perhaps a single copy of this apograph, made by the Library primarily as an insurance against loss or damage and not readily available for consultation. Further, it is doubtless true that "the majority of readers of the Homeric poems ... were much less interested in the minutiae of textual criticism, in the arguments for preferring one reading to another" than in having a text purged of lines judged by the experts to be unmistakably spurious; and it may well have been generally believed - essentially rightly, and probably even on the admission of Aristarchus himself - that whereas only obvious interpolations were actually omitted from his text, his intralinear readings were often a more uncertain and speculative matter: Aristarchean caution could ensure that only lines omitted by the overwhelming majority of his sources were absent from his edition, but it would, in the nature of the case, have been impossible to apply a similar principle consistently to the intralinear variants: where his sources were more or less evenly divided between two readings, he would, by choosing to adopt one of them, willy-nilly have been excluding a variant with very substantial support. Finally, it would have been more economical for a publisher to retain and correct his existing exemplar of Homer than to produce a completely new one, and much easier merely to correct its numerus versuum than to collate it with the edition of Aristarchus word for word.

To summarize the findings of this Chapter: - The conclusion that our vulgate, when purged of its interpolations, is identical in its numerus versuum with the edition of Aristarchus is inescapable. Both the publication of papyri since the time when Bolling first came to this conclusion and a critical examination of the weak points in his argument have served only to strengthen this conclusion. The lack of correspondence between our vulgate and the edition of Aristarchus in their intralinear readings, while perhaps at first sight surprising, is capable of satisfactory explanation.
NOTES ON CHAPTER I

1. On Aristarchus's edition(s) see Pfeiffer 212-17. Pfeiffer rejects, and I would say refutes, Erbse's improbable view that Aristarchus did not produce a complete Homeric text of his own at all.

2. EE 3-15 and the articles listed at EE 7; ALI 5-30.

3. See West 27, 40, 66, 68f. and below, p. 81.


5. Cf. West 27 (speaking of Aristarchus's sources): "If enough MSS. were collated, a fairly constant common basis would be obvious; lines which only appeared in a handful of MSS. could safely be disregarded."


8. See below, Chapter IV n. 20. A quasi-exception is the omission of ll. 18. 10 by the lone MS. P3 (saec. xiii), obviously accidentally; Aristophanes had omitted lines 10f.

9. AJP 70 (1949) 372.

10. Above, p. xviii. There is one striking exception which Bolling himself elsewhere (EE 178) acknowledges as such - the deliberate omission of the Catalogue of Achaeans and Trojans in ll. 2 by some scribes in order to save labour and material: they evidently regarded it as a boring list whose omission would be acceptable to the reader. The introduction to the Catalogue, 484-93, is found in all MSS. and papyri, but the Catalogue itself, 494-877, is omitted by 21 MSS. and one papyrus according to Allen's apparatus; for its probable omission by a second papyrus see Barbara McCarthy, CP 27 (1932) 154f.


13. In 1916 Bolling rightly said of this last scholium, "Aristonicus ... [implies] that 808 was not Aristarchean": his attempt in 1925 (EE 89f.) to deny the necessity of this conclusion is special pleading, not only because of the evidence of the other scholia quoted in my text but also because of the form of this
scholium itself: its wording clearly implies a διπλή περισσυγόγινη, signifying disagreement with Zenodotus (Aristonicus goes on to give an argument against the line on the basis of the internal evidence), and this sign is reproduced in the margin of A; the very fact that Aristarchus used this sign here in conjunction with the criticism Ζηνόδοτος ὑποτάσσει τοῦ πόσεν (sic: not τῶν στόχων) naturally implies that the line was completely absent from his own edition, but we can go further than this: this sign is attached by its lemma, its wording (ὑποτάσσει τοῦ πόσεν) and its position in A to 807, not 808: if Aristarchus had merely obelized 808, the διπλή περισσυγόγινη would surely have been attached to that line (there would be no objection to applying two signs to the same line: cf. the combination of obelus and διπλή in the margin of A and implied by the scholia at Il. 15. 64, 69 and 71, and the frequent combination of obelus and asterisk); nor is an athetesis of 808 recorded; so that the conclusion that this scholium implies the total absence of 808 from the edition of Aristarchus is virtually inescapable.

14. AJP 37 (1916) 26: "... L9 V16 omit E 808, which may best be ascribed to the slight but double temptation (..... ἐνύκα, ...... ἡ α and ρηδό ......., σοὶ δ' .......) to haplography." Now the former "temptation" could conceivably have led to the omission of 808, though the homoioteleuton is very slight; but the latter "temptation" is even slighter, and would in any case have been more likely to lead to the omission of 809 rather than 808. Allen's judgment ad loc., "v. om. L9 V16 ex homoeomesos", presumably refers to 808 τοῦ οὗ ἔγαν, 809 ήτοι μὲν ἔγα: this homoeomeson is quite pronounced, and could have led to the omission of 808, but, again, it would have been more likely to lead to the omission of 809. The matter is one to which we shall presently return.

15. AJP 37 (1916) 25.


17. See above, n. 14.

18. See below, p. 131.

19. I am thinking both of his comments on Aristonicus's note on Il. 5. 807 (see above, n. 13) and of his interpretation of θαυτόν in the Didymean Schol. A.

20. Unfortunately Van der Valk's discussion of this line (RTSI II 492f.) completely ignores the papyrus and manuscript evidence. Against his hypothesis of simple Aristarchean forgetfulness see Erbse, Gnomon 37 (1965) 536f.


27. Above, p. X.


29. For this argument cf. Ludwich and Van Leeuwen - Mendes da Costa (above, n. 28).

30. Erbse's comment on Schol. A on Il. 17. 221, "Aristonicus (h.e. Aristarchus) versum P 220 aut ignorasse aut neglexisse videatur", is special pleading. There is no reason to doubt that the line was read by Aristarchus, as it is unanimously attested by the post-Aristarchean tradition, including three papyri (the same three named in connection with Il. 17. 219 below, p. 149), and is commented on in the exegetic scholia. I am not sure what Erbse means by "neglexisse": if he means merely "ignored" in a sense compatible with the interpretation given in my text, I have no quarrel with him, but if he means "overlooked", the answer is that Aristarchus was not that careless.


32. E. Schwartz (Die Odyssee [Munich 1924] 312) thought the scholium should be referred instead to line 141, but this question need not concern us here.

33. There are two notes on this line in Schol. H, both presumably going back to Didymus: οὔτε Ἀρίστοριχος οὔτε Ἀριστοφάνης οὔτε Ζηνόδωτος ἐκταταν τοῦ τῶν στύχων καὶ οὔτος ὁ στύχος εἰν τοῖς Ἀριστοριχέως οὐ φέρεται. This wording suggests that Didymus, finding a conflict in the testimony of the ordinary MSS. current in his own day, consulted the editions of the three great Alexandrians and then wrote his report.

34. See the table in the Prolegomena to Allen's Iliad, pp. 199f.


37. West 16f., summarizing Collart's view; cf. Bolling, ALL 22f.

38. Against this it could be objected that an old exemplar with an expanded text could always have been sold, but such exemplars would probably not have been in a fit physical state to palm off on the public, and might in any case have been difficult to get
rid of in a market which was demanding "Aristarchean" texts.
Further, demand for complete texts of the Iliad or Odyssey may well have been relatively slight.

...some evidence in the post-Aristarchean transmission of a line are due not to it being an interpolation but to some transcriptional error; some can be due to post-Aristarchean emendation. In both these categories may be termed "non-significant", as opposed to the "significant" omissions which constitute evidence of interpolation. How can significant omissions be distinguished from non-significant ones? Normally an insignificant omission will be confined to a few MSS, whereas a significant omission will be more widespread, but diagnosis must take other factors into account like the relative dates of the emending and containing MSS, and/or papyri, whether there is pre-Aristarchean evidence for the line, whether the line is dispensable, whether there is a homographic temptation to emend, and, if not, how long the passage is, whether the line is a non-exact (repeated line) or virtual (secondary), whether one can perceive a motive for or reflection to interpolation (e.g. a context inviting emendation, interpolation), whether one can perceive a motive for post-Aristarchean emission. Obviously, if the line absolutely essential to the sense be omitted by a single sixteenth-century MSS and one can perceive a strong homographie temptation to emend, one will have no hesitation in judging that the omission is purely horizontal. Conversely, if the responsible repeated line is absent from all MSS, one papyrus earlier. When (say) the 16th-century and there is a strong conjectural text and concordance interpolation, one will have no hesitation in thinking that the line is an interpolation in the Aristarchean version. These two examples, of course, represent opposite vertices, but in most cases there will be no difficulty in deciding on which side of the issue an emendation falls. In some cases, however, the evidence will be too close, a careful examination will be necessary, and sometimes a conclusion with only a fair degree of probability may be the best one can achieve. Finally, in a few cases the evidence will be so scanty that we shall be unable to say whether a line is Aris-terarchean or non-Aristarchean. In such cases we shall have to suspend judgment until more papyri are published.

I shall now (1) consider some of the priorities for diagnosing Aristarchean transmission in greater detail and then illustrate the
CHAPTER II

NON-SIGNIFICANT OMISSIONS IN OUR MSS.

Some weaknesses in the post-Aristarchean attestation of a line are due not to its being an interpolation but to mere transcriptional error; some may be due to post-Aristarchean excision. Omissions in both these categories may be termed "non-significant", as opposed to the "significant" omissions which constitute evidence of interpolation. How can significant omissions be distinguished from non-significant ones? Normally an insignificant omission will be confined to a few MSS. whereas a significant omission will be more widespread, but diagnosis must take other factors into account too: the relative dates of the omitting and containing MSS. and/or papyri, whether there is pre-Aristarchean evidence for the line, whether the line is dispensable, whether there is a homoiographic temptation to omission and, if not, how long the passage is, whether the line is a διψωρούμενος (repeated line) or virtual διψωρούμενος, whether one can perceive a motive for or temptation to interpolation (e.g. a context inviting concordance interpolation), whether one can perceive a motive for post-Aristarchean excision. Obviously if a line absolutely essential to the sense is omitted by a single sixteenth-century MS. and one can perceive a strong homoiographic temptation to omission, one will have no hesitation in judging that the omission is purely accidental. Conversely, if a dispensable repeated line is absent from all MSS. and papyri earlier than (say) the 14th century and there is a strong temptation to concordance interpolation, one will have no hesitation in judging that the line is an interpolation in the Aristarchean vulgate. These two examples, of course, represent opposite extremes, but in most cases there will be no difficulty in deciding on which side of the fence an omission falls. In some cases, however, the evidence will be less clear: a careful assessment will be necessary, and even then a conclusion with only a fair degree of probability may be the best we can achieve. Finally, in a few cases the evidence will be so evenly balanced that we shall be unable to say whether a line is more likely to be Aristarchean or non-Aristarchean: in such cases we shall have to suspend judgment until more papyri are published.

I shall now (1) consider some of the principles for diagnosing accidental omissions in greater detail and then illustrate the
principles in general by applying them to a few selected passages; and
(2) assess the evidence for post-Aristarchean excision.

(1) Accidental omissions

I have shown elsewhere\(^1\) that purely accidental omission can extend
to a small group of MSS. beyond a single Allen family. An extreme case
is the omission of Od. 17. 432 (which is essential to the sense) by
three of Allen's families and two other MSS.:\(^2\) an investigation based
on the solid criterion of indispensability to the sense will reveal
that this is, roughly, the upper limit of the support which one may
normally expect for an accidental omission (though some exceptions in
a special category will be considered in Chapter V), even allowing for
the probability that an exceptionally strong homoiographic temptation
has occasionally led two different scribes to omit the same line or
passage completely independently of one another.

One point which needs to be made is that accidental omission of a
single line without any homoiographic temptation is common: numerous
examples may be gleaned from Bolling's 1916 lists of "surface
corruption".\(^3\) Accidental omission of a passage of two lines or more
without homoiographic temptation is rare but not unknown.

I have elsewhere given certain and probable examples of accidental
omission;\(^4\) I shall now discuss three lines which Bolling, on the
strength of the external evidence, regards as interpolations in the
vulgate but which I shall argue may well be (in one case almost
certainly is) Aristarchean and genuine. In each case Bolling has
failed to give adequate weight to the possibility that the limited
external evidence against the line is merely the result of a copyist's
slip. The three lines are II. 4. 369, 3. 319 and 13. 46. My selection
is not merely exempli gratia, as all three lines are introductory
speech-formulae of a type which certainly has been interpolated
elsewhere, i.e. a line containing a simple verb of speaking after
another verb of speaking - whether simple or with particular overtones
(e.g. ὡτεινε, ἤλλατάνεσα) - has already preceded, and this is a type
of interpolation to which we shall be returning in Chapters V and VI.\(^5\)
Bolling rejected the three lines in various treatments between 1914 and
1950.\(^6\) In two cases the relevant papyrus evidence has expanded since
1950.

(a) II. 4. 369 is omitted by Pap. 32 (Pack 722, saec. iii p.C.),
and is absent from the text of A (but added in the margin). However, it is present in the remainder of our minuscules and in Pap. 4 (Pack 697, saec. i p.C.) and Pap. 396, published in 1951 (Pack 1176, saec. ii-iii p.C.). In my opinion the evidence for and against the line is fairly evenly balanced. On the one hand the omission could well be accidental, the product of the homoiarchon καὶ in 368 and 369: Allen ad loc. states flatly "om. ... ex homoearch." On the other hand, the line could well be an early post-Aristarchean interpolation: it is far from essential to its context; if it was absent from the edition of Aristarchus there would have been a strong temptation to concordance interpolation - II. 4. 368 closely resembles 336, 369 = 337 (except that μίν is substituted for σφέας); we know that other speech-formulae of this type were interpolated early in the post-Aristarchean era; and it is noteworthy that the only minuscule which omits the line is the earliest we possess (Allen's D is not extant at this point). However, I would regard the evidence in favour of the line as strong enough, and early enough, to slightly outweigh the evidence against it.

(b) II. 3. 319 is omitted by Pap. 49 (Pack 695, saec. iv-v p.C.) but is read by all our minuscules and by Pap. 4 (Pack 697, saec. i p.C.) and Pap. 3 (Pack 634, saec. iii p.C.). In 1956 Mette himself published Pap. 392a (Pack 696, saec. iii-iv p.C.), covering II. 3. 280-398: his collation reads simply "Lücken; 393 μαχεσαμενον": I assume, therefore, that the papyrus either reads 319 or is not extant at this point. The line is not essential to the sense - cf. e.g. II. 16. 231-3 - and may be an interpolation; but in my opinion it is rather more likely to be genuine and its omission accidental. The external evidence against the line is not strong: the two or three papyri which contain it are each earlier than the single papyrus which omits it, and our earliest witness in favour of the line is over three centuries earlier than the only witness against it. Admittedly we can hardly speak of a homoiographic temptation to omission, since the homoioteleuton 319 τε, 320 -τε would not normally be likely to lead to the omission of 319; but, as we have seen, there are numerous cases of the omission of single lines without any homoiographic temptation at all. Moreover, I believe the internal evidence favours the retention of the line: there is a certain inherent parallelism between the two prayers of both armies at 296-301 and 318-23 which favours the repetition of 297 at 319: 296 ... εὐχοντο θεοῖς ..., cf. 318 ...
When the theme of the duel recurs in Book 7, there is a similar parallelism between the two prayers of the Achaeans at 177-80 and 200-205, again with the verbatim repetition of a similar introductory line, viz. 178-201 ὁ δὲ ὁ τοῖς εὐκερχεῖν Ἀχαῖῳ τοῖς Τριάδων τε = 319; 298 Ζεῦ ..., cf. 320 Ζεῦ ...; 299 ὁπόπτερον ..., 321 ὁπόπτερος. When the theme of the duel recurs in Book 7, there is a similar parallelism between the two prayers of the Achaeans at 177-80 and 200-205, again with the verbatim repetition of a similar introductory line, viz. 178-201 ὁ δὲ ὁ τοῖς εὐκερχεῖν Ἰών ἐν ὅπλαν ἐν ὁμήρων. Moreover, the parallelism between 3. 316-24 and 7. 175-81 also favours the presence of 3. 319: 3. 316 κλήρους ἐν κυνεῖ, 7. 175f. κληρον ... ἐν ... κυνεῖ; 3. 318 = 7. 177, only here in Homer; 3. 319, cf. 7. 178; Ζεῦ πάτερ at the beginning of 3. 320 and 7. 179; ὁ θεός ἅγιος ἔφαν, πᾶλαι ὁ 3. 324 and 7. 181, as a sequence only here in Homer. However, the limitations of these structural considerations must be recognized, and I am not claiming that they prove the authenticity of the line.

(c) In the case of II. 13. 46, Ἀρχαντε πρότω προσέφη, μεμαθώτε καὶ αὐτῷ (= II. 16. 555), Bolling has badly misjudged the evidence: the omission of the line by Pap. 10 (Pack 899, saec. i p.C.) and the first hand of Ludwich's Fz (= Allen's L5, 1465 A.D.) is almost certainly accidental, the result of a very strong homoiarchon - Ἀρχαντε begins both 46 and 47; the line is virtually indispensable to its context, since after the very general Ἀργείων ὣρουν of 44 we should expect a more specific introduction to a speech (47-58) addressed specifically and exclusively to the Aiantes. The line is present in two other papyri, Pap. 85 (Pack 900, saec. i a.C. - i p.C.) and Pap. 60 (Pack 870, saec. iii-iv p.C.), as Bolling himself records. Apoppos of both this line and II. 4. 369 (see (a) above) Bolling says that "we [must] reckon ... with the probability that the isolated variants in the [minuscule] MSS. are merely accidental" and acknowledges the "temptation[s] to haplography", so that his failure to allow at least a strong possibility that the two isolated papyrus omissions are also accidental is anomalous. He does concede, speaking of the hypothesis that all the papyrus omissions in his list of seven introductory speech-formulae are accidental, that "that this may be true of one or two examples cannot be denied", but this must be compared with his bald statement "My explanation is that these [seven lines] are interpolations", and with the fact that in EE he lists both (a) and (c) as post-Aristarchean interpolations without even adding the question-mark which he applies to other examples which he regards as doubtful.
(2) The possibility of post-Aristarchean excision

What evidence, if any, for post-Aristarchean excision is there (a) in extant post-Aristarchean criticism and (b) in the post-Aristarchean MSS. and papyri themselves? We shall consider each part of this question in turn, though the two parts overlap to some extent. However, before proceeding we must stress that such excisions would not have left traces in more than a very small proportion of the extant MSS. and papyri, especially since the excised lines would tend to return to the text through contaminatio. Such disturbances in the tradition would be so slight that they would tend to be indistinguishable from cases of purely accidental omission: the latter kind of omission is so widespread that it is natural that there should be a number of lines so omitted which happen to be both dispensable and of such a type that they might have seemed suspect to someone bred in the tradition of Alexandrian criticism. Thus the importance of the whole question of post-Aristarchean excision is relatively minor: even if we allow that it sometimes occurred and has in places affected our MSS., Bolling's position on the post-Aristarchean interpolations will scarcely be affected, since whether the omission of a line is due to accidental omission or to post-Aristarchean excision the external evidence against it will in either event be equally valueless. Of course there are cases where, even though only one or a very few extant texts omit a dispensable line, there is at least some significant chance (but something less than an overwhelming probability) that the omission is evidence of interpolation, this chance being stronger where the omitting text(s) are early, where the omitted line is a ὅψεωςων and where there is no homoiographic temptation to omission; and if in such cases we allow post-Aristarchean excision as a further possibility (i.e. in addition to the possibility of accidental omission) compatible with the line's authenticity, the chances of its being an interpolation will thereby be slightly reduced. That is the limit of the importance of the question of post-Aristarchean excision for the establishment of the Homeric numerus versuum.

(a) Post-Aristarchean criticism

Van der Valk (TCO 284f.) argues that actual excision (as distinct from mere athetesis) "was a common practice with ancient critics, as appears e.g. from the anti-Aristarchian critic, whose statements have
been preserved in Athen. 5 Cap. 1-20. This anti-Aristarchian ... removes from the text B 409 ... 5 15-9 ... and a 141, 2." However, the definiteness of Van der Valk's "removes from the text" goes further than the evidence warrants. Athenaeus (177C-178A) does express the opinion that II. 2. 409 is an interpolation (177C 11 νες ... προσέγραφαν: see further Bolling, ALI 75), but this need not imply that he or his source actually deleted the line from any copy of the Iliad. On the other hand, it is a curious coincidence that the line is omitted by a single MS., Allen's P10 (saec. xii-xiii), and it is just possible that this is the result of deletion by Athenaeus or his source, or else by someone else who read the discussion in Athenaeus or his source. However, I regard it as at least as likely that the coincidence is the product of pure chance and that the omission by P10 is merely a transcriptional error, even though there is no homoiographic temptation worth speaking of: as we have seen, there are numerous instances of accidental omission of a single line without any such temptation. Alternatively, we could suppose that the suspicion of the line reflected in Athenaeus was originally triggered off by a post-Aristarchean accidental omission and that P10 is a descendant of the MS. containing it. Likewise, Athenaeus's claim that οι τερι Αρίσταρχον ... προσενηψαν Od. 4. 15-19 (or 17-19) need not imply that the lines were actually deleted from any post-Aristarchean text. (They are present in all our MSS.) With the third passage mentioned by Van der Valk, Od. 1. 141f., the situation is rather different. At 193B Athenaeus says: διαμαρτάνονοι δε πολλοι παρα τη ποιητη έφεξις τιθέντες τούτους τούς στόχους· he then quotes three lines which could be either Od. 1. 139-41 or 4. 55-7, and concludes, after a misguided argument that the third line does not suit its context, διατερ το δύστιχον άκαρκει, i.e. "Hence the first two lines alone are sufficient". It is not entirely clear whether, as Van der Valk assumes, Athenaeus (or his source) means to condemn the fourth line as well, which he does not mention (it is the same in both Book 1 and Book 4): the fourth line completes the sense of the third, but if the third alone were omitted the transition from the second to the fourth would give satisfactory (though altered) sense. However, Van der Valk is probably right: the formulation το δύστιχον άκαρκει militates against the hypothesis that in this version the fourth line was envisaged as following the second and completing its sense, and the reason why Athenaeus quotes the third line without the fourth is
probably that his argument from the internal evidence concerns only the third. Now Athenaeus's language here, especially ἐξεχεύσεις τε ἄλλης ..., does seem to imply a recommendation that the third line (probably, as we have seen, together with the fourth) be actually deleted from those texts which contain it. However, even if this implication is present, Athenaeus's source (for it does seem likely that Athenaeus is here following an earlier writer) may essentially have been simply passing judgment on the relative merits of divergent texts current in his own day: Van der Valk, in the passage from TCO which we have quoted, designates the lines "remove[d] from the text" by Athenaeus's source as Od. 1. 141f., but elsewhere in TCO he assumes that this critic's treatment is meant to cover the passages in both Od. 1 and Od. 4;¹⁹ but I think it most likely that this critic intended to deal only with the passage in Od 4: 4. 57f. are omitted by a very substantial minority of our MSS., including our second oldest minuscule, L8, and must be an interpolation; and as this interpolation has spread to the majority of our MSS. it may well have been made fairly soon after the time of Aristarchus. We may surmise, then, that Athenaeus's source discovered that the lines were present in only some of the MSS. to which he had access, suspected them because of this weakness in their attestation and then misguided tried to clinch the matter by a false argument from the internal evidence; and we can then assume that he was unaware that the lines had already occurred, universally attested, in Book 1 in what is, as far as this argument is concerned, an identical context. Of course it is quite possible that he actually deleted Od. 4. 57f. from some MS. or MSS. which contained them; but this would be something quite different from "excising a genuine line on the basis of the internal evidence alone".²⁰ Alternatively, he may have been aware that the lines were to be found in both Book 1 and Book 4 and may have felt that the external evidence against them in 4 also told against their authenticity in 1.²¹ Finally, the thesis that Od. 4. 57f. are genuine and that their weak attestation is due entirely to post-Aristarchean excision is ruled out by the strength of the witness against them.

We may next consider Plutarch's "deletions" from the text of Hesiod.²² Plutarch wrote a commentary on the Works and Days which is no longer extant. Proclus in his own commentary on the Works and Days - also no longer extant in its original form - often referred to
the views expressed in Plutarch's commentary; and Proclus was used extensively by the compiler of the "old" Hesiodic scholia and occasionally by Tzetzes. According to the scholia Plutarch "expelled" or "crossed out" certain lines: 353f. ἐκβάλλει,23 561-3 and 757-9 διαγράφει. On the face of it, one might think, these verbs would be better rendered by delet than by dammat; but our confidence in this distinction is shaken when we find that where, on 353f, the scholiast says τούτων ὁ Πλούταρχος ἐκβάλλει τοὺς στύχους, Tzetzes says ὁ μὲν Πλούταρχος τούτως ὀβελύζει τοὺς στύχους; and it is hardly likely that the distinction would have been particularly important to Plutarch, who presumably published a commentary with lemmata but not an actual edition of the text:24 thus in each of these three cases he probably did no more than express a judgment that the passage in question was spurious, though I suppose it is conceivable that he did actually delete the lines from his own copy of the text (except in the case of 353f. if we accept Tzetzes's ὀβελύζει as an accurate description). Further, we may note that none of the three passages is omitted by any of the extant MSS. or papyri. Finally, support for the interpretation that the scholiast's ἐκβάλλει is indeed equivalent to ὀβελύζει or dammat is provided by the convincing demonstration by Ludwich, basing himself on a view expressed by Lehrs, that in Schol. A on Π. 16. 97-100 (athetized by Aristarchus) says παντελῶς ἐκβάλησέν τοὺς δ' στύχους, a demand for total deletion is really being made. Van der Valk thinks so - "It is clear that this critic wished to remove the lines from the text" (RTSI II 487) - but, in the light of the parallels just discussed, I think it probable that the meaning is merely "One must completely reject the lines", i.e. "One must emphatically deny their authenticity", without implying that the passage should be literally deleted from the text:26 the strength of the phraseology is sufficiently explained by the extraordinary vehemence of the attack on the passage which immediately follows. After all, if the critic "wished to remove the lines from the text", why did he not do so? Or did he? The picture of this scholar copying out at least the first
half of the first line of this passage for his lemma (this much is preserved in T), and perhaps the whole passage, then composing his carefully-planned and highly rhetorical condemnation of it (it contains a string of four scathing rhetorical questions) and finally solemnly deleting each of the four lines in his MS. of the text is faintly absurd. At any rate, his judgment has had no effect on any of the extant MSS. 27

To conclude this section:— Perhaps the various exercises in ancient criticism which we have discussed go some way towards establishing the existence of a climate in which occasional post-Aristarchean excision from the text of Homer would have been possible, but we cannot say more than this. The evidence is thus inconclusive.

(b) Post-Aristarchean MSS. and papyri

We may first ask whether our post-Aristarchean MSS. and papyri exhibit any evidence that athetesis by Aristarchus ever led a post-Aristarchean critic to delete, or a scribe to deliberately omit, the athetized passage. On p. 197 of the Prolegomena to his edition Allen says,"[Aristarchus' signs ... did not have the effect of removing lines from the text ... and were not intended to do so. Hence we do not expect to find, and do not find, Aristarchus' athetesis followed by omission in the text. There are, however, a few coincidences between omissions in our MSS. and his athetesis (or that of others), but they are so few as to be negligible."

These remarks are entirely justified. Allen then proceeds to list 15 passages from the Iliad which he claims constitute such coincidences, 28 but at least six of them should in fact be removed from the list. (1) His inclusion of Il. 21. 73, with the comment "ath. Ar.", is presumably a simple blunder, since he is elsewhere aware that this line was completely absent from Aristarchus's text. 29 (2) I shall argue in Chapter V 30 that Il. 2. 558 was not athetized but omitted by Aristarchus. (3) Schol. T reports that Zenodotus of Mallos read the additional line Il. 13. 731: it was absent from Aristarchus's text, as both this scholium and the manuscript tradition indicate, and we have no record of its having been athetized by anyone. 31 (4) Similarly, Il. 8. 466-8 must also have been absent from Aristarchus's edition, since they do not appear in the text of any MS. certainly earlier than the 13th century, 32 and the note on them in Allen's very late Bm4 (saec. xv), τρεῖς στόχου παρ' ἐν' ὀλῃ ἀπετοῦντα, must be the product of a late Byzantine collation.
and must mean, "These three lines are missing in some MSS." - a late and ignorant misuse of the verb ἀδειεύν which is not without parallels (see Bolling, EE 50f.). (5) Allen's entry "α 556 ath. Ar. om. Vl V23" is misleading because the correspondence is not exact: Aristarchus athetized both 556 and 557, whereas it is only 556 which is omitted by some MSS. - obviously as a result of the homoiographon 555 ... οὔ δὲ ... ἀπονα, 556 ... οὔ δὲ ... ἀπὸνα ... , as Allen himself remarks in his apparatus ad loc. ("om. ... ex homoeomemo").

(6) Aristarchus athetized Il. 9. 694; a single MS., Allen's V16 (saec. xii), omits 694f., obviously through a mere transcriptional error. This time Allen does acknowledge the inexactness of the correspondence, but in reality this inexactness is sufficient reason for omitting the passage from his list: if one were to include all such partial correspondences his list would need to be expanded considerably: cf. the list of "approximate correspondence[s]" in Bolling, EE 4 n. 3.

What of Allen's remaining examples? (7) Il. 2. 409 has already been discussed. (8) Il. 23. 92 was athetized by Aristarchus, but the only extant text which omits the line is pre-Aristarchean (Pap. 12 [Pack 979, saec. iii a.C.]), so that this example is irrelevant to our consideration of post-Aristarchean excision. (9) Il. 2. 141: Allen's entry "X—P37" is erroneous, but there are other reasons for believing that the line was athetized; its omission by V32 (misprinted as "V22" in Allen's list) is best explained as being due to the strong homoioteleuton in 140 and 141. (10) Il. 21. 195: strictly speaking this line should not be in Allen's list, because our sources speak of pre-Aristarchean omission, not athetesis. In any event, the omission by 05 and the possible omission by Pausanias are not particularly likely to be due to post-Aristarchean excision. The omission by 05 (saec. xii) is probably due to the homoiarchon 194 τῷ οὔδε, 195 οὔδε. Pausanias at 8. 38. 10, referring to the River Achelous, says: ἐξήσυν ἐν 'Ἰλαδίῳ Ὀμήρος ποταμῶν τῶν πάντων ἄρχοντα εἶναι. Now even if Pausanias read 195 such a statement would be perfectly possible, for after speaking about rivers in general in 184-93 Achilles refers to κρεσῶν 'Ἀχελῶνος in 194; and indeed 196f., ἐξ οὗ περὶ πάντων ποταμῶν ... νάουσαν, which refer to Okeanos if 195 intervenes and to Achelous if it does not, deal with genesis rather than power. Nevertheless, the correspondence between Pausanias's
...ν ἐστιν and Homer’s πάντες ποταμοὶ arguably makes it rather
more likely than not that Pausanias’s comment was based on a text
which omitted 195. If so, a post-Aristarchean transcriptional error
will again be a possible cause, though I think it more likely that
Pausanias based his comment on some earlier source which followed the
(pre-Aristarchean) shorter version of the text (the evidence for which
is assembled by Bolling at EE 188f.). (11), (12) Next, there are two
lines (each omitted by a single MS.) whose omission is most naturally
explained as an accident caused by a homoiographon: 11. 8. 284 om. VI6
(saec. xv) - 283 ... ἔδνια, 284 καὶ ... ἔδνια ..., 285 ... καὶ ... ἔδνια;
11. 16. 261 om. VI6 (saec. xii) - homoioteleuton 260 ἔδοντες, 261
ἔχοντας. (13) The omission of 11. 9. 44 from the text of T (1059 A.D.)
is in all probability another transcriptional error, possibly caused
by the slight homoiomeson 43 ... τοῦ ... τοῦ ..., 44 ... τοῦ ..., the
first hand of T adds the line in the margin. (14) Similarly, the
omission of 11. 23. 810 by a solitary MS., Allen’s VI4 (saec. xiii),
is most naturally explained as accidental, even though there is no
homoiographic temptation this time beyond the ending of 809 and 810 in
ν. (15) This leaves only the curious case of 11. 7. 353. Aristarchus
athetized the line; it presents serious difficulties, and may well be
an interpolation "added in order to supply a verb to the phrase οὐ νῦ
τι κέρδος ἦμιν [at the end of 352], which does not need one" (Leaf ad
loc.). Now the quotation of this passage by Dio Chrysostom stops
short at line 352, which he cites in the form ... οὐ νῦ τι κάλλιον
ἔστι. After this, the beginning of line 353, ἔλαμβαν ἑκτελέσθαι, would no longer be able to fulfil the function of completing the sense
of 352 which it performs in the direct tradition, and with a stop after
352 line 353 would not give satisfactory sense, even if we read ἔνα
for ἔνα. Understandably, then, Allen states in his list that Dio
omits 353 "ut videtur". What are we to deduce from this? That some
post-Aristarchean critic excised 353 and supplied the copula in 352?
Then why should he have arbitrarily changed κέρδον, read by all our
MSS., to κάλλιον? More plausible is the thesis that Dio’s version is
due merely to a lapse of memory: κάλλιον for κέρδον would be an easy
slip to make, and, after all, the sense of ἔλαμβαν ἑκτελέσθαι hardly
differs from that of Dio’s simple ἔστι. Alternatively, it is just
possible that Dio was "quoting at second hand" (Bolling) and that
his source, himself quoting from memory, knew a text of the pre-
Aristarchean type which omitted 353 because the interpolation had not
spread to it. It is even possible that Dio's text, or that of his
source, read χάλλων instead of κέρδον in 352, as a traditional
variant, even though it has not been transmitted in any of the extant
MSS.: for other instances of the interchange of κέρδον and χάλλων
see Allen's apparatus on II. 15. 197, 226, 22. 103, 108. But if we
allow that χάλλων may have stood as a traditional variant in some
post-Aristarchean texts, then we cannot completely exclude the
possibility that someone in the post-Aristarchean era who had such a
text actually deleted 353 and in 352 merely substituted έστι for
ήμυν.

Finally, one should add that Allen's list needs to be supplemented
by three examples mentioned by Bolling at EE 4. II. 8. 557f. were
omitted by Zenodotus and athetized by Aristophanes and Aristarchus;
they are omitted by Ludwig's Ηβ (= Allen's VII, saec. xvi), almost
certainly as a result of the pronounced homoioteleuton 556 νήνευος
αὐτήρ, 558 ἀπετευχ αὐτήρ.45 II. 2. 143 and 4. 117 are each omitted by
one of Ludwig's MSS.; the former line has been added in the margin
by the first hand. Although there are no homoiographic temptations
this time (except that 4. 116 and 117 both end in ν), the most natural
explanation of these two omissions would seem to be that they too are
accidental.

In spite of these three additions, however, Allen's assertion
that coincidences between athetes by ancient critics and omissions
in our MSS. are "so few as to be negligible" has, on balance,
turned out (at any rate as far as the Ηλιαόδ is concerned) to be even
more justified than his list of examples at first sight suggests.46

Much the same could be said of the corresponding list for the
Odyssey drawn up by Allen in 1910.47 After remarking that
"coincidences with ancient athetes are few", he lists five passages,
which we shall now proceed to consider. (1) Οδ. 11. 604: this line
was not athetized by Aristarchus or by any ancient critic: it is a
post-Aristarchean interpolation, as is indicated both by the scholia
(when carefully read) and by its absence from the text of Pap. 11
(Pack 1102, saec. i-ii p.C.) and a weighty minority of Allen's MSS. -
his family c (fathered by Η3, saec. xiii), Pal. (1201 A.D.) and L7
(saec. xiii). The line = Hesiod Theogony 952. The whole matter has
been excellently treated by Bolling at EE 212. (2) One of Allen's
entries is "δ 58": this should read "δ 57f." or, conceivably, "δ 57"
(in the latter event the correspondence would be only partial). We have already discussed this passage. 48 (3) Od. 4. 783 (= 8. 54) is omitted by numerous MSS. - Allen's three families a, k (including our oldest Odyssean minuscule, L4 [saec. x-xi]) and q and his MSS. H3 (saec. xiii) and Pal. (1201 A.D.), and also, according to Ludwich, by his (= Allen's) J (now lost - date unknown) and, "ante correcturam", by his E (= Allen's M3, saec. xiii-xiv). Thus it was obviously absent from Aristarchus's text; it is an instance of concordance interpolation (4. 78lf. = 8. 52f.). Hence the M scholium on the line, περιττὸς δοκεῖ oδιος ὅ στιχος, should not be regarded as evidence of an ancient athetesis; in all probability it is a late Byzantine comment occasioned by a collation which revealed that the line was absent from part of the tradition; the cautious δοκεῖ supports this interpretation. However, we cannot completely exclude the possibility that Aristarchus found the line in a few of his MSS. and recorded the fact in his ὑπομνήματα, that Didymus had a note quoting the line and adding an adverse judgment - e.g. ἐν ἔννοια μετὰ τοῦτον [4. 782] φέρεται στίχος περιττὸς. "πάντα ...", 50 that this note was preserved in a scholium, now lost, from which the line entered the text, and that the scholium which we actually have is a relic of the judgment contained in Didymus's note; but of course this hypothesis would still not involve an ancient athetesis of the line. (4) Allen includes Od. 13. 391 in his list. However, this is only a partial coincidence. 391 is omitted by Allen's U5 (saec. xiii). There is a scholium attached to 390 (Allen's edition ad loc. is misleading) which reads: ὑπονοεῖται ὅ στιχος, διὰ τὸ μὴ ἐξελν ὑπερβολὴν. ἐν τῇ λ γοῦν τῆς Ἰλιάδος εἴεισι τριακοσίων ἀντέστι καὶ μὴ παροδῆς Ἀθηνᾶς. 51 Unless the suspicion mentioned in the scholium were merely of the transmitted version of 390 (for an emendation substantially increasing the number might have been proposed), it would have to extend to the whole passage 389-91. The omission of 390 alone would produce nonsense; and the omission of 391 alone would not remove the reason for the objection, since 389 says much the same as 391. Thus it is highly unlikely that the omission of 391 by U5 is the result of an excision under the influence of the M scholium; in all probability it is a mere transcriptional error. The only homoiographon is the homoiomeson 391 πάντα θεά, 392 ἔπειτα θεά: This would normally be much more likely to lead to the omission of 392 than of 391, but one may compare the omission of Lucan 7. 90 by two MSS. through the influence, apparently, of the homoiomeson
90 magnae, 91 magnum. (5) There is an H scholium on Od. 3. 209 which reads: τερεύτως ἄρης γὰρ ὁ πρὸ αὐτοῦ. The line may well have been athetized by Aristarchus. It is omitted by Allen's R11 (saec. xv), probably by accident, and perhaps through the operation of the slight homoiomeson 208 μοι, 209 έμοί. However, one must admit that the chances that the omission is due to deliberate excision provoked by the scholium are enhanced by the fact that the MS. which omits the line is a member of the family founded by Dindorf's H (= Allen's H3), the MS. which contains the scholium.

If any lines at all were excised in post-Aristarchean times, I think it is reasonable to suppose that the athetized lines would be among those most likely to suffer in this way. Thus the fact that there is so little coincidence between athetesis and omission by our MSS. (bearing in mind the vast number of recorded atheteses), and that many of the coincidences can be put down to the operation of homoiographa, strongly supports the hypothesis that our MSS. show no or virtually no evidence of post-Aristarchean excision.

So much for the athetized lines: what of other coincidences?

We know that ll. 21. 213 was read by Aristarchus, as his version of the line is discussed in the "Ammonius" commentary. Van der Valk (RTSI II 515) comments, "The line is missing in a number of mss. and so we may conclude that it was condemned by some ancient critics." However, there would be no obvious motive for an excision, and there is no reason to suppose that the omission of the line by six MSS. is other than accidental, the result of the strong homoioteleuton 212 -δένης, 213 δένης. Van der Valk strangely seems to have overlooked the possibility of this obvious explanation.

There are a number of instances where post-Aristarchean excision seems somewhat less unlikely than this, but where accidental omission still seems far more probable. I have discussed two such examples elsewhere, Od. 10. 475-9 and 20. 46; let us now consider another. ll. 8. 244 is omitted by Pap. 1 (saec. v-vi p.C.) and is misplaced in seven fairly late MSS., the misplacement indicating transference from the margin of an exemplar. The line is not essential to the sense and recurs at ll. 15. 376, where it is present in all our MSS. There is a homoiomeson ll. 8. 243 ἐςαύ, 244 ἐα. The manuscript evidence against the line is too slight to make it likely that it is evidence of post-Aristarchean interpolation, especially in view of the
homoiomeson and the fact that the line is read by Pap. Ant. III. 158 (saec. iii p.C.),
though this hypothesis cannot be completely excluded, especially as the line is a δυσοροφυκός. The most plausible explanation of the behaviour of our MSS. is that the line was accidentally omitted through the operation of the homoiomeson. However, it is just possible that the line was deleted by some rash critic bred in the Alexandrian tradition who realized that the line recurred at II. 15. 376 and felt it to be more appropriate in the latter place.

To conclude: the evidence for post-Aristarchean excision from the text of Homer is meagre and ambiguous; and if such excision did occasionally occur its importance for the question of the numerus versuum is minimal.
NOTES ON CHAPTER II


2. Loc. cit. (above, n. 1) 30 n. 68.

3. Iliad: AJP 37 (1916) 3-7 (MSS.) and 22f. (papyri); Odyssey: ibid: 452-4 (MSS.) and 457 (papyri); for the Iliad papyri see also the Prolegomena to Allen's edition pp. 85-91.

4. Loc. cit. (above, n. 1) 28-31. I can now strengthen my case for the indispensability of Od. 10. 475-9 by pointing out that F.M. Combellack ("Omitted Speech Formulas in Homer", University of California Publications in Classical Philology Vol. 12 No. 4 [1939] 43-56) has considered, in much greater detail than I did myself (loc. cit. [above, n. 1] 24 n. 27), the circumstances in which a concluding speech-formula is absent in Homer, and that an immediate transition from Od. 10. 474 to 480, with omission of the speech-formula, would not come into any of the categories of exceptions listed by Combellack. For my knowledge of his article I am indebted to Dr. K.J. McKay.


7. H.J.M. Milne, Catalogue of the Literary Papyri in the British Museum (London 1927) p. 21 No. 11, says, "First century [p.c.]. (On the recto are accounts of the reign of Augustus.)" This should evidently be regarded as correcting the tentative dating to the third century p.c. in the original publication (F.G. Kenyon, Classical Texts from Papyri in the British Museum [London 1891] p. 94), which was largely based on the impression, presumably mistaken, that the accounts on the recto were probably saec. ii p.C.

8. For the dating see above, n. 7.

9. In the original publication Kenyon (op. cit. [above, n. 7] p. 82) stated, "The MS. is of comparatively late date, probably not earlier than the 4th or 5th century ...", but he revised the dating to saec. iii p.C. on pp. 105f. of The Palaeography of Greek Papyri (Oxford 1899), and this was accepted by Milne, op. cit. (above, n. 7) p. 2 No. 5. Allen gives the dating as saec. iv-v in both his editions, but this is presumably an oversight.


11. E.g. CP 17 (1922) 217.

12. Loc. cit. (above, n. 11).

13. The same objection applies to his earlier discussion at AJP 37 (1916) 23f.

15. Loc. cit. (above, n. 11) 217.

16. The homoioiteleuton -το in 409 and 410 could fairly easily have led to the omission of 410 but would not be particularly likely to lead to the omission of 409.

17. See Bolling, EE 228f.

18. We should note that, even if this passage was deleted, the situation may well have been rather different from that posited in the hypothesis which I put forward in my Introduction - viz. that "occasionally some post-Aristarchean critic ... [may have] excised a genuine line on the basis of the internal evidence alone" (above, p. xviii) - since Athenaeus's source probably knew some MS. of the pre-Aristarchean type which omitted the passage (cf. Bolling, EE 228). Moreover, the phrase τινὲς ... τροσφραγαν used of II. 2. 409 at Athen. 177C may have suggested to some of those who read it that there was some manuscript evidence against the line, Athenaeus may have believed as much himself, and this may in fact have been the case, either because there was a weakness in the pre-Aristarchean attestation of the line or because there was an early post-Aristarchean accidental omission.

19. TCO 277 n. 3: "Another critic, whose remarks are preserved in Athen. 193b, cancelled α 141, 2 and δ 58, 9" (read "δ 57, 8").

20. Cf. n. 18 above.

21. Eustathius in his commentary on Od. 1. 141 mentions Athenaeus by name, paraphrases his discussion and assumes that this is the condemned line, but this assumption has no value as evidence.


23. 354f., not 353f., according to Solmsen ad loc. (F. Solmsen et al., Hesiodi Opera [Oxford 1970]); but this question is irrelevant to our present discussion.

24. It is clear from Fragment 108 in Sandbach (above, n. 22), p. 220, that Proclus did not have access to any such edition (though of course this does not prove that Plutarch did not produce one).


26. Thus Ludwich (op. cit. [above, n. 25] Vol. II 140 n. 121) regarded παντελῶς ἐκβλητέον as equivalent to ἀπετητέον.
27. The isolated omissions of 98 and 99 by two and one of Ludwich's MSS. respectively are the result of the homoiarchon μὴ τὰς in each line: cf. Bolling, AJP 37 (1916) 5.

28. This list is an expansion of his earlier list in CR 15 (1901) 244 n. 1, repeated at Origins (above, n. 22) 305; note that in the Origins and Prolegomena lists "Ω 536" is a mistake for "Ω 556" (rightly in CR).

29. CR 15 (1901) 244 n. 1, and his editio minor ad loc. ("om. Ar."); and cf. his editio maior ad loc., where he quotes the relevant scholium. I shall discuss this line in Chapter V (below, pp. 146-51).


31. This line will be further discussed below, pp. 36f.

32. The earliest MS. to read the lines is Allen's P10 (saec. xii-xiii). They are a clear example of concordance interpolation: 466-8 = 35-7, 463-5 = 32-4.

33. So also R. Merkelbach, Gnomon 27 (1955) 273.

34. See above, n. 28 ad fin.

35. Allen acknowledges this inexactness in his CR list but not in his two later lists.

36. In his three lists Allen mentions only V1 and V23, but from his apparatus one can add three other MSS.

37. For similar criticisms of Allen's lists see Bolling, CQ 22 (1928) 104 and Merkelbach (above, n. 33).

38. Above, pp. 19f.

39. See also Merkelbach (above, n. 33).

40. See below, Chapter IV n. 15 (4).

41. See further below, Chapter V n. 29.

42. See also the remainder of Leaf's note, Monro and Van Leeuwen ad loc. and Van der Valk, RTSI II 226f.

43. In his apparatus Allen says merely "fortasse".

44. CQ 22 (1928) 104 n. 2; cf. ALT 104f.

45. It is possible that the athetized passage was not merely 557f. but 557-9: see Bolling, EE 115f.

46. For similar conclusions cf. Bolling, CQ 22 (1928) 104f.; Merkelbach, Gnomon 27 (1955) 272f.

47. Papers of the British School at Rome 5 (1910) 74.

49. See Allen (above, n. 47) 5f.

50. Cf. Schol. A on Il. 22. 158, quoted and discussed below, p. 35.

51. Dindorf prints the version of the paradosis, καὶ παροῦς, without comment and thus presumably without realizing its impossibility. However, an emendation such as I have printed is required (1) by the logic of the argument, (2) by the contents of Il. 11 and (3) by the parallel passage in Eustathius (1744. 50-56), who says καὶ Ἀθήνας ὄχι. I have followed the emended version — with post-Classical μή — printed by A. Kirchhoff, Die homerische Odyssee (Berlin 1879, Hildesheim 1973) p. 499, though if the phraseology at this point goes back to Aristarchus — as it may well do — οὖ may well be right; in fact καὶ<περ ὄν> παροῦς would conveniently account for the loss by haplography. Another possibility would be καὶ ἀποῦς, though I do not find this particularly attractive.


53. Cf. Von der Mühll ad loc.: "Damn. (?) Ar."

54. See e.g. Allen's Prolegomena p. 197; J. Baar, Index zu den IIias-Scholien (Baden-Baden 1961) 4f. s.v. ἀθετέω; and Dindorf's Odyssey-scholia pp. 806f. (Index) s. vυ. ἀθετοῦντας ὀτίχολ.

55. Van der Valk remarks that the line "cannot be listed under the heading of the στίχολ περὶτοτοι, because [it] adds an interesting detail about the human appearance of the god Skamander". In fact, however, the only conceivable motive for its excision would be the objection that it was superfluous and repetitious: 212 προνοέω, 213 φάεκτο; 212 θεός ὄν, 213 θεός ἐκ ὄν; and 212 on its own would arguably constitute an adequate introduction to the speech of 214ff. However, I do not regard post-Aristarchean excision on these grounds as at all likely.

56. No two of these six MSS. (whose dates range from the 12th to the 15th century) belong to the same family: four of them each come from a different family and the remaining two are classified among Allen's "independent MSS."; but (a) the homoioteleuton is so pronounced that omission could well have occurred independently more than once; and (b) if it did occur only once, there is no difficulty involved in attributing the manuscript omissions to descent from a common ancestor: Allen's Iliadic families tend to be much less closely-knit than his Odyssean ones, and four of the MSS. which omit Il. 21. 213 are not actually as isolated from each other as my initial statement suggests: V19 is a member of the family h; Mol is a member of the family c, which is related to h; the independent MS. V32 is related to c; and V32 shares with 05 a link with the MS. C: see the relevant sections of Allen's Prolegomena.

57. Bolling rightly classifies the omission as surface corruption at AJP 37 (1916) 7.

58. Loc. cit. (above, n. 1) 28 (2), 31, esp. n. 72.
59. The line is placed before 243 in six of Allen's MSS.: Ca2, O2 and V12, which are all fifteenth-century members of the family q; U2 (saec. xiii-xiv) and V3 (saec. xiv), both members of the family h; and P4 (saec. xv), a member of the family t. These three families are to some extent interrelated: see pp. 192f. of Allen's Prolegomena. Moreover, according to Ludwich's apparatus 244 is placed before 242 by the first hand of his P, = Allen's M8 (saec. xiv), which Allen classifies among his "contaminated MSS." and which is related to the family h.


61. Thus Bolling (above n. 57) classifies the omission as surface corruption.
It is natural to assume that some of the lines in our MSS. in whose attestation there is a significant weakness are survivals from pre-Aristarchean times which have re-entered the text from scholia, no longer extant, which quoted them. It is clear that a few such survivals have re-entered the text from extant scholia. The Zenodotean line 11. 13. 808a, quoted by Schol. A and T on 808, appears in the text of Allen's U10 (1486 A.D.) and U11 (saec. xv-xvi); the scholia (HMQ) on Od. 1. 93 quote the Zenodotean version of Od. 1. 284f., which appears in many of our MSS. as 93ab; Schol. T on 11. 13. 254 says ἐν τῷ μετὰ τὸν θυεῖαν Ἰδομενεύ ... (our 255): the line is found in the majority of the later minuscule MSS. but is absent from all four post-Aristarchean papyri which cover the passage and from the text of both tenth-century MSS. and of four of the five eleventh-century MSS., and these omissions make it highly probable that the line owes its presence in the text of the later MSS. to the scholia (and that - what is in any case likely - the τῶν of the scholium are pre-Aristarchean); 11. 22. 158a, cited by Schol. A (in a note which probably comes from Didymus) with the introduction ἐν ἐνίοτες δὲ φέρεται στεφάνος ὑπὸ τοῦ θυείαν εὐτελίς, has made its way into the text of Allen's U11 (saec. xv-xvi): Od. 10. 315, present in nearly all our minuscules, probably comes from the note of Didymus (preserved in Schol. Q) which quotes it; 11. 14. 231a, cited by Schol. T (Didymus?) on 231 (τῦνὲς δὲ <προσ>γράφουσι "ἐρχομενω ..."), though absent from all our minuscules and from Pap. 10 (Pack 899, saec. i p.C.), Pap. 26 (Pack 916, saec. ii p.C.) and Pap. 9 (saec. vi p.C.), is found in Pap. 60 (Pack 870, saec. iii-iv p.C.), whither it probably made its way from the ancestor of our T scholium; and the presence of 11. 2. 558 in the majority of the minuscule MSS. may well be due to the notes (and their ancestors) in the extant scholia, though on the other hand the line's début in the post-Aristarchean tradition may have been due to someone who knew a version of the story connected with the line from a source quite independent of Aristarchus's writings or any continuous commentary on Homer. As we have seen, Od. 11. 525, omitted by one papyrus but present in another and in all our minuscules, was absent from Aristarchus's text but mentioned in his commentaries, and its
interpolation in our vulgate seems to have started before the time of Didymus and Aristonicus. It seems likely that its entry into the post-Aristarchean tradition was due ultimately to its preservation in Aristarchus's ζηνόδοτος, whence it probably emerged initially in a brief interlinear or marginal note. Finally, there is the rather complex case of II. 13. 731. The T scholia on II. 13. 730 record: ζηνόδοτος δὲ ὁ Μαλλάτης προστεθησάν· ἢ ἄλλῳ δ' ἀρχηγῷ, ἐτέρῳ κῦθαρν καὶ δοῦλῷ" (= our 731), and this is confirmed by Eustathius (957. 10): προσγράφω κατὰ τοὺς παλαιοὺς ὁ Μαλλάτης ζηνόδοτος καὶ τούτων τῶν στύχων· ἢ ἄλλῳ δ' κ.τ.λ." The line is omitted in a quotation in the pseudo-Plutarchean De Vita et Poesi Homeri and also by the third-century p. c. Pap. 60 (Pack 870) and by a substantial minority of our minuscule MSS.: it is absent from the text of both tenth-century MSS., A and D, and four of our five eleventh-century MSS. (I include C, saec. xi-xii). Surprisingly, however, it is included in quotations by Plutarch, Lucian and Clement of Alexandria (for references see Bolling, EE 19). The line may have been interpolated for the first time soon after Aristarchus, but is more probably a survival from the pre-Aristarchean era in view of (a) its character: it is not a οὐφοροῦμενος (though κῦθαρς καὶ δοῦλος also ends Od. 1. 159 and Hymn to Apollo 188) and shows some originality; and (b) its early attestation: Zenodotus of Mallos (not to be confused with the famous Alexandrian Zenodotus of Ephesus) was a Κρατήτειος (Schol. AT on II. 23. 79), which would naturally mean that he was a personal pupil of Crates rather than a mere follower of his opinions, especially in view of the relatively short life-span of the Pergamenian "school" (see Pfeiffer 235), and Crates is said in the Suda to have been a contemporary of Aristarchus. It seems probable that it was this Zenodotus who was the author of the work Πρὸς τὰ Ἵπα 'Αριστάρχοι ἀδετοῦμεν τοῦ ποτηροῦ and not a third Zenodotus, designated in the Suda as ζηνόδοτος 'Αλεξανδρέως, γραμματικὸς, ὃ ἐν ἄστει κληθείς, to whom the Suda attributes it. We may conjecture, then, that Zenodotus of Mallos defended II. 13. 731 in his work on the atheletes of Aristarchus (which may well also have dealt with some lines which Aristarchus completely omitted from his text) - cf. Van der Valk, RTSI II 502 - and that some readers of this work who were impressed by his argument added the line to their texts. It is surprising that while the line is present in two-thirds of the available sources between Aristarchus and the end of the third century (if we include
Zenodotus of Mallos himself), it is absent from six of our seven earliest minuscules: this must raise the suspicion that (a) the line had considerably less support in the MSS. of the first four and a half centuries after Aristarchus than the meagre evidence preserved by chance prima facie suggests; (b) the stream which carried the line within the post-Aristarchean tradition had run dry, or (more probably) had become a mere trickle, by the tenth century; and (c) the mediaeval collections of scholia were solely or primarily or at any rate partially responsible for the line's capturing a place in the majority of the MSS. from the twelfth century on. However, while (a) seems probable, the hypothesis of (c), though possible, is by no means necessary, since the phenomena can be adequately accounted for by the supposition that the line survived the Dark Ages in the text of a single influential MS.

A post-Aristarchean invention which may have entered the text via a lost scholium explaining its genesis and meaning is Od. 1. 329a, ἕξ ποσίν ἐκβεβαίως τριλόκτυλος ἐξεφαινη, a pun on metrical terms by the emperor Julian, which rudely interrupts Penelope's distress in the text of Allen's Pal. (1201 A.D.) and VI (saec. xv), and reposes, with greater propriety, in the margin of U5 (saec. xiii). On the other hand it is possible that someone who knew Julian's joke simply wrote the line, without explanation, in the margin of his Odyssey. Secondly, the post-Aristarchean exegetic scholium on Od. 4. 598 has entered the text as 598a in a number of MSS., some of which add δῶρ to the final word Νέστορει in order to provide a hexameter-type ending, though the rest of the line remains grossly unmetrical.

We must now ask what the chances are that any given weakly-attested line should be a pre-Aristarchean survival which has come from a scholium now lost. Probably very low (except where we have a special reason for positing the existence of a scholium: see e.g. my discussion of 11. 8. 183 in my next paragraph). It is true that the extant scholia - particularly those on the later Books of the Odyssey - are fragmentary, but we must bear in mind that the vast majority of the additional lines which they quote or refer to have not been introduced into the text of any of our MSS. or post-Aristarchean papyri, and that even those which have tend to be absent from the papyri and from MSS. earlier than the twelfth century. Cf. Allen on the relative lateness of most of the Alexandrian intralinear readings
in our MSS. It is in any case natural to assume that the tendency to import material from the scholia into the text would increase as relatively extensive marginal scholia - as distinct from self-contained commentaries with no more of the text than lemmata - became increasingly popular after the introduction of the minuscule hand at the end of the eighth century. It seems likely, then, that the temptation to import the additional lines of the scholia into the text would have been strongest during a period when there was probably not a great deal of textual information in the margins of MSS. (or, for that matter, otherwise easily available) beyond what is contained in our extant scholia and in Eustathius.

We can next ask how many of the non-Aristarchean plus-verses of the "eccentric" Ptolemaic papyri reappear in some of the post-Aristarchean MSS. and vulgate papyri. The most conspicuous case is I. 8. 183, which is found in the third-century B.C. Pap. 7 (Pack 819), in Eustathius and in many of our MSS., but is absent from the only two post-Aristarchean papyri which cover the passage (Pap. 197 [Pack 817, saec. ii-iii p.C.] and Pap. 198 [Pack 820, saec. ii p.C.]) and from the text of all extant MSS. earlier than the 13th century. West (84f.) comments, "The presence of this line [in Pap. 7] is surprising. ... It does not occur elsewhere, and independent interpolation at two different stages in the tradition is most unlikely. Presumably it was omitted by Aristarchus, but was accidentally left uncancelled in a few copies." But why "copies" in the plural? It is extremely unlikely that such an accident would occur more than once in the case of any given line, in a single but influential copy; and the propagation of the error in that copy's descendants could hardly be termed "accidentally leaving the line uncancelled" in those descendants. When modified in the way I have indicated, West's theory is not impossible; but there remain two other possibilities: that the line was re-introduced not long after Aristarchus direct from an "eccentric" text, or that it was preserved in a scholium, now lost, from which it entered the text. In my opinion the lateness of the line's appearance on the post-Aristarchean scene makes this last explanation the most attractive of the three.

II. 22. 316 is found in the third-century B.C. Pap. 12 (Pack 979). It is also found in the sixth-century Pap. 9 - our earliest post-Aristarchean source covering the passage - and in the vast majority of
our minuscules, but six of them omit it from their text. These six are: our two oldest MSS., A and D, the only two which have survived from the tenth century, each from a different Allen family (in each MS. a later hand has added the line in the margin — see Ludwich's edition ad loc.); P15 (saec. xiii) and P6 (saec. xiv), which are, according to Allen (Prolegomena 104), "practically identical"; and Vi5 (saec. xiii) and W2 (saec. xv), which both belong to Allen's family i. If we had no evidence earlier than the tenth century, we would have little hesitation in judging the line to be non-Aristarchean: the combined weight of A and D is considerable, and they receive some support from the later tradition; there is no homolographic temptation to omission; the line is a δωροφορούμενος (= II. 19. 383), and a temptation to concordance interpolation exists (latter half of II. 22. 315 = latter half of II. 19. 382). However, the presence of the line in the sixth-century Pap. 9, combined with the evidence of Pap. 12 that the line at any rate originated in pre-Aristarchean times, must at least make us hesitate slightly: to put this as strongly as possible, in the twelve-century span between the third century B.C. and the ninth-century p.C. only two witnesses have had a chance to testify about the line, but both have spoken in its favour. Further, the combined weight of the omitting MSS. arguably falls just below the upper limit (as defined in Chapter II) normally allowable for accidental omissions. It is understandable, then, that A.G. Laird, in reviewing Bolling's EE, stated, "On MS. evidence I hesitate to reject X 316." Moreover, I do not believe a strong case can be made out against the line on the basis of the internal evidence; remarks like those of Leaf and Bolling, "The ... line is out of place in the old part of the poems where the ὁπλοστοία is unknown" (Leaf ad loc.), "... X 316 is the only evidence that the author of the Ἑκτόρος ἀναρρέως knew of the ὁπλοστοία" (Bolling), have an old-fashioned ring about them nowadays: even if we assume that the ὁπλοστοία was a relatively late accretion during the process of the gradual growth of a fairly short poem on the Μῆνις Ἀχιλῆος into a monumental Iliad in Homer's hands, there is no reason why, in the final version of his poem, Homer should not have added a reference to the workmanship of Hephaestus at 22. 316. However, I myself consider that the accumulated evidence against the line, as set forth at the beginning of this paragraph, outweighs the evidence in its favour, and that it is probably non-Aristarchean. There is no
difficulty in interpreting its attestation by Pap. 9 as serving merely to establish a terminus ante quem for its début in the post-Aristarchean tradition, especially as it is so strongly attested after the tenth century. In that case it will probably have entered that tradition either from a lost scholium which quoted it or direct from a pre-Aristarchean text — although, in the light of the temptation to concordance interpolation, we cannot completely exclude the possibility of independent post-Aristarchean interpolation.

11. 12. 130a, present in one pre-Aristarchean papyrus but absent from another, will be discussed in detail in Chapter V. If the line was ever present in any post-Aristarchean texts, this was probably due to the ancestor of the textual T scholium which cites it.

We have now established that a few of the weakly-attested lines of our MSS. which we can name were in the text of some pre-Aristarchean MSS. Only a small proportion of the remainder are liable to fall into this class. I have already argued that only a few of our weakly-attested lines not known to be pre-Aristarchean are likely to have come from lost scholia, and the same applies to the other possible ways of survival. It is, I assume, self-evident that lines would only very rarely have been "accidentally left uncancelled" (West 85) in texts whose numerus versuum was corrected in accordance with the edition of Aristarchus. Alternative theses — that the scriptoria consciously declined to accept the judgment of Aristarchus on many of the plus-verses in their texts, or that many lines were deliberately re-interpolated from "eccentric" texts not long after Aristarchus — are also unlikely in an era when there was obviously great concern to obtain a text purified of interpolations and when the numerus versuum of Aristarchus must have been regarded as authoritative, as is indicated by the rapid demise of the "eccentric" papyri and their replacement by texts which contained the Aristarchean numerus versuum. Moreover, it is noteworthy that so few of the numerous plus-verses in the extant "eccentric" papyri have survived in the text of any of our vulgate papyri and MSS. Of course the value of this observation is to some extent reduced by the fact that different pre-Aristarchean papyri were obviously interpolated in different ways — as we have just noted, one papyrus contains 11. 12. 130a while another does not, and we may add that one contains 11. 12. 250a while another does not (see West pp. 126 and 286f.) — but on the other hand there are also agreements.
in the manner of interpolation—two pre-Aristarchean papyri contain II. 12. 189b and 190a (see West pp. 93 and 120), and II. 2. 848a, cited in a papyrus commentary of c. 250 B.C., is known also from the scholia. Another point:—It might be argued that, just as an interpolation tends to gain increasing support with the passage of time, so a pre-Aristarchean line left uncanceled in one copy, or reinserted in a copy after comparison with an "eccentric" text, would tend to be transmitted in very few of the vulgate papyrus texts, and that the chances of its being found in the one or two extant papyri which happened to cover the passage would be small—in other words, that the external evidence in such a case would tend to be indistinguishable from the external evidence for a line which was interpolated for the first time after Aristarchus. This argument is reasonable as far as it goes, but the total evidence of the papyri against the weakly-attested lines of our MSS., combined with the tendency of our earliest minuscule MSS. to omit them, seems strong enough to imply that the vast majority of them were probably added at a stage when "eccentric" papyri would no longer have been extant.

To summarize: of the numerous lines in whose post-Aristarchean attestation there is a significant weakness, I would argue that only a few, over and above those found in the extant scholia and "eccentric" papyri, are likely to be survivals from pre-Aristarchean times.

This was Bolling's assumption too in 1925 (EE 14f.), though he did not argue the point adequately; but in 1944 (ALI 13) he declared that he had "long been inclined to believe" that he had "underestimated" the number of such lines, though he did not back his change of opinion with adequate argument either—in fact the tentativeness of his "inclined to believe" was deliberate, for he declared it impossible to form "a more definite opinion" in the present state of our knowledge. However, for the reasons set forth above I regard his earlier instinct as sounder than his later one and his 1944 emphasis on our ignorance as exaggerated. For example, in 1944 he asserted that "an attempt to gauge [the number of such lines] more accurately ... would seem to require ... a broadening of the investigation to include the verbal variants", but Allen had already conducted such an investigation, and his findings supported Bolling's earlier position; and in the same year Bolling stated, "It is quite possible that we should do better to think, for instance, of some Byzantine scholar endeavoring to improve
his (already somewhat interpolated) Alpha [i.e. Aristarchean] Text by combining it with his other sources of information", but the fact that the additional lines cited in our extant scholia have had so little influence on our vulgate militates strongly against this hypothesis in so far as it is intended (as it obviously is) to account for the majority of the weakly-attested lines of our MSS. (though Bolling concludes by calling the hypothesis merely a "possibility").
NOTES ON CHAPTER III

1. The occurrence of the line in the text of U10 and U11 is mentioned in the apparatus of the 2nd ed. (1908) of the Oxford editio minor by D.B. Monro and T.W. Allen; curiously, neither the 3rd ed. of the editio minor (1920) nor Allen's editio maior (1931) repeats this information, presumably either because it was thought to be of no consequence or through inadvertence—though the original entry may have been a mistake.

2. In this category I include Allen's C (saec. xi-xii). The papyri involved are Pap. 47 (Pack 903, saec. i a.C.), Pap. 10 (Pack 899, saec. i p.C.), Pap. 60 (Pack 870, saec. iii-iv p.C.) and Pap. 9 (saec. vi p.C.).

3. The present tense φερεται is no barrier to this judgment, since, as we shall presently see, there are numerous similar scholia, in T and elsewhere, where the line quoted is absent from all our MSS. and post-Aristarchean papyri, and in some of these cases there is positive evidence to link the scholium with the ὑπομνήματα of Aristarchus.

4. The ὀνειρη attached to line 158 in A strongly suggests that the additional line was mentioned by Aristarchus in his ὑπομνήματα.

5. The Q scholium runs: καλοῦ δαλαλέου] ἐν τοῦτοις [".Expectes τοῦτ νυν ἐν νοῦς" — Dindorf] ἐπετέθακτο τὸ "καλοῦ δαλαλέου, ὅπε ἐς φρήνος ποσῶν ἤεν", οὐ μέντοι καὶ ἀρσοτάρχῳ. One of Dindorf's conjectures almost certainly restores the original text; the lemma must be a recent addition, and the comment must originally have been attached to line 314: if it ran something like ἐν νοῦς (or: ἐν τούτων) τοῦτω ἐπετέθακτο στίξος "καλοῦ ..." κ.τ.λ., the corruption ἐν τούτοις could be a careless telescoping of the first three words. The tense of ἐπετέθακτο shows that the line was pre-Aristarchean. The fact that the vast majority of our MSS. contain it is rather surprising: only four omit it (including, however, the oldest, Allen's L4; there are no papyri). Could the scholia alone be responsible for this? Bolling (EE 14f.) assumes they were, and I think this is the most probable explanation: first, if there were as many early MSS. of the Odyssey as of the Iliad, we should probably find more which omitted the line; and secondly, although I shall argue presently in my text that the tendency to import material from the scholia into the text operated mainly at a relatively late stage, some such material undoubtedly started to enter the text at an early stage, presumably in most cases via brief marginal or interlinear jottings extracted from the detailed independent commentaries. However, it is just possible that Od. 10. 315 was re-interpolated, soon after Aristarchus, direct from an "eccentric" Ptolemaic text: this early start would easily account for the line's relatively strong attestation. Finally, in view of the strong temptation to concordance interpolation (cf. II. 18. 389f., Od. 10. 360f., I. 130f.), we cannot completely rule out the possibility of independent interpolation not long after Aristarchus.

6. I do not think the likelihood of this hypothesis is significantly diminished by the fact that our scholium cites the line with
κατὰ φθολα βροτῶν whereas the papyrus reads μετὰ.

7. This line will be discussed in detail in Chapter V (below, pp. 164-76.

8. See above, p. 4.

9. See F. Susemihl, Geschichte der griechischen Literatur in der Alexandrinerzeit Vol. II (Leipzig 1892, repr. Hildesheim 1965) 14f., esp. n. 83; the suggestion, put forward in different forms by Wolf and Schrader, that these two minor Zenodotuses may actually be one and the same person is rightly judged by Susemihl to be implausible.

10. I here append a list of lines of which this is true together with their corresponding scholia. It is a natural assumption that lines quoted in scholia of this type are pre-Aristarchean, and of course this assumption is strengthened by the very fact that the lines are absent from all extant post-Aristarchean MSS. and papyri, but where further positive evidence is available that a line is pre-Aristarchean I have indicated as much; sometimes this is clear from the very form of the scholium. T.W. Allen's list of "anonymous additions ... in Schol. T" (Homer: the Origins and the Transmission [Oxford 1924, repr. 1969] 288f.) contains some omissions and three incorrect references: I have supplemented the former and corrected the latter without comment. Now for the list:- II. 2. 848a (definitely pre-Aristarchean: for the scholia see below, pp. 53f.); II. 2. 555ab (Schol. T on II. 20. 329 τυνές δὲ καὶ φέρουσι τὸ "Καῦκωνας ..."); definitely pre-Aristarchean: see Bolling, EE 78); II. 8. 131ab (Schol. T on 130-31 ἐν τοῖς τῶν παλαμῶν φέρονται δύο στΈχοι "Τράδες ..."; the time-reference in παλαιῶν admittedly lacks precision); II. 8. 168a (Schol. A [Aristonicus] on 168 ἡ ὑποτάσσουσιν "ἡ μὴτε ...": there follows a detailed criticism of the line, and from the scholium as a whole, and the ὑπαλή attached to 168 in A, there can be no doubt that Aristarchus mentioned the line in his ὑπομνήματα); II. 9. 140a (Schol. A [Aristonicus] on 140 ἡ ὑπαλή, ὑποτάσσουσι στΈχοι "τὴν ...", εὕθεως πάνω: again, A has a ὑπαλή attached to 140, and, again, there can be no doubt that Aristarchus mentioned the line in his ὑπομνήματα); II. 9. 158a/159a (Schol. A on 159 [cf. Schol. T on 158] ἰφαῖσὶν ὁ Ἀρίσταρχος ὑποτάσσουσι στουτοῦ "οὗνεκ' ...": here the pre-Aristarchean origin of the line is made crystal clear, and Bolling's scepticism here and on the two preceding lines in my list [EE 118f.] is completely groundless); II. 10. 349a (added by Aristophanes: see Bolling, EE 127); II. 12. 238a (Schol. A [Aristonicus] on 328: the line is quoted [...] τυνές υποτάσσασι στΈχοι "δῶσε ..." within a note accounting for an Aristarchean ὑπαλή); II. 13. 367a (Schol. T on 367 τυνές δὲ ἔπαγγελτο "φοιτῶν ...": this immediately follows undoubtedly Didymean material on the same line, and Erbse ad loc. understandably attributes the whole scholium to Didymus, which would naturally suggest that he found the line mentioned in Aristarchus's ὑπομνήματα); II. 13. 433abc (Schol. T on 433 τυνές ἄλλοις ὑποτάσσουσι: "πολὺ ..."); II. 14. 136a (Schol. A [Aristonicus] on 136 ἡ ὑπαλή περιστομεγέν [preserved in A], ὑποτάσσει "ἀντιπέθυ ..."; cf. Schol. T ibid.); II. 14. 24lab
(Schol. T on 241 τινές ἐπάγομεν "αὐτάρ ..."); 11. 14. 236a (Schol. T on 263 γράφεται ...); 11. 14. 279a (Schol. T on 278f. τινές δὲ <προσ>γράφουσιν "έμπου ..." [προσγράφουσι τίνες καὶ ἔτερου στέχου τοῦτον Eustathius 985. 34f.]); 11. 14. 531a (Schol. T on 351 ἐπάγομεν δὲ τινὲς "δή ρα ..."); 11. 15. 5a (Schol. T on 5 προστιθέασι δὲ καὶ τὸ "Εξέτο ..."); 11. 15. 21ab (Schol. T on 21 τινές δὲ <προσ>γράφουσι "πρών ..." [Eustathius 1003. 13f. προσγράφουσι τίνες καὶ τούτους τοὺς στέχους]); 11. 15. 78a (Schol. T on 78 τινές μετὰ τούτου γράφουσι "Ζην ..."); 11. 15 689a (Schol. T on 689 ἔπευξαντος τοῦτο τόδε: "ἄλλα ..."); 11. 16. 607a (Schol. T on 607 ἐν τῷ ἐπεφέρετο "Μηρδυνθ ...": the tense suggests that the line was pre-Aristarchean); 11. 16. 867a (Schol. T on 867 τινές <προσ>γράφουσιν "Τματι ..."); 11. 18. 551a (Schol. T on 483-606, Erbse Vol. IV p. 530, top: ἐν δὲ τίςν ἔγραφετο μετὰ τῶν "ήμων ..." [551] "καρπών ..." [551a]: definitely pre-Aristarchean, because cited by Agallis of Corcyra [on whom see G. Wentzel, RE 1 (1894) 718]); 11. 19. 137a (Eustathius 1176. 11-14: definitely pre-Aristarchean, because cited by Dioscurides the pupil of Isocrates); 11. 23. 538ab (Schol. A [Aristonicus] on 538 [cf. Schol. T ibid.] ἢ διωλή [preserved in A],> ὅτι ἐν τιςν ὑποτάσσονται τοῦτω "τὰ τρίτα ...": the lines were thus known to Aristarchus); 11. 24. 205a (Schol. A [Aristonicus] on 205 [cf. Schol. T ibid.] ἢ διωλή [preserved in A],> ὅτι ὑποτάσσονται στέχου ... ἢδανατοι ...": the line was thus known to Aristarchus); 11. 24. 804a (Schol. T on 804 τινές γράφουσι ..., and mentioned in Pap. 104 [Pack 643, saec. i p.C.]); Od. 2. 51ab (Schol. HN on 51 Ἀρσενόφανής προστιθεομέν "ἄλλοι ...": the line was thus known to Aristarchus); Od. 4. 90a/93a (Schol. HN on 90 ἐν τιςν ὑπὸ τοῦτον φέρεται ὁτίχος "οὐδὲ τι ...", γελοείως κ.τ.λ.: this sounds like Aristonicus [cf. Bolling, EE 229f.], in which case Aristarchus will have known the line). 11. 12. 130a (cited and discussed by Schol. T on 130, certainly pre-Aristarchean, and not contained in any extant post-Aristarchean MSS. or papyri) will be discussed presently in my text.

11. See above, Chapter I n. 35.


13. There is one possible exception: the line is read by Allen's V13, saec. xii-xiii.

14. This explanation was regarded as probable by Bolling (EE 15), and Erbse, in his edition of the scholia ad loc., comments, "incertum an scholium Didymi exstiterit de hoc versu ... ."

15. AJP 48 (1927) 378. Bolling showed no such hesitation at AJP 37 (1916) 30, EE 22 or II. Ath. ad loc., but at EE 196 he did say "a verse that was apparently ... originally foreign to the vulgate" (my emphasis).


17. Cf. p. 3 above.

19. See below, pp. 53f.

20. See above, pp. 37f.
CHAPTER IV

DID ARISTARCHUS OMIT GENUINE LINES?

(1) The conservativism of Aristarchus

If we can accept with Van der Valk that Aristarchus actually excised many genuine lines from his text on mistaken subjective grounds and without external evidence against them, the conclusion reached in Chapter III will imply that some of the few survivals from pre-Aristarchean times (identified and unidentified) in our MSS. may well be genuine. However, the evidence demonstrates conclusively that Aristarchus did not proceed in this manner.

We learn from the scholia that a number of the lines which Aristarchus retained in his edition were absent from a greater or smaller proportion of the MSS. he consulted. Such lines he often athetized, but that was of course quite a different matter from total omission. Before proceeding, we must examine the terms used in the scholia to denote the proportion of his MSS. which omitted a given line. (The terms are of course also used in other contexts, to signify how many MSS. contained a particular intralinear reading.)

D.B. Monro, in the Appendix to his edition of Ὅδ. 13-24, has argued convincingly (pp. 432-4) that the term πᾶσαι, when correctly used, denotes all the MSS. which Aristarchus regarded as "superior" - αἱ χαριστεραι (sometimes αἱ χαρισταται) - and regularly mentioned by name when discussing readings, i.e. the κατὰ πόλεις and κατ' ἄνδρα texts.¹ Opposed to these were texts variously referred to as αἱ κωνῖναι, αἱ κωνίστεραι, αἱ δημώδεις, αἱ εἰκαστεραι, τὰ εἰκαστερα, τὰ φαιλότερα τῶν ἀνισοπράφων, etc.,² which Aristarchus must at least have consulted from time to time, and some of which he may have collated in full. As Monro has pointed out (Appendix 433f.), πᾶσαι cannot denote all the MSS. available to Aristarchus, for then "[the formula most commonly used,] ... ἀριστορχος και πᾶσαι[,] would express an absolute unanimity, and there would be nothing to comment upon. [This] is also shown in some cases by the form of the annotation: e.g. - ... 11. 13. 499 αἱ πᾶσαι ἔξοχον· οἱ δὲ ἔξοχον" (Schol. τ) ... "³ The πᾶσαι = αἱ χαριστεραι equation is supported by (inter alia) the following scholia, all cited by Monro (p. 434):

Schol. A on 11. 19. 95: οὕτως ἐν ἁπάσαις "Ζεὺς ἄσατο" ... ἐν δὲ
where Schol. A on \( \text{II. 2. 196} \) says
\[ \varepsilon \chi χ \nu δ \varepsilon \text{ καὶ αἱ χαριστάται} \]  
and where Schol. A on \( \text{II. 1. 598} \) says
\[ \nu \omega \nu \chi \nu \chi ε \nu \nu \nu \sigma \nu \nu \nu \nu \varepsilon \nu \nu \nu \nu \sigma \nu \nu \nu \nu \nu \nu \varepsilon \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \varepsilon \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu \nu 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χαριστεραὶ (αἱ τῶν, confirmed by A's ἂπασαί) is opposed to that of αἱ ὀκ. A similar judgment may be passed on the use of the terms πολλὰς and πολλαῖς in the scholia.

We may now move on to consider the evidence for Aristarchus's conservatism in establishing his numerus versuum. I shall present here the evidence which I consider to be the most important, though my treatment does not pretend to completely supersede Ludwich's: while I shall adduce some evidence which he has not adduced, there is other valid evidence which he has adduced but which I shall pass over in silence. Likewise, my treatment is no substitute for a thorough perusal of Parts III and IV of Bolling's EE — though I shall have occasion to disagree with some of his judgments on particulars.

I shall pass over cases where we are told that a passage in Aristarchus's edition was omitted by a single text — whether that of Zenodotus or one of the other χαριστεραὶ and start by listing the passages which Aristarchus included in his edition in spite of their absence from two of the χαριστεραὶ named by the scholia (and possibly, of course, from some of his other sources as well): II. 10. 497 and 15. 33, each omitted by Zenodotus and Aristophanes (the former line athetized by Aristarchus); II. 18. 10f., omitted by Rhianus and Aristophanes; and II. 17. 133-8, omitted by Zenodotus and the Chia.

I shall next list some passages which Aristarchus included in his edition in spite of their absence from "some" of his sources (τῶν, ἔννοι, ἔννοι): II. 20. 269-72, Od. 1. 171-3, 185f., 8. 81f., all of which he read but athetized; II. 2. 141, 5. 768, Od. 4. 569, 8. 333-42, all of which he read but probably athetized; and II. 17. 545f., 21. 195, Od. 3. 309f.

Next, we may note that Aristarchus read (but probably athetized) two passages absent from "many" of his sources: Od. 11. 428 and 15. 74.

We may next cite several cases where Aristarchus retained a passage in his edition even though it was absent from "the majority" of his texts — i.e. presumably, as we have seen, the majority of the χαριστεραὶ. Od. 19. 130-3, which he athetized (Schol. ἥθειντοι δὲ ἐν δὲ τοὺς πλεῦστος οὐδὲ ἐφέροντο); Od. 11. 454-6, which he probably athetized (Schol. οὐδὲ οὗτοι ἐφέροντο ἐν τοῖς πλεῦστοις,
A stronger phrase than ἐν τοῖς πλεύσον τοῖς πλεύσων is found in the Η scholium on Od. 4. 285-9: οὐκ ἐφερόντο δὲ σχεδὸν ἐν πάσας οὐκ πέντε. Aristarchus merely athetized the passage.

Finally, can we cite any examples where Aristarchus included in his text a passage omitted by all the χαριστήραι?

First, the scholia on Od. 1. 356-9 tells us that Aristarchus athetized the lines and that ἐν δὲ ταῖς χαριστήραις γράφεται οὐκ [οὐδ' coni. Dindorf] ἡσαυ. On the face of it this would mean that all the χαριστήραι omitted the passage, but it is not unlikely that, with condensation leading to oversimplification, some exceptions have been ignored.

Secondly, the scholia on II. 23. 92 report that Aristarchus athetized the line and that ἐν πάσας ὅπως οὐκ ἡν ὁ στίχος. Bolling (EE 198) comments, "The latter remark is misinterpreted by Gerhard (87) 'in allen Ausgaben gefehlt habe'; that would have been as at ὅ 511 ἐν οὐδεμιᾷ ἐφέρετο"; and the objection is repeated by West (171): "Gerhard mistranslates this 'der Vers in allen Ausgaben gefehlt habe'." Bolling and West presumably take the note on II. 23. 92 to mean "The line was not present in all editions, i.e. it was absent from some of them", but this is most unlikely to be the correct interpretation: if this were the meaning one would expect the negative to precede ἐν πάσας - if that phrase was to be retained; but the normal way of describing such a situation would be ἐν τοῖς οὐκ ἡν (or οὐκ ἐφέρετο). Moreover, as the formula οὐκ ἡν/ἡσαυ/ ἐφέρετο/-οντο is regularly used in the scholia to denote the absence of a passage from a group of texts, that group regularly being defined in a phrase of the type ἐν τοῖς, ἐν τοῖς πλεύσον τοῖς, etc. which precedes (rarely, follows) the negative verb, it is easy to understand how, through the force of analogy, absence from all the χαριστήραι could come to be rendered ἐν πάσας οὐκ ἡν, even though adherence to the usual verbal pattern led in this case to a slight grammatical error. Such an error can easily be paralleled. Thus Gerhardt's translation is in all probability right. Now the source of the scholium is Schol. T, so that (as we have seen) it is perfectly possible that πάσας is an exaggeration concealing some exceptions; but it is a striking fact that both of the two extant pre-Aristarchean sources which cover the passage - Aeschines I. 149 and Pap. 12
(Pack 979, saec. iii a.C.) - omit the line. This not only lends strong support to Gerhard's translation of the scholium but also strongly suggests that if πᾶσας is an exaggeration it is not a gross one.

Thirdly, the scholia (HP) on Od. 4. 511 say: ἐν οὐδεμιᾷ ἔφησεν. καὶ λέγει γὰρ ἑστὶν εὐτελῆς. ἡμιδισαμεν 6' ἄν πῶς καρδιάς τῶν Ἀριστοτέρου ἀπελεύσασκαί αὐτῶν. Bolling comments (ΕΕ 206), "The scholium is treated as Didymean by Blass, but it seems to me that Ludwich has good reason to doubt this: the question asked is too naïve, after we have just been told that the line was not in Aristarchus' edition." However, I would hold that we have not "just been told that the line was not in Aristarchus' edition": although the term πᾶσας in the scholia could (though need not) often be interpreted so as to include the edition(s) of Aristarchus, it is natural to suppose that it normally refers only to Aristarchus's ("better") sources, especially since, as Monro has pointed out, "the formula most commonly used (Ἀριστοτέρου καὶ πᾶσας) points to a distinction between πᾶσας and the Aristarchean text". This judgment is supported by the note of Schol. Τ on ΙΙ. 23. 92 discussed in my previous paragraph, since in this scholium (assuming that Gerhard's translation of it is correct) πᾶσας must exclude the edition of Aristarchus, which contained the line. It follows than ἐν οὐδεμιᾷ ἔφησεν is most likely to mean "The line was not present in any of Aristarchus's better sources", and that we cannot legitimately infer from this that it was absent from Aristarchus's edition itself. Moreover, the fact that the line is found in all the extant MSS. and in the only papyrus covering the passage strongly supports the view that it was read by Aristarchus. If it was, then Bolling's objection that "the question asked is too naïve" falls away and ceases to militate against the view that the whole of the scholium is Didymean. Moreover, as a parallel to the expression θαυμάσασας ἄν ἄν πῶς ... one may quote θαυμάσασε 6' ἄν τις ... πότεν ... from the indisputably Didymean Schol. A on ΙΙ. 3. 406. We may then ask why Aristarchus should have refrained even from athetizing Od. 4. 511. The answer may be that it had considerable support among the εἰκαστέραι, that it is not a διωροσμένος and that Aristarchus found the antithesis in 511f. pleasing (ὡς ὁ μὲν ἔνθε' ἀξιόλογον - σῶς ὅς κου ἔκφυγε κήρας ἄδελφος) and felt that an explicit reference to the death of Λιας
was desirable at this point. However, it is possible that Aristarchus did notathetize the line, and that, whereas the basic information ἐν οὐδεμιᾷ ἐφέρετο comes from Didymus, a later scholiast, finding no reference to athelesis in the sources available to him, wrongly concluded that Aristarchus had not athelesis the line and added the comment διαμάθαμεν Χ.Τ.Λ. (the authorship of the intervening καὶ ... εὐτελῆς being uncertain). It is even possible that the section of
the scholium from ἐν οὐδεμιᾷ to εὐτελῆς combines information from
Didymus on the reading of Aristarchus’s sources with a note in
Aristonicus giving the reason for an athelesis by Aristarchus.37
Finally, I would say that ἐν οὐδεμιᾷ ἐφέρετο is somewhat less likely
to conceal exceptions among the χαριεστέρας than either of the similar
expressions in connection with Od. 1. 356-9 and II. 23. 92.

To summarize: it is clear that Aristarchus included in his text
various passages against which he found some, considerable, or very
strong manuscript evidence.

It is also clear that Aristarchus was prepared to include some
passages in his text even when he believed that the external evidence
against them was supported by the internal evidence. Among the
passages already listed there are six clear cases where the scholia
provide us with one or more excerpts from Aristonicus giving for an
athelesis by Aristarchus a reason or reasons based on the internal
evidence and where there are no grounds for doubting that Aristonicus
obtained the reason(s) from Aristarchus's own ὑπομνήματα; I list the
passages, giving also the external evidence against each: II. II. 497
(omitted by Zenodotus and Aristophanes); 38 II. 20. 269-72 (omitted
ἐν ἔνθες); 39 Od. 1. 171-3 (omitted ἐν τὸν); 40 Od. 4. 285-9
(omitted σχοδὸν ἐν πάσας); 41 Od. 1. 356-9 (omitted ἐν ταῖς
χαριεστέρας γραφές); 42 II. 23. 92 (ἐν πάσας ὑπὸ ἦν). 43 Among the
passages already listed, it is highly probable that Aristarchus
thought there was some internal evidence against (a) II. 2. 141,
5. 786, Od. 4. 569, 8. 333-42, each absent from "some" of his
sources; (b) Od. 11. 428 and 15. 74, each absent from "many" of his
sources; and (c) Od. 5. 337, 11. 454-6 and 19. 130-33, each absent
from "the majority" of his sources. 44 Returning to the first list
of this paragraph, where the evidence for objection by Aristarchus
on the basis of the internal evidence is clearest, we should stress,
first, that there was very strong external evidence against
Od. 4. 285-9 and 1. 356-9 and also (if Gerhard's translation of the scholium is correct) against II. 23. 92; and secondly that against Od. 1. 171-3 Aristarchus was able to raise an unusually strong objection on the basis of the internal evidence. Yet he admitted all four passages into his text, content merely to brand them with marginal obeloi.

The other side of the coin is that we know of no lines omitted by Aristarchus which were present in more than "some" of his sources. A full justification of this statement will have emerged by the end of this book, but one apparent exception can conveniently be considered here. In II. 21. 152-60 Asteropaios introduces himself to Achilles as the leader of the Paeonians, but in the Catalogue (2. 848-50) the only Paeonian leader mentioned is Pyraichmes. The difficulty is discussed both by Schol. T. on II. 21. 140 and in the "Ammonius" commentary, Pap. Oxy. 221 (= Pack 1205, saec. ii p.C.), Column VI lines 16-30 (on II. 21. 155f.), and both refer to two alternative solutions: (1) insert a line after II. 2. 848 mentioning Asteropaios; (2) regard Asteropaios as not qualifying for mention in the Catalogue as being merely a subordinate leader until the death of Pyraichmes (II. 16. 284-92). Now Schol. T, after propounding the dilemma, proceeds as follows: καὶ οὐ μὲν (i.e. those favouring solution (1) above) ὑποτάσσομεν στὶχον ἐν τῷ τῶν Παιόνων καταλῦσι ... "Πηλεγύνος θ' υίὸς περιδέξιως Ἀστεροπαίος" [II. 2. 848a], δὲν καὶ ἐν πολλαῖς τῶν Ἰλιάδων φέρεσθαι <φασιν> (i.e. in many pre-Aristarchean MSS.). <οὐ ὀς φασιν> 47 οὐ θαυμαστῶν τὸ παραδεξαῖθαι αὐτὸν ἐν τῷ Καταλύσι κ.τ.λ. (= solution (2) above). According to Schol. T, then, the additional line was present in "many" pre-Aristarchean MSS. However, the Ammonius commentary, after citing II. 21. 155f. in a lemma, proceeds as follows: ἐν τῇ κατ' Εὐπριτίξθην καὶ ἐν τινι ἀλλαίς καὶ ἐν διὰ[καθ]θημ' ἀνεγνώθην 49 'Αστεροπαίος οὕτως · "... Πηλε[γύνο]ς θ' υίὸς περιδέξιως[ος] 'Αστεροπαι[α]ς" [II. 2. 848a] ... el] μὴ παραδέχομαι δὲ τῶς τῶν [ἐν διὰ κάθ]θῆν περὶ αὐτοῦ στῆ[χον], οὐδὲν χαλαῖκα κ.τ.λ. (= solution (2) above). According to "Ammonius", then, the additional line was present in the edition of Euripides and in some others" - a much less strong statement than Schol. T's ἐν πολλαῖς. There are sound reasons for regarding the Ammonius version as the more accurate of the two:— (a) The Ammonius version has greater authority as being the earlier of the
two: the papyrus was written in the second century p.c. and its
scholia were probably compiled in the latter half of the first
century p.c., whereas the MS. T was written in 1059 A.D. and the
bT compilation on which its scholia are based was Byzantine.
(b) The Ammionius version commands greater respect because of its
greater particularity, one of the MSS. being identified by name.
(c) The Ammionius version is stylistically more objective than the T
version: τολλατς in Schol. T occurs within an oratio obliqua clause
reporting what is said by those who favour the solution of inserting
II. 2. 848a, and it is natural that those who favoured this solution
should have made the most of the limited manuscript support enjoyed
by their protégé (or at any rate that they should have been represented
by the scholiast as so doing); "Ammionius", on the other hand, simply
reports the attestation in propria persona. (d) The insertion of II.
2. 848a provides a convenient λόγος of an obvious difficulty - that,
of course, is why it was interpolated in the first place. Now
Aristarchus was not untroubled by inconsistencies between the Catalogue
and the rest of the Iliad - the inconsistency which would result if
II. 2. 558 were admitted to the text constituted one of his reasons for
excluding this line (as we shall see in Chapter V). Of course a text
without II. 2. 848a could be defended by exegesis; but compared with
the prominent Asteropaios (II. 12. 101-3, 17. 217, 351-5, 21. 139-204,
23. 560-62, 807f.) Pyraichmes is mere cannon-fodder (16. 284-92 only),
and other subordinate leaders, both Achaean and Trojan, are named in
the Catalogue, so that (to say the least) Aristarchus could hardly
have judged the internal evidence to be positively inimical to 2. 848a.
That being so, he would not have omitted it from his text unless it
was absent from the vast majority of his MSS. That someone jotting
down Homeric parallels in private notes on an unknown poet in about
250 B.C. happened to think of περιδέξιος 'Αστεροπαίος (Pap. Hamb. 137,
Pack 1221, West pp. 59-62) does not conflict with this conclusion: it
confirms the report of "Ammionius", just as Schol. T's report that
τιμές ἐγγύουν II. 12. 130a is confirmed by the presence of the line
in one pre-Aristarchean papyrus (though it is absent from another),
but West (61) goes too far in claiming that the "occurrence [of II.
2. 848a] in this papyrus shews that it must have had a considerable
currency". 
The conclusion to which the evidence clearly points, then, is that Aristarchus, in establishing his *numerus versuum*, proceeded extremely cautiously. We have no reason to believe that he would ever have allowed his own assessment of the internal evidence to lead him to exclude a line from his text unless he also had very strong external evidence against it, i.e. unless it was absent from the vast majority of his MSS. Of course Aristarchus not only counted MSS. but weighed them - the fact that he singled out certain MSS. (the χαριστήρων) for special attention shows this - but the fact that he retained in his text some lines which were absent from all or nearly all his χαριστήρων even though he also found internal evidence against them shows that he could not have ignored the εἰκαστήρων when establishing his *numerus versuum* (even though they may well have patently been, in general, the more heavily interpolated group of texts). This conclusion could be avoided if we were to play up the possibility of unmentioned exceptions in expressions such as ἐν ταῖς χαριστήρων and if there were evidence that presence even in one of the χαριστήρων was enough to guarantee a line a place in Aristarchus's edition, but the evidence is in fact the opposite of this, since Aristarchus omitted the following lines present in at least one of the χαριστήρων: Od. 2. 51a, present in the edition of Aristophanes; Ο. 10. 349a, present in that edition "and others"; Ο. 2. 848a, present in the edition of Euripides "and in some others", as we have just seen; Ο. 2. 866a, also present in that edition (according to Eustathius; προσγράφουσα τως according to Strabo); Ο. 5. 808, 13. 808a, 14. 136a, each present in the edition of Zenodotus. Further, any suggestion that Aristarchus may have excluded lines present in the majority of his εἰκαστήρων must be ruled out. As we have seen, he included various lines absent from "most", "nearly all" or "all" the χαριστήρων in spite of his objections to them on the basis of the internal evidence, and where the adverse testimony of the vast majority of such highly-valued witnesses was insufficient to move him to exclude such a line, we cannot suppose that the adverse testimony of a mere minority of the despised εἰκαστήρων would have been allowed to prevail. Moreover, it is highly unlikely that lines so very weakly attested in the χαριστήρων would have been present in more than half the εἰκαστήρων: even if we assume that the εἰκαστήρων were heavily interpolated, such interpolations as were absent from nearly all the χαριστήρων would be unlikely to have spread very widely among the
elkai ôitéra, and the evidence of the pre-Aristarchean papyri shows that different texts were interpolated in different ways. A fortiori, then, the lines which Aristarchus excluded from his edition would have been present in no more than a minority of the elkai ôitéra. The case of II. 23. 92 provides external support for this conclusion. As we have seen, the scholia tell us that it was absent from "all" the xarékôitéra. Aristarchus objected to it on the basis of the internal evidence, and modern critics have followed him (see e.g. Leaf ad loc.). But he retained it in his edition, merely obelizing it. Was it present in as many as half his elkai ôitéra? This is most unlikely, as the two extant pre-Aristarchean texts covering the passage would presumably both have been classified by Aristarchus as elkai ôitéra, and both omit the line: Pap. 12 is presumably merely a typical product of a commercial publishing-house, and has some odd plus-verses (see e.g. West 179 on II. 23. 162a), and the quotation by Aeschines, with its three plus-verses, comes from a very inferior text: see the strong condemnation by Leaf on II. 23. 77-91, and West 171f. Of course it is reasonable to suppose that if Aristarchus regarded the internal evidence against a line as exceptionally strong he would have been prepared to exclude it with somewhat less external evidence against it than he would normally have demanded; but the evidence for his extreme conservatism in establishing his numerus versuum is so abundant and so strong that the judgment expressed in the first two sentences of this paragraph must be allowed to stand without qualification.

(2) Did Aristarchus's sources ever mislead him into omitting genuine lines?

We must now ask: could some of the lines against which Aristarchus had very strong external evidence conceivably be genuine? What factors, if any, could have led to the omission of genuine lines from the vast majority of his MSS.?

(a) Oral transmission

(i) The possibility of accidental omissions in some of Aristarchus's sources through oral transmission

The first point to be discussed is whether our extant minuscule MSS., and the earlier MSS. of which we have a partial knowledge through papyrus fragments, reports in the scholia and elsewhere, and quotations
by Classical authors, show, by their divergences, any signs of one-time oral transmission: are they descended from a single archetype or from different archetypes reflecting slightly different oral versions of the poems? And after the poems were first put into writing, to what extent (if any) and by what means did the rhapsodes alter the transmitted text? For if some of the divergences of the written tradition are the product of oral transmission, we must consider whether omissions of lines by some pre-Aristarchean MSS. may not sometimes be due to a lapse of memory on the part of one of the professional singers or reciters who dictated or wrote down the Homeric poems or who, at an earlier stage of the transmission, learnt and performed the poems and taught them, entirely orally, to disciples.

Perhaps the strongest evidence for oral transmission as the source of such divergences — whatever we eventually decide to make of it — consists in the presence within the universally-attested lines of numerous instances of variants where the two versions differ widely from each other. The scholia report some such variants in the pre-Aristarchean texts, but the most copious sources of information are the extant MSS. and papyri themselves: it is natural to suppose that the vast majority of such variants in the post-Aristarchean MSS. and vulgate papyri go back to pre-Aristarchean times. Examples will be cited presently, but we may first quote two diametrically opposed views on this question. Bolling wrote in 1925 (EE 33f.), "All variants known to us are of the kind that arise in a written, not an oral, tradition, and that tradition can have, on account of its small divergence, but a single fountain-head. ... I shall add ... my belief that this fountain-head of our tradition is an Athenian text not earlier than the sixth century." In 1949 he was even more definite on the latter point: "All that we know about Homer comes from a single manuscript written in Athens in the sixth century." On the other hand Milman Parry wrote in 1932, "[The] variant readings [of our manuscripts], while some are due to copyists, are for the greater part the variants of an oral tradition." Who is right, Bolling or Parry?

The first thing which needs to be said is that one cannot possibly support Bolling's view in the form in which he has expressed it: many of the variants are undoubtedly such as could well have arisen in an oral tradition. I would go further than this and say that there are many intralinear variants which are arguably most naturally accounted
for by the supposition of some form of oral tradition.

Some of the widely-differing variants could be fairly plausibly explained as conjectures, within a written tradition, designed to facilitate or improve the text, but most cannot be plausibly explained in this way. At Od. 23. 187f., for example, Odysseus says of his bed ἀνάφων ἄρ' οὖν κεν τις ζωὸς βροτὸς, οὔδε μαλ' ἠβάν, ἓνεκα μετοχιζομένον according to Pap. 20 (Pack 1148, saec. iii p.C.) and the majority of our minuscules, but οὔδε γυναικῶν is read by Pap. 28 (Pack 1106, saec. iii-iv p.C.) and by a substantial and weighty minority of our minuscules - four of Allen's families (including our two earliest minuscules, L4 and L8) and also, according to Ludwich's edition, his M (= Allen's V5, saec. xiii) and X (= Allen's V4, saec. xiii), and it is mentioned as a variant by Eustathius. The context strongly supports the former reading, which must surely be right, and the most natural explanation of the latter reading is arguably that it was a blunder in oral transmission by a reciter who was momentarily mesmerized by the frequent combination ἀνάφων ἄρ' γυναικῶν etc. An ordinary scribe, with the written text before him, would be much less likely to be mesmerized in this way, and if he was so mesmerized we would not have expected the error to spread as far as it has - for the MSS. and papyri, when weighed as well as counted, are very evenly divided here. And this brings us to another point: there are many cases where the MSS., pre- and/or post-Aristarchean, are more or less evenly divided between such variants. One such case is Od. 4. 668, where nearly half our MSS., including our earliest minuscule, L4, read ἄλλα οἱ αὐτῷ | ζεὺς ὀλίσσει βῆνι πρὶν ἔμηκεν μέτρον ἱκσάων, while the majority, and Apollonius Dyscolus, read πρὶν ἔμηκεν ἔμηκεν γενέσαιον. The scholia (ἩQ) tell us that the former was read by Aristarchus, the latter by ἀι κοινοτέρων: the natural implication of this wording is that the former reading was also to be found in at least some of the χαριτώτερων. The notion of Van der Valk (TCO 162f.) that the former reading is a conjectural emendation by Aristarchus is extremely implausible, not only because of the consideration just mentioned, but also because (1) the former reading occupies such a large proportion of our MSS., (2) the latter reading contains nothing which Aristarchus could have regarded as seriously objectionable, and (3) the reluctance of Aristarchus to alter even the intralinear text without manuscript authority is well documented: see e.g. Monro, Appendix 445f. What, then, is the origin of the variants? One can think of various
remotely possible reasons why one reading, at some stage before Aristarchus, might have been deliberately changed to the other, but even if we were to suppose that someone — against all likelihood — motivated by one of these considerations to make the change, the relatively even division of the tradition would remain surprising. Once again, then, this division is arguably most naturally explained by the vagaries of an understandably non-infallible memory during the process of oral transmission: perhaps the original reading was ἤμνης μέτρον ἔκθεται (which suits Antinous’s νεός πᾶς, 665), which was corrupted to ἤμνη πήμα γενέσθαι under the influence of the somewhat similar line Ὀδ. 17. 597, τοὺς Ζεὺς ἐξολέσεις ἤμνη πήμα γενέσθαι. There is also a third variant here, ἤμνη πήμα φυτέυσαι, read by M2 (saec. xiii), R5, (saec. xiii–xiv) and Eustathius, which could well also have arisen during oral transmission.

A final and particularly significant example: at Ὀδ. 20. 289 the majority of our MSS. read ὃς ὁ τοῦ κτείτεσσι κεπολωθὸς κατρῶς ἐτέλ | μνᾶσκετ 'Οὐσιος ... ἀδιακτά, but a fairly substantial minority — three of Allen’s families (including our oldest minuscule, Allen’s L4), Eustathius, and also, according to Ludwich’s edition, his X (= Allen’s V4, saec. xiii) — read κτείτεσσι κεπολωθὸς θεσσεσσοῦσαι. Both readings give excellent sense; both are thoroughly Homeric; yet neither combines with the preceding words to form anything remotely resembling a formulaic sequence; in fact there is not a single parallel passage to tempt a scribe to make either a deliberate alteration or an accidental slip.73 κατρῶς ἐτέλ and κατρῶς ἐμοτο are themselves formulae, coming usually at the end of a line, as here,74 but nowhere do they occur in conjunction with anything resembling κτείτεσσι κεπολωθὸς: I can find nothing closer to this combination than Η. 6. 47 πολλὰ δ’ ἐν ἄφνειοι κατρῶς κελμῆλα κετταλ. Likewise θεσσεσσοῦσαι as a line-ending has its parallels,75 but I can find nothing closer to κτείτεσσι κεπολωθὸς θεσσεσσοῦσαι than Η. 2. 670 καὶ φῶν θεσσεσσοῦν πλούτον κατέχεις Κρονίων. Moreover, I can think of no plausible reason at all why a deliberate change from one reading to the other should have been made. However, an accidental change could easily have occurred in oral transmission, through a lapse of memory, in spite of the absence of any specific temptation.

We should conclude that there is good reason for supposing that many of the divergences of the Homeric MSS. are probably to be explained by the operation of some form of oral transmission. At the
very least, we must pronounce Bolling's supreme confidence that the
tradition represented by the MSS. (both pre- and post-Aristarchean) is
entirely a written one to be unjustified.

To the evidence of the radically different intralinear variants
one might add the evidence of the very high proportion of plus-verses
in the "eccentric" Ptolemaic papyri, though I am reluctant to press
this, since the difference of this phenomenon from that of
interpolation in the written post-Aristarchean tradition is, at any
rate on the whole, one of degree rather than of kind. However, the
degree of difference is certainly great, and if rhapsodes were
responsible for at least some of the pre-Aristarchean additional lines,
as is commonly assumed (see e.g. West 13), the question of the manner in
which they added them must receive more attention than it is normally
given, and this is a matter to which I shall return presently.

M.W.M. Pope came to the conclusion that "what we must have ... to
account for the variant readings is different dictations from an
originally single version. This version one must imagine not as a
written one but as a memorised one, learned from the lips of the poet
..., and transmitted at first by singers, who would naturally retain
for some time their capacity for original composition, and later
perhaps by pure reciters." It is not clear whether Pope envisages
a version memorized virtually verbatim by the poet, who taught it to
his disciples by frequent repetition, or a "single version" which was
single because the poet recited it in precisely that form only once,
section by section, specifically in order that it might be memorized
by his disciples, who succeeded in doing so almost perfectly from that
single recitation. The latter interpretation might seem to be
supported to some extent by Pope's remark in his previous sentence
that "to do this [viz. to collate different versions of a story into a
single text] even with different versions taken down from the same
singer would be difficult enough", though in my opinion the former
interpretation, whether or not it accurately represents Pope's meaning,
offers a far more plausible hypothesis, in that the posited degree of
accuracy of memorization by the poet's disciples is much easier to
account for if the poet himself knew and repeated his composition
almost verbatim. Strong objections would be raised against both
hypotheses by a number of students of comparative oral poetry, but I
regard the arguments of G.S. Kirk, who has given careful consideration
to the important Yugoslav material, as sufficient to show the possibility of highly accurate memorization by reciters without written texts in a reproductive oral tradition; and though Kirk has not adequately come to grips with the question of how likely a highly creative oral poet (as opposed to a primarily reproductive singer, or a mere reciter) would be to memorize his own work almost verbatim, so that there would be a relatively fixed version at the outset to be orally handed down, the evidence within the poems themselves of a good deal of memorization in the process of composition – the repetition of clearly non-formulaic passages (i.e. passages which are not themselves formulae), sometimes of considerable length, where one passage is separated from its twin by hundreds or thousands of intervening lines – is, in my opinion, sufficient to render plausible the view that "Homer" (i.e. the monumental poet or poets of the Iliad and Odyssey) would be able to repeat his own poems virtually verbatim. I regard the major difficulty for Pope's explanation of the variant readings to be not that in this case the differences of the MSS. (especially the pre-Aristarchean MSS.) from one another would be greater than they actually are, if by this is meant no more than that in a purely oral transmission we should naturally expect greater variation than this, but rather the strong internal evidence that the poems have been expanded, after their initial monumental composition, by very substantial spurious additions which were apparently not reflected in the divergence of the pre-Aristarchean MSS. For example, Od. 11. 565–627 has been very widely regarded as an interpolation in the text of the Nekyia, almost certainly rightly; we know that Aristarchus regarded the passage as spurious, and the scholia contain, besides a general reference to its condemnation (with the citation of a reason based on the internal evidence), a series of notes (on lines 570, 577, 593, 601, 602) attacking various details within it; yet we have no evidence that Aristarchus found it lacking in any of his MSS. The argumentum ex silentio cannot be pressed too hard, but in view of the length of the passage, and the detailed attack on it preserved in the extant scholia, we should have expected a record of its omission by some pre-Aristarchean MSS. to have come down to us if there had been any such omission. Of course if the Nekyia in our Odyssey was based on a short independent poem telling of a visit by Odysseus to the Underworld, we cannot rule out the possibility that the passage 11. 565–627 was
inserted into this independent poem before the latter's incorporation into the *Odyssey*, in which case we would of course not, on any theory of the transmission of the text, expect there to have been MSS. of the *Odyssey* containing a Nekyia without 11. 565-627; but then the case for the spuriousness of the whole Nekyia episode of Od. 10 and 11, already strong on other grounds, would become overwhelming, given the post-Homeric concept of the Underworld which Od. 11. 565-627 reveals; and if the whole Nekyia episode is spurious, it is worth noting that the absence of any reference in our surviving sources to MSS. of the Alexandrians which omitted the long passage 10. 490-12. 38 (approximately) suggests very strongly that there were no such MSS. Next, there are very strong arguments, based on both language and content, in favour of the view that the end of the *Odyssey*, from 23. 344 on, is a post-Homeric addition, yet there is not the slightest shred of evidence that this passage was absent from any of the MSS. known to the Alexandrians. Had it been, it is virtually certain that Didymus would have recorded such a huge and startling divergence between the edition of Aristarchus and some of the latter's sources, and that our scholia, which contain a great deal of information about pre-Aristarchean omissions, would have included this remarkable and important case. In fact we know that Aristophanes of Byzantium and Aristarchus judged the genuine *Odyssey* to end at 23. 296, not 343, and that Aristarchus athetized 310-43; there is actually no internal evidence against 297-343 of any great weight, though the passage could be spurious. Now the opinion of the two great Alexandrians that the genuine *Odyssey* ended at 296 is reported by nearly all the major collections of scholia used by Dindorf and by one of the minor ones, and also by Eustathius: again, if Aristarchus had known of any MS. of the *Odyssey* which ended at this point he would have recorded the fact in his *ὑπομνήματα*, Didymus would surely have reported this remarkable large-scale divergence, and at least some of the collections of scholia which report the opinion of Aristophanes and Aristarchus would surely have included the information on the supporting diplomatic evidence from Didymus: in the circumstances the total silence of the scholia on this point suggests very strongly that the judgment of the two Alexandrians was based on the internal evidence alone. Finally, II. 10, the Doloneia, has been very widely regarded, on grounds of language, style, content and its relationship to the rest of the *Iliad*, as a post-Iliadic interpolation, in many respects *Odyssean*
rather than Iliadic in nature, and perhaps even of a post-Odyssean date; and again, as it happens, the scholia raise the question of its authenticity: ἔσο ν τὴν βαφφούς ὑπ' Ὀμήρου ἱδῇ τετάχθαι καὶ μὴ ἣναι μέρος τῆς Ἱλιάδος, ὅπο θ' Πελοπόννησου τετάχθαι εἰς τὴν ἴννα ποιήσεων (Schol. Τ, repeated almost verbatim by Eustathius at 785. 41-3).

Bolling (EE 31 n. 1) comments, "Schol. Τ at K 1 merely records an opinion of certain unnamed critics; its silence about the existence of MSS. lacking the Doloneia is practically an admission that there were none such." Now in fact it is uncertain whether Schol. Τ is here recording merely a conjectural opinion of some (Alexandrian?) critics, as Bolling seems to assume, or whether the scholium represents a tradition with historical origins, at least to the extent that the Doloneia was first inserted in the Iliad in the sixth-century Panathenaic text. However, it remains highly improbable that among the Alexandrians' MSS. there were any lacking the Doloneia; had there been, we should have expected this fact to have come down to us, for the same reasons as those which I have just mentioned in connection with the end of the Odyssey.

Now if one accepts that the Homeric poems were expanded by very substantial spurious additions which were not reflected in the divergence of the Alexandrians' MSS., Pope's thesis encounters certain difficulties, for it could be argued that we should naturally expect that one singer would have expanded the poems in one way, another in another, with the result that substantially different versions of the poems would have been offered in different branches of the oral tradition: for example, while one dictation of the Odyssey would have included the present ending of the poem, the chances that another dictation by some other singer or reciter would have omitted it would have been high. Pope's theory, it could be argued, can respond to this challenge only by supposing that the poems were expanded uniformly, that the tradition was highly cohesive over a lengthy period, that whenever one singer made a substantial addition to the poems all other singers learnt it from him and incorporated it into their own versions. However, this supposition of uniform expansion hardly seems likely. It might be suggested that the major additions to the poems were made in Chios (or wherever else in Ionia "Homer" lived) while the poems were still the exclusive property of a single close-knit group of aoidoi, the earliest "Homeridae", and that only after the urge or ability to expand the poems (except in a very minor way) had been
extinguished, and they had become virtually crystallized in a single expanded form, did they spread to the Greek mainland through the mouths of travelling reproductive singers and reciters. Now one cannot be dogmatic about the date of the Homeric poems' monumental composition and their spread to the mainland, but the cumulative weight of the evidence (such as it is) tends to suggest that the Iliad was composed soon after the middle of the eighth century, the Odyssey towards the end of that century, and that the Iliad began to become known on the Greek mainland in the last years of the eighth century, the Odyssey either at the same time or in the early years of the seventh century. It would seem to be unlikely that all the major additions to the Homeric poems should have been made in the brief period between their composition and the beginning of their dispersal through the Greek mainland, especially if Page is right (or even close to the truth) in his judgment on a section of the "continuation" of the Odyssey, 24. 205-411: "The evidence is indeed overwhelming, and leaves no room whatever for doubt: that scene as a whole was composed by a poet familiar with the idioms, syntax and vocabulary of an era not earlier than the sixth century B.C."; "... was composed at an era much later than the bulk of the Odyssey"; and the thesis of uniform expansion becomes implausible if much of that expansion took place during the period of the poems' dispersal: it is hardly likely that for a century the poems were performed on the mainland by a single group of singers or reciters, whose membership changed as older members died and young men were accepted as apprentices, but which always travelled from one place to another as a group, so that it was the single expanded mainland version which came down to us - especially unlikely when we bear in mind the probability that singers and rhapsodes would have continued to come over from Ionia to the Greek mainland at various points in time after the beginning of the poems' dispersal, just as, at a much later stage, in the closing years of the sixth century, the rhapsode Cynaethus of Chios turned up in Syracuse, and, still later, Ion of Ephesus was apparently touring the Greek mainland towards the end of the fifth century.

However, Pope's thesis can be salvaged by other means. It is not necessary, within the framework of that thesis, to suppose that the Homeric poems were written down from dictations produced in various
widely-separated parts of the Greek world and that most of these 
dictations have influenced our tradition: one could envisage instead
(1) only two or three dictations all made by a single group of 
reciters, consisting of a master and a few disciples, which
constituted only a fraction of the reciters then performing in the 
Greek world and which performed a single expanded version of the 
Homerica poems - that known by the master - with only minor variations
between one performance and another. (One could propose a number of 
alternative versions of the same basic hypothesis - e.g. slightly
different dictations by a single reciter.)

Alternatively, (2) one might suppose that the whole of our (and
the Alexandrians') written tradition goes back ultimately to archetypes
present in sixth-century Athens, that in mid-sixth-century Athens there
were two or three written versions which differed from each other by
the presence or absence of substantial post-Homeric passages, that
Peisistratus's Panathenaic version was virtually the same as our own,
that its recitation at the Panathenaea and on other occasions soon made
it familiar to Athenians, and that it acquired such authority that
those who possessed texts which lacked whole episodes contained in that
version would want to have them supplemented by the insertion of those
episodes, which they obtained either by copying them from the
Panathenaic text or from a rhapsode's dictation (the latter method
would account for the presence of orally-generated variants within
passages which were contained only in the Panathenaic text);
similarly, if their own texts contained any conspicuous expansions not
included in the Panathenaic version they may have deleted them as
spurious. This whole process is easier to envisage if we suppose that
the posited two or three substantially different written versions -
probably each represented only by a single copy - belonged to
rhapsodes, and that the main motive for the adjustment of all texts
to bring them roughly into line with the Panathenaic text (i.e. as far
as the presence or absence of whole episodes was concerned) was
provided by the demands imposed by the regulations for recitation at
the Peisistratean Panathenaea (for other likely motives cf. (3)
below). In other words, we are positing for mid-sixth-century Athens
a process of relative standardization of the text in accordance with
a single authoritative version, a process analogous to, though much
more crude and rough-and-ready than, the process which led to the
universal acceptance of the numerus versuum of Aristarchus after
150 B.C.; and we are supposing that in both cases relatively slight divergences going back to different oral versions would be preserved, though in the case of the post-Peisistratean sixth-century Athenian texts the divergences would in many places affect the numerus versuum.  

However, (3) it is possible to suppose that our written tradition goes back ultimately to a single sixth-century MS. and at the same time to suppose that a certain form of oral or semi-oral transmission had some influence on that written tradition - though this would constitute a modification of Pope's theory rather than a simple defence of it. We could build up some such hypothesis as the following (though many of my details could be varied):— The first complete written text of the Homeric poems was produced by the Homeridae of Chios in c. 570 B.C.: it was an expanded version of the poems which was substantially the same as the edition of Aristarchus and our present-day vulgate. In c. 540 B.C. Peisistratus, who had discovered the existence of this text from a Chian rhapsode visiting Athens, commissioned a copy of it: it was in this sense that he πρῶτος ἐκφύλωσεν the Homeric poems to Athens. (A possible variant of this hypothesis would be that Chios possessed no written text of the poems in the mid-sixth century, that Peisistratus was the first to organize their writing down [at any rate in a complete form], and that he did this by getting the Chian visitor to dictate the poems to Athenian scribes.) It was this version of the poems which Peisistratus prescribed for recitation at the Panathenaea and which, consequently, the rhapsodes competing at that festival were required to know. From now on the cultural importance of Athens in the Greek world increased, as did the importance of the Panathenaea; mainland rhapsodes representing traditions which may have been cut off from the Ionian tradition for generations would have flocked to Athens to take part in the Panathenaic rhapsodic competitions and would soon have become familiar with the Panathenaic text, which would have commended itself to them not only through necessity (the "Panathenaic rule") but also because of the authority with which its known Chian and Homerid origin appeared to invest it and because of the dramatic nature of those of its episodes which their own oral versions (or some of them) would have lacked - e.g. the Doloneia, the Nekyia, the encounter between Odysseus
and his father at the end of the *Odyssey*. At the same time it was the Chian-Panathenaic text which was transmitted in writing, and as literacy increased so did the circulation of this text both on the mainland and in Ionia, especially since in the fifth century Athens may well have been the chief centre in the Greek world for the production and export of copies of literary texts. Moreover, the increase of literacy, together with the authority gained by the written (Panathenaic) Homeric text, would presumably also have led to the increasing reliance of apprentice rhapsodes on the written version rather than on the oral versions of their masters: the increase of literacy in Yugoslavia in combination with the publication of songbooks is having a similar effect. However, there is no need to assume that between 540 and (say) 400 B.C. written reproduction of the Homeric poems was always a process in which a scribe simply copied a written exemplar: from time to time a rhapsode who knew the Panathenaic version more or less by heart but who had no text of his own may well have dictated the poems to a scribe or written them out himself from memory, either for use by his disciples, or because in a literate era possession of a complete text of the poems, allegedly the *ipsissima verba* of Homer himself, which interested people would be allowed to consult and (perhaps for a fee) copy, was a status-symbol and perhaps a source of revenue, or, more specifically, because at a stage when complete texts of Homer were still a relative rarity the proprietor of a publishing-house might well commission a rhapsode to dictate or write one. These texts produced from the memories of rhapsodes (and, of course, the descendants of such texts) would have differed from the original sixth-century Chian-Panathenaic version, and from each other, in some of the wording within the lines (e.g. by the substitution of one formula for another) and through the addition, at various points, of single lines and small groups of lines such as those found in the "eccentric" Ptolemaic papyri, cited by the scholia or said by the scholia to have been weakly attested in the pre-Aristarchean tradition: I would assume that most of these additional lines would be due to lapses of memory (this would apply especially to the numerous examples of concordance interpolation), but that some (including some examples of concordance interpolation) would be deliberate attempts to improve the text. (The possibility of accidental omissions of lines will be considered presently.) For the existence of an ancient belief that the rhapsodes polluted the text of Homer, especially by the
addition of spurious lines, see Schol. Pindar Nem. 2. 1c ... oì περὶ 
Κῦναλθον, οὗς φαινεσθαι τῶν ἑπτὸν ποιήσαντας ἐμβαλέσθη εἰς τὴν Ὄμηρου 
ποίησιν, 1e ... oì περὶ Κῦναλθον ὑβδόσθη ... τὴν Ὄμηρου ποίησιν ... 
ἐμφυμάδευσεν καὶ ἀπηγγέλλων: ἐλιμηθάντο δὲ αὐτῇ πάνυ. 100 Of course it 
is only to be expected that the rhapsodes would have made alterations 
and additions to the poems in the process of recitation, but what is 
needed is an explanation of how these oral divergences would have got 
into the written text, and the explanation I have given is, I would 
suggest, more plausible than the notion that the rhapsodes themselves 
made numerous alterations and additions to their own written texts. 
Apart from other considerations, the physical difficulties involved 
in adding lines (especially groups of several lines) to their texts 
would arguably have acted as a disincentive if, as seems probable, 
the text was written with its lines close together and with narrow 
margins. Moreover, we remain with the apparently orally-generated 
intralinear variants to account for: such trivial slips in the course 
of memorization and performance would surely not have led rhapsodes to 
alter their own written versions accordingly.

To summarize: what we have to account for is how oral 
transmission of the Homeric poems could have produced textual 
divergences as wide as, but no wider than, those known from the 
external evidence, and we have put forward three possible hypotheses: 
(1) slightly differing dictations of a single expanded oral version 
performed by a single group of reciters; (2) approximate assimilation 
of widely-differing written versions to the sixth-century Panathenaic 
text; (3) slightly differing dictations (and/or copies written by 
rhapsodes themselves), produced from memory, of the written sixth-
century Panathenaic version.

I have so far presented (3) as an alternative to (1) or (2), but 
it is of course perfectly possible that even if (1) or (2) represents 
the truth, later rhapsodes' dictations or autograph copies produced 
from memory were the source of additional divergences.

Finally, (4) some apparently orally-generated intralinear 
variants, some additional lines and a few omissions of lines may be 
due to something analogous to oral transmission, viz. a situation 
where an ordinary scribe, in the process of copying a text, came 
across a passage he thought he knew by heart, turned away from his 
exemplar and simply wrote out the passage from memory, making some
mistakes. However, this situation would only have occurred when the poems had already become well known, i.e. at a relatively late stage of the tradition, so that omissions produced in this way would not have spread to more than a very small proportion of the Alexandrians' MSS., especially since there would probably have been a tendency for a line omitted in this way to return, through contaminatio, to some of the descendants of the MS. in which it was first omitted. In fact in both pre- and post-Aristarchean times such omissions would have been indistinguishable, in their effect on the tradition, from ordinary scribal accidental omissions stemming from a wandering of the eye rather than of the memory.

I must emphasize that (4) cannot easily explain all the apparently orally-generated intralinear divergences, because copyists' mistakes of this kind would tend to be preserved in only a small proportion of the MSS. (both pre- and post-Aristarchean), whereas in fact the MSS. (pre- and/or post-Aristarchean) are (as we have seen) in many cases more or less evenly divided.

In fine, the hypothesis that oral or quasi-oral transmission produced, mainly through the fallibility of the human memory, many of the divergences in the Homeric MSS. is eminently plausible, and Bolling's outright rejection of this hypothesis is quite unwarranted.

Before proceeding, we should note that if (2) - which is of course only one of several possibilities - happens to represent the truth, some of the intralinear variants in the MSS. (both pre- and post-Aristarchean) and some of the fluctuations in the pre-Aristarchean numerus versuum could go back to "Homer" himself, i.e. to the slight differences between one performance and another by the monumental poet(s).

(ii) The likely effect of oral omissions on Aristarchus's own numerus versuum

We now come to the crucial question of the extent to which lapses of memory would have produced omissions of genuine lines in some of the MSS. used by Aristarchus. I have already argued that omissions produced by process (4) "would not have spread to more than a very small proportion of the Alexandrians' MSS.", and it follows that Aristarchus would certainly not have omitted such lines from his edition; but since processes (1) and (2) would take us back to the
earliest written texts, accidental omissions in dictations in these circumstances (i.e. omissions caused by a lapse of memory of a dictating rhapsode himself as well as those which arose at an earlier stage of transmission and were passed on to him) would have affected a larger proportion of the Alexandrians' MSS.; and the proportion of MSS. affected under hypothesis (3), in so far as it involved the omission of lines present in the Peisistratean text, would tend to be somewhere in between. However, especially if we accept that the chances of the same line's being omitted in more than one dictation (or rhapsode's autograph produced from memory) would be slight, and if we assume that some form of contaminatio would probably have operated even in the pre-Aristarchean period in such a way that accidental omissions would to some extent have tended to return to the text, these lapses of memory would be most unlikely to lead to the absence of genuine lines from the vast majority of Aristarchus's MSS.; and our earlier study of Aristarchus's modus operandi points to the conclusion that where he found a line attested by a substantial minority of his MSS. he would not have omitted it from his text. Further, although we have already conceded that "it is reasonable to suppose that if Aristarchus regarded the internal evidence against a line as exceptionally strong he would have been prepared to exclude it with somewhat less external evidence against it than he would normally have demanded", it is equally reasonable to assume that an accidentally-omitted genuine line would probably never have been of such a character as to lead Aristarchus to judge the internal evidence against it to be exceptionally strong.

If hypothesis (2) happens to be correct, we must grant that lines which "Homer" omitted in most performances but included in some may have been transmitted first in only one branch of the oral tradition and then in a minority of the early MSS., but (a) even these lines would tend to have enough diplomatic support to be included in Aristarchus's edition, and (b) if any of them were not so included, a text which lacked them would have a stronger claim to authenticity than a text which contained them, on the very grounds that "Homer" himself omitted such lines in most of his performances.

Finally, if hypotheses (1) and (2) are wrong, and the truth is represented by something along the lines of hypothesis (3), there may well have existed another class of weakly-attested genuine lines in
the Alexandrians' MSS. - a class, moreover, whose attestation would tend to be substantially weaker, and whose chances of being omitted from the edition of Aristarchus would be correspondingly higher, than in the case of any of the other categories we have so far considered. The first complete written version of the poems posited by hypothesis (3) would surely have omitted - through forgetfulness somewhere along the line of descent - some lines which were present in the version "Homer" sang and which, at the time of this first dictation, were still present in other branches of the oral transmission or, where lines were forgotten by the dictating reciter himself, in the versions of all (or in some cases nearly all) other reciters. Now at the stage when fully oral transmission was still flourishing, and the influence of the Peisistratean written version on the reciters' oral versions just commencing, it is natural to suppose that the assimilation of the reciters' oral versions to the Peisistratean written version would usually have been far from exact, and that the result would have been a general correspondence of episodes and extended passages but the persistence of smaller divergences. Thus when, at this early stage, a reciter representing a different branch of the tradition from that represented by the Peisistratean text dictated his own approximation to the Peisistratean version, most of the lines omitted, through forgetfulness, from the original Peisistratean text would be present in this new dictation. It could be argued that this new dictation would be unlikely to produce a progeny anywhere near as numerous as that of the official sixth-century Panathenaic text, and that Aristarchus may consequently have found some genuine lines so weakly attested that he omitted them from his edition. However, while I accept the general drift of this argument, I would introduce certain important qualifications which would considerably reduce the likelihood of the omission of genuine lines by Aristarchus:—

(a) I have assumed that the dictated approximations to the Panathenaic text which first reintroduced the omitted lines would have been made not long after the initial appearance of that text in Athens. With the spread of written copies, trainee rhapsodes would have been likely to take a written text as the basis of their recitations, and to the extent that they used virtually exact transcriptions of the Panathenaic version the omitted lines would have disappeared from the rhapsodic tradition: thus if the new rhapsodic dictations (or autograph copies) were made only at a later stage, when rhapsodes were
already tied to written versions of the poems, the omitted lines would not have been reintroduced at all. The omitted lines, then, if they were reintroduced at all, would have had an early start in the written tradition, and this, in conjunction with the probable operation of a certain degree of contaminatio, would give them a good chance of appearing in a substantial minority of Aristarchus's MSS. (b) I have said that most of the lines omitted from the original Peisistratean text would be present in a new dictation of the kind we have posited, and this might seem to give Aristarchus a not insubstantial collection of very weakly attested genuine lines; but the other side of the coin is that, if there was more than one new dictation of the Peisistratean text at this early period by rhapsodes from another branch or branches of the tradition - as, on hypothesis (3), there may well have been - then all such dictations would have included nearly all the omitted lines, and this would tend to increase their support in Aristarchus's MSS. very considerably. (c) Even if only one of these early rhapsodes dictated or wrote out his text for a publishing-house, his version would thereby secure a substantial influence on the subsequent written tradition. (d) To the extent that these early rhapsodes produced their texts either (i) for their own disciples or (ii) to hire out on their travels to people who wanted to copy them, the influence on the later written tradition would again be considerable; in the case of (i) the early rhapsodes' versions would soon have obtained, through subsequent copying, very considerable currency among later rhapsodes, and would thus have secured a firm place among commercial texts if, as seems probable, the latter were sometimes descended from rhapsodes' written texts, or dictations from memory, produced in the post-Peisistratean era. (e) If hypotheses (1) and (2) are wrong and (3) is right, then the view that these memorized versions of the early rhapsodes would have a substantial influence on the later transmission receives considerable support from a fact we have mentioned earlier - the large number of apparently orally-generated intralinear divergences where the MSS., pre- and/or post-Aristarchean, are more or less evenly divided. (f) (This argument is also relevant to hypotheses (1) and (2).) It is inherently unlikely that the lines omitted from the Peisistratean version through forgetfulness would be numerous. The strong internal evidence for a considerable amount of expansion during the period between Homer and Peisistratus tends to suggest that the tradition would be highly retentive of the initial Homeric deposit.
Moreover, the weakly-attested lines of the pre-Aristarchean era known to us from the scholia, the Ptolemaic papyri and other sources consist largely of lines repeated (sometimes with slight modification) from other parts of the poems, and where, as often, these lines can be seen as instances of concordance interpolation, or of assimilation to a normal pattern, addition is a more natural explanation than omission if the divergence has arisen in the course of oral transmission: for example, a reproductive singer or reciter who knew II. 18. 389f. τὴν μὲν ἔκτετα καθέτευς ἐπὶ ἄρδουν ἀργυροῆλου, | καλοῦ δαίδαλου ὑπὸ δὲ ἄρηνος ποσῶν ἥνεν, Od. 10. 366f. ἔδεε δὲ ἐλεοαγοῦσα ἐπὶ ὄρνου ἀργυροῆλου, | καλοῦ κ.τ.λ. (= II. 18. 390) and the similar sequence in Od. 1. 130f. would be more likely to add the καλοῦ ... line after Od. 10. 314 (= 366) if it were originally absent than to omit it if it were originally present (the line was found at Od. 10. 315 in "some" pre-Aristarchean MSS.); and a singer or reciter who was accustomed to beginning almost every Homeric speech with a vocative would be more likely to add ὕποε', Ἄθηνας κυνόφοτην, Τριτυγέλω, found after II. 4. 69 in Pap. 41 (Pack 699, saec. iii a.C., cf. West pp. 65 and 68) and II. 13. 255 Ἰδομένεω, Κρητῶν βουλαπφόρος χαλκοχώτων, found in "some" pre-Aristarchean MSS., if the lines were originally absent than to omit them if they were originally present. Furthermore, there is internal evidence, of varying strength, against many of the pre-Aristarchean weakly-attested lines.

Finally, we should remind ourselves (i) that hypothesis (3) is only one of three possibilities and (ii) that Aristarchus would not have omitted lines attested by a substantial minority of his sources.

However, even when all these qualifications have been made, we are left with the possibility that some genuine lines had so little support in Aristarchus's MSS. that he omitted them from his edition; but I would argue that the effect of my qualifications is that, even if hypotheses (1) and (2) are wrong and (3) is right, it is unlikely that there would be more than a very few lines in this category.

The conclusions to be drawn from all that we have so far said about the question of oral transmission are as follows. On the one hand we must reject Bolling's assumption that every passage of which we are told merely that it was absent from "some", or even one, of Aristarchus's sources is - unless the omission stems from a transcriptional error (a fact normally discernible for Bolling from
the passage's being essential to the sense and/or flanked by a homoiographon) - necessarily an interpolation; but on the other hand the hypothesis that some pre-Aristarchean omissions arose through lapses of memory only very slightly increases the chances that any given non-Aristarchean line which we know to be pre-Aristarchean is genuine, and it remains highly unlikely that there is even as much as a single genuine line among the few unidentified non-Aristarchean survivals from pre-Aristarchean times which we have assumed to lurk among the weakly-attested lines of our MSS. and vulgate papyri.

(b) Pre-Aristarchean omission through transcriptional error

We can now move on to consider the effects on the pre-Aristarchean tradition of another kind of accidental omission of lines: omission through simple transcriptional error. Our study of such omissions in the post-Aristarchean tradition in Chapter II should lead us to expect that there would be a number of similar cases in the pre-Aristarchean tradition, i.e. cases where, as a result of such omission, a passage was absent from one or two of Aristarchus's MSS., including some cases, most of them involving a single line, where the homoiographic temptation to omission is very slight or non-existent. Bolling has made some allowance for such cases, though in my opinion not enough; but while this criticism might increase the number of genuine lines included in Aristarchus's edition, it would not increase the number of genuine lines excluded from it, since Aristarchus would not have omitted a line which was absent from only a few of his MSS.

We may next ask whether there may not have been a very few cases where an omission through transcriptional error was transmitted to the majority of Aristarchus's MSS.: supposing that the Peisistratean text was not the first written version of Homer but was itself a copy of another (Chian?) text, may there not have been some lines accidentally omitted from the Peisistratean transcription which were present in only a very small proportion of Aristarchus's MSS.? Conceivably; but (i) it is highly unlikely that the Peisistratean transcription would have contained more than a very few accidental omissions of this type; (ii) the original text from which the Peisistratean text was transcribed may very well itself have spawned an influential progeny; (iii) on hypothesis (2) such omissions would tend strongly to be confined to a minority of the Alexandrians' MSS.; (iv) on hypothesis (3), lines omitted in this way would (like the lines omitted in oral transmission),
for reasons explained above, tend to return (if they returned at all) to a substantial enough minority of Aristarchus's MSS. to make omission from his edition unlikely; and (v) where a line accidentally omitted in transcription was indispensable to the sense and its absence left an obvious logical lacuna in the text, the factors tending towards its return would be strongly reinforced.

(c) The possibility of pre-Aristarchean excision

We must finally consider another possible cause of the omission of genuine lines in Aristarchus's MSS.: deliberate excision — whether by reciters, publishers, scribes, textual critics or schoolmasters — without diplomatic backing. Such possible excisions may be divided into two broad categories: (i) excisions stemming from a characteristically Alexandrian misunderstanding of Homeric style or mentality, certain lines being regarded as (e.g.) ἡλιόθεν μετεννεγμένου, περιττοῦ, εὐταλεύς or (in a relatively trivial sense) ἀπερείπετς, and therefore spurious; (ii) excisions arising out of a reaction of profound shock, or strong disapproval on moral grounds, to the content of a passage: such excisions might be (a) cases of bowdlerizing, the aim being to remove passages which were regarded as liable to be strongly offensive or morally harmful to readers in general and/or as incompatible with the high educational purpose — the inculcation of moral virtue — which the study of the poems in schools was supposed to subserve; or (b) they might be based on a conviction that passages so offensive could not possibly be the work of Homer; or (c) the two considerations might be combined. There is of course a link between (i) and (ii) (b) and (c) in the form of the concept of ἀπερείπετα; the difference consists in the degree of shock felt. We shall now consider each of the two categories in turn.

(i) Excision stemming from characteristically Alexandrian attitudes

Such excisions would be most unlikely to be made before the Hellenistic era. Thus the pre-Hellenistic κατ' ἄνω ἕκτα "editions" would in all probability have been free of them: in fact all that we know of the edition of a certain Euripides (if it really was pre-Hellenistic) is that it contained, in company with some other MSS., additional lines at 11. 2. 848a and 866a; and although the edition of Antimachus is cited in the scholia fairly often, no omission by
him is recorded. 110

We may next consider the πολιτικαί. We do not know enough about
them to be able to date them with any confidence: e.g. after
examining the evidence provided by their known readings Bolling
(EE 39-41) concluded that they were late ("somewhat before 200 B.C."),
Allen 111 that they were early; Allen also argued, adducing interesting
supporting evidence, that "the Egyptian administration recognized age
as a criterion of value in a book". 112 Of course there is no need to
assume that all the πολιτικαί were of even approximately the same age.
However, if we regard the internal evidence as too scanty and
inconclusive to be of much help, and stress the external probabilities
as presented by Allen, remembering also that by the time Aristarchus's
work was complete the main Library in Alexandria had been in existence
for well over a century, and that the texts used by Zenodotus as the
basis for his edition, or at any rate some of them, would probably
have been deposited in the Library and would thus be accessible to
Aristarchus, then it would be reasonable enough to conjecture that at
least half the πολιτικαί would have been at least as early as the
beginning of the third century and most of the remainder not much later
than this. This conclusion is supported to some extent by the fact
that Zenodotus and the Chia agreed in omitting some lines after 11.
17. 132: I have argued earlier 113 that these lines were probably 133-8
and that in this case the omission was in origin a transcriptional
error. If so, the natural conclusion is that Zenodotus based his text
at this point on that of the Chia; one could suppose that he had some
other diplomatic support for the omission as well, but this supposition
is not necessary if one believes (as I do) that Zenodotus was probably
ready to omit lines from his edition with only very slight external
evidence against them and possibly sometimes without any such evidence
at all, 114 nor is the supposition even particularly plausible given
that a transcriptional error of this type would be unlikely to have
spread very far. Thus there is a good specific case for one of the
πολιτικαί being earlier than Zenodotus. When we also bear in mind that
the πολιτικαί came from widely scattered parts of the Greek world, and
from places other than the great cultural centres of that world
(Athens, Alexandria and Pergamum), ranging from Sinope in the far
east to Marseilles in the far west, 115 then the following conclusions
seem warranted: (a) the influence of characteristically Alexandrian
critical principles on the πολλωκαὶ used by Aristarchus would have been minimal, and thus these texts would have contained no, or at the most very few, excisions in category (i); (b) the chances that the same excision would be common to two or more of these texts would be extremely slight. Moreover, it seems reasonable to suppose that these texts - or at any rate the vast majority of them - would not have been "editions" or "recensions" - ἐκδόσεις or διορθώσεις - in anything resembling the sense in which these terms were applied to the texts laboriously prepared by the great Alexandrians.

What concrete evidence do we have for excision by πολλωκαὶ? In none of the few cases where omission by any of these texts is explicitly attested do we need to posit excision as the cause of the omission. We have already discussed the omission of II. 17. 133ff. by the Chia (and Zenodotus): even if my preferred explanation - that the lines were 133-8 and that their omission originated as a transcriptional error - is rejected, other interpretations of the evidence are possible without introducing the hypothesis of excision. The absurd zoological argument tentatively mentioned in Schol. A as a possible motive for an excision cannot be taken seriously: no one could have been led to hack at the text on the basis of such a consideration (though I suppose it is not completely beyond the bounds of possibility that Zenodotus was encouraged by it to follow the already-existing omission in the Chia). We have also already discussed the omission of II. 18. 39-49 by the Argolikē: if this is not merely a transcriptional blunder stemming from the pronounced homoioiteleuton, it is most naturally interpreted as evidence that the passage is an interpolation. The omission of Od. 1. 97-101 by the Massaliōtikē and perhaps some other pre-Aristarchean MSS. will be briefly discussed in Chapter VI; here we may simply comment that if the omission is not evidence of interpolation, it is best explained as the result of a reciter's lapse of memory caused by the fact that ἔδρασα τοῦ καλὰ πέδιλα (= ending of 96) is immediately followed by βῆ (= first word of 102) at Od. 2. 4f. and 4. 309f. Finally, how are we to interpret the omission of II. 21. 290-92 by the Krētikē attested by the Ammonius commentary? Of course if the Krētikē was in fact unknown to Aristarchus, the question need hardly concern us here, but we cannot be sure of this. Bolling (EE 189f., ALI 171f.), predictably, saw the omission as evidence of interpolation, an opinion which had already
been advanced more tentatively by Wecklein (59 n. 1). However, not only do 291f. have no connection with the motive for the interpolation posited by Bolling, but their omission would be awkward: Achilles, battling against the turbulent River Scamander, is in danger of being overwhelmed, and, fearing death, prays to Zeus for deliverance (272-83); it would be surprising if the two divinities who appear in answer to this prayer were to tell him not to stop fighting until he had penned up the Trojans inside their citadel and killed Hector (293-7) without first explicitly predicting the outcome of his immediate plight. Further, the objections of Wecklein and Bolling to the language of 290-92 carry little weight: "das hier nichtssagende οὐ δὲ εἰσέκαψαν ἀυτοῖς" (Wecklein, cf. Bolling), meaning "as you yourself will discover" (cf. Od. 2. 40), actually adds an extra note of reassurance to the prediction, and it is hardly correct to speak of "the unusual meaning here given to λαύσην" (Bolling, cf. Wecklein). However, excision is at least equally unlikely to be the explanation of the omission: while one could see a motive for the excision of 290 alone (athetized by Aristarchus: see Schol. A and "Ammonius" ad loc.), there would be no plausible motive for the excision of the whole group 290-92. Admittedly accidental omission of 290-92 - whether by a scribe or a reciter - becomes plausible only faute de mieux. There is no homoiographic temptation, and the passage is a long one to be accidentally omitted by a scribe without such temptation. I cannot help wondering, therefore, whether 290-92 represents the full extent of the omission in the Krētikē. The whole of the long discussion on the authenticity of 290ff. in the Ammonius commentary focuses exclusively on the internal evidence until the very end, where we are told that Seleucus athetized 290-92 ώς περισσότερος and reported that they were absent from the Krētikē. Could it be that the Krētikē actually omitted the four lines 290-93, but that Seleucus followed it only to the extent of athetizing the first three, feeling the fourth to be necessary to the context? I make this tentative suggestion because we can then plausibly put the omission down to a scribal blunder under the influence of the homoiomeson 289 ΤΑΠΤΟΙ, 293 ΑΥΤΑΠΤΟΙ.124 It could be objected that in this case the statement in the Ammonius commentary about the omission is rather misleading, but one can cite a precise parallel from modern times: see my discussion in Acta Classica 17 (1974) 22 n. 38 ad fin. To conclude: there is no concrete evidence for excision by any of the Πολιτικα'.
There is no explicit reference in the scholia to any omission by the ἐλκοτέρεια (or κοντότερεια etc.) as such, although, as we have seen, it is reasonable to suppose that some MSS. of this class would be included in some of the references of the type ἦν τοις οὐκ ἐφεξοντο. But to what extent would the ἐλκοτέρεια have contained omissions due to excision in category (i)? It is not impossible that the influence of Alexandrian critical attitudes led to the occasional excision, but the chances that more than one of the ἐλκοτέρεια consulted by Aristarchus would have contained the same excision would be extremely slight. West's list (13f.) of omissions in the "eccentric" Ptolemaic papyri - perhaps on the whole the equivalent of Aristarchus's ἐλκοτέρεια - includes 14 cases where the papyrus concerned is the only source known to us which omits the passage. I do not regard excision as a likely cause of any of these omissions. In most of these cases West herself, explicitly or implicitly, rejects excision as unlikely, but her discussions are constantly vitiated by an underestimation or ignoring of the chances that an omission is purely accidental: she seems unaware how frequently a single line is omitted in the later tradition without any homoiographic temptation, and has sometimes ignored the presence of a homoiographon. In the latter category are her treatments of Il. 12. 403, Od. 10. 69f., 128 75, 129 17. 364.

Finally, what of the pre-Aristarchean Alexandrian καὶ ἀνόρα editions? Could it not be (one might argue) that Zenodotus sometimes excised a line on the basis of the internal evidence alone, or followed an earlier excision, or followed an accidental omission which he found in one, or a minority, of his sources, and that some or all of the later καὶ ἀνόρα editions followed Zenodotus, so that a deceptive momentum was produced which misled Aristarchus into omitting the line from his edition? In a sense it is hardly worth taking the trouble to refute such a Domino Theory, since even if the premise - the pre-Aristarchean momentum - is granted, the conclusion - omission by Aristarchus - will not follow, given the weight which Aristarchus would have assigned to his remaining sources, the majority of which would have contained such lines. But my position is greatly strengthened by the well-documented reluctance not only of Aristarchus but also of his predecessor Aristophanes to follow the omissions of Zenodotus. Aristarchus had a special sign, the διπλή περιετελειμένη, to indicate disagreement with the text of Zenodotus, and he often used it in
connection with the numerus versuum. For example, he applied the sign to II. 1. 219, 131 4. 89, 132 16. 432-58, 133 21. 195, 134 all of which Zenodotus had omitted but which Aristarchus regarded as authentic, and for similar examples see e.g. Bolling, EE 48-51. There are also some passages which Zenodotus omitted but which Aristarchus included in his text, merely athetizing a portion of the passage, e.g. II. 2. 60-70 om. Zen., 64 ath. Ar.; 135 II. 404-25 om. Zen., 420 or 420-23 ath. Ar. 136 Passages omitted by Zenodotus and merely athetized by both Aristophanes and Aristarchus are legion: Bolling, ALI 39 (1), lists 18 from the Iliad alone, to which one should add another, II. 9. 23-5, inadvertently misplaced by Bolling in his list (2) (ibid. - but cf. ALI 115, EE 117f., II. Ath. ad loc.). In some of these cases the precise situation is doubtful because of corruption in the scholia, and one example should certainly be removed from his list, 137 but the general picture is clear enough. What needs stressing here is the relative conservatism of Aristophanes when compared with Zenodotus: against these 18 or so Iliadic passages which Zenodotus omitted but Aristophanes merely athetized one may set only two, II. 10. 497 and 15. 33, where the scholia tell us that Aristophanes followed Zenodotus’s omission, 138 and none in the Odyssey (with the quasi-exception of Od. 8. 142, to be discussed presently). This does not augur well for the Domino Theory under consideration. Aristophanes is the only source cited for the omission of II. 18. 597f., athetized by Aristarchus: this omission may well have originated as a copyist’s slip, perhaps merely in the copy of Aristophanes’s edition used by Aristarchus. 139 What about the edition of Rhianus? Of the 45 readings which the scholia ascribe to him, 140 only three affect the numerus versuum: he omitted II. 19. 10f., Od. 1. 279 (or 278) and 4. 158-60. The reasons for these omissions need not detain us here: 141 we may simply note that as far as we know none of these passages had been omitted by Zenodotus - we know for certain that he read Od. 4. 158-60, because the scholia cite a reading of his in line 159 - and only one, II. 19. 10f., was omitted anywhere else (by Aristophanes). Our evidence for the edition of Rhianus is admittedly fairly limited, but as far as it goes it too tends to tell against the Domino Theory. About the editions of Sosigenes and Philemon very little is known: only five readings of Sosigenes are cited in the scholia and only two of Philemon, none of them affecting the numerus versuum. 142 The scholia cite two intralinear readings of Philetas, but there is no clear
evidence that he actually prepared an edition. The tradition that Aratus produced an edition of Homer may well be correct, but if so we know nothing whatsoever about its contents; the scholia never mention it, which strongly suggests that it was not used by Aristarchus. Compatible with this supposition is the fact that Aratus never lived in Alexandria; his edition of Homer is supposed to have been produced at the court of Antiochus I of Syria. (See Pfeiffer 120-22.) To return to Aristarchus: it is worth stressing that we know of a number of cases where Zenodotus's omission was supported by one or more other texts but where Aristarchus still retained the passage in his edition, in most cases not even athetizing it. He did athetize Il. 10. 497, omitted by both Zenodotus and Aristophanes, but there is no record of his having athetized Il. 15. 33, omitted by the same two scholars. He clearly defended Il. 21. 195, omitted by Zenodotus and others; and from the A scholium on Il. 17. 133ff., omitted by Zenodotus and the Chia but retained by Aristarchus, it seems likely that he actively defended this passage too. He certainly defended Il. 4. 89, omitted by Zenodotus; was it omitted by some of his other sources too? We cannot be sure, but it seems fairly likely that it was, since it is also omitted by Pap. 41 (Pack 699, saec. iii a.C.) and since on this occasion Aristonicus (Schol. A) uses the neutral term οὐδὲ γράφει of Zenodotus's omission instead of describing it (as he so often does) as a simple excision (συνετημένει, ἢρκεν, etc.: see Bolling, EE 48-51) and cites Zenodotus's wording at the end of the preceding line (which accompanied the omission of 89) with the neutral introduction τὸ ἀντραλεύτουν οὕτως γράφει, not e.g. μεταγράφει as at Il. 1. 219ff. (cf. μετέγραφε at Od. 3. 230) or μετετέθει as at Il. 3. 423-6. Next, Schol. A on Il. 7. 256f. (or 255-7?) reports: τοὺς στόχους τούτους οὔ προσένειν ἐννοεῖ, ὡσπερ οὔδὲ ζηνόδοτος, ἀλλὰ τὸ τῆς συνέπεσας οὕτως ἔχει παρ' αὐτῷ ... (see Bolling, EE 91ff.). Now admittedly οὗ προσένειν is not quite as explicit as οὔ γράφουσα, but the reference to the συνέπεσα (cf. Schol. A [Aristonicus] on Il. 3. 423, 9. 23, 16. 89) shows clearly that the lines were completely absent at any rate from the text of Zenodotus, and this and the scholium's ὡσπερ make it almost certain that the lines were also completely absent from the texts of the others who refused to "let them in". However, there is no reference to an athetesis by Aristarchus, and from
Schol. A's mention of Aristonicus in connection with the Zenodotean οὐνέπεια (immediately after the section quoted above) it would seem that Aristarchus signalled his disagreement with Zenodotus's omission by applying διαλέγειν τερματικά to the passage. Similarly, although, as we have seen, Rhianus and Aristophanes agreed in omitting ll. 18. 10f., it is noteworthy that Aristarchus, while aware of possible grounds for objection against the couplet, retained it in his text, merely marking line 10 with a διαλέγει, reporting the evidence of his MSS., mentioning the difficulty and putting forward his own defence of the couplet. 150

To some extent my argument against the Domino Theory has been one ex silentio, but the existing evidence is more than enough to refute it. Above all, the independent and critical attitude of Aristarchus towards the earlier Hellenistic editors of Homer stands out, as does, once again, his conservatism.

Of course it goes without saying that omission by Zenodotus was not enough to entitle a line to a place in Aristarchus's edition. It stands to reason that most of the lines which Aristarchus omitted from his edition because they were present in only a small minority of his sources would also have been absent from the edition of Zenodotus. It should therefore come as no surprise to us that Didymus, probably in the circumstances which I have described in Chapter I, should report of Od. 8. 142 οὔτε Ἀριστοφάνης οὔτε Ζενόδωτος ἐκτιθέντα τῷ τοῦ ὄνομα στίχῳ: we do not even know that this line was of pre-Aristarchean origin. 151

(ii) Bowdlerizing and similar excisions

Bolling's argument against the general supposition that the Homeric poems have in places been bowdlerized is that "the supposed bowdlerizing shows its effects too sporadically to permit us to regard it as a vera causa" and that "we have no right to assume that interpolators were always proper, pure-minded people; yet without that assumption we cannot argue that a line is genuine because 'shocking'" (EE 54f.). One must accept Bolling's second point, but of course this in itself merely asserts that the "shocking" character of a line does not guarantee its authenticity: equally, it does not preclude its authenticity either, and is of course entirely compatible with the hypothesis of bowdlerizing. However, Pasquali goes too far
in claiming that "in opere classiche è sempre genuino quello che dà scandalo, quello, cioè, che offende il sentimento morale progredito o communque mutato dei posteri".\(^{152}\) this simplistic view, rightly criticized by Bolling (AL I 26f.), overlooks two facts: (a) some interpolations would have been made soon after the original composition of the Homeric poems, and (b) within any single given era opinions on the bounds of propriety to be observed in a work of literature will differ, so that (for example) a fifth-century B.C. interpolator may have inserted a passage in Homer which would have offended some (but not others) of his contemporaries. On the other hand Bolling's first point - the sporadic occurrence of the supposed bowdlerizing - is extremely weak, and Pasquali's reply is fully justified: "Che l'epurazione del testo omerico non sia conseguente e totale, è naturale: l'esperienza di oggigiorno c'insegna che edizioni spurgate in uso della studiosa gioventù eliminano passi relativamente innocenti e ne mantengono i quelli altrettanto scandalosi":\(^{153}\) one may add that modern government-backed systems of censorship of the arts tend to have rather similar results. Moreover, many Homeric passages would have been saved from excision by the readiness to offer allegorical interpretations;\(^{154}\) and there is no reason to suppose that our scholia record more than a fraction of the expurgations which were actually made over the centuries: the majority would probably have had such a slight effect on the manuscript tradition that Aristarchus's elaborate collations would have failed to reveal any trace of them. However, the thesis that some pre-Aristarchean omissions reflect excision stemming from expurgatory motives is in no way dependent on the assumption that some person or persons systematically went through the whole of Homer with the aim of deleting everything offensive: on the contrary, it is much more likely, I think, that the process was a much more haphazard and impulsive one in which a rhapsode while dictating, a scribe while copying or a schoolmaster while preparing his lesson occasionally felt moved to omit (or, in the last case, delete) a passage which he found particularly strongly objectionable. The view that the Homeric text has been "bowdlerized" in this or that place surely need not imply a belief that a systematically censored edition of Homer, like Bowdler's Shakespeare or Duff's Juvenal, was ever produced: if we think that the verb "bowdlerize" necessarily implies such a systematic
undertaking we shall have to choose another term, though my own opinion
is that the word is correctly used to describe individual cases of
excision within the haphazard process I have envisaged. However, what
is at stake is not mere terminology but logic: my point is that
Bolling's refusal to contemplate individual cases of expurgatory
excision except as part of a planned, elaborate, radical and systematic
censorship of the Homeric corpus as a whole is arbitrary. An
incentive for such occasional excisions would certainly have been
present. As early as the sixth century B.C. Xenophanes, according
to Diogenes Laertius, "wrote ... against Hesiod and Homer, reproving
them for what they said about the gods", and Sextus Empiricus quotes
his lines πάντα θεοίς ἀνεύθηκαν "Οὐμηρὸς ἡ Ἡσείδος τε | ὅσα παρ' ἀνθρώπους ὑπνόδεα καὶ φῶς ἑστών, | κλέετον μουχεέεν τε καὶ ἀλλήλους ἀπατεύεν. We have already referred to allegorical
interpretations of Homer, and it seems probable that the allegorizing
by Theagenes of Rhegium in the late sixth century was intended largely
as a defence of the poet against charges such as those levelled by
Xenophanes. In the fourth century the 'Ομηρομάτες Zollus attacked
Homer's representation of the gods (as we shall presently see), and in
Plato's Republic (376C-392C) we have an elaborate criticism of Homer's
representation of both gods and heroes. The tendency to idealize the
heroes as moral exemplars could certainly have led to the excision of
some passages in which they failed to live up to expectations; and as
evidence of such an idealizing tendency, and of the general moral
purpose which the study of the poems by the young was regarded as
serving, one may cite two passages: Plato Protagoras 325E-326A, where
Protagoras says that teachers place before their pupils ἄγαθων
ποιματα καὶ ἐκμαθήνειν ἀναγκάζουσιν, ἐν ὁις πολλαί μὲν νουθετήσεις ἔνειον, πολλαὶ δὲ διεξόδοι καὶ ἑκατον καὶ ἐγκάμπα καλαμάν ἄνδρῶν ἄγαθῶν, ὡν ὁ παῖς θηλῶν μιμήται καὶ ὀρθώτατοι οἰκονόμοι γενέσθαι; and
Xenophon Symposium 3. 5, where Niceratus says: ὁ πατήρ, ἐπιμελοῦμενος δικαίως ἄνηρ ἄγαθες γενοῦση, ἰνάγκασε με πάντα τὰ Οὐμήρου ἐκη μαθεῖν. We must not lose sight of the other possible motive for excisions
in category (ii) - the conviction that the offensive nature of such
passages was incompatible with Homeric authorship. It is possible that
such a conviction sometimes led Zenodotus to excise a passage against
which there was no diplomatic evidence.

We must now move on to consider some possible examples of
excision in category (ii). Schol. A tells us that II. 15. 18-31 was omitted by Zenodotus: Ζηνόδωτος οὐδὲ ἐλώς τὴν κόλασιν τῆς Ἡρας γράφει. The theology of the passage would certainly have been regarded by some as seriously offensive, and we must consider the possibility that Zenodotus was following an earlier excision. In this passage Zeus describes his barbaric treatment of Hera and some other gods, and it is not difficult to show that such theomachies caused concern well before the time of Zenodotus. From the context in which Schol. B on II. 20. 67 mentions Theagenes of Rhegium it seems likely that the latter’s allegorizing of Homer was partly designed to counter theological objections to the theomachies. Pindar in Ol. 9. 29-41, after referring to stories of battles between Heracles and three gods, continues ... μὴ νῦν λαλάγει τὰ τοιαῦτα: ἔα πόλεμον μάχαν τε πᾶσον ἃ χωρὶς ἀθανάτων - roughly, "Say nothing of any stories which involve the gods in war or battle". Plato in the Republic makes Socrates say (378B-D): οὐδὲ τὸ παράταν [λεκτέον ἐν τῇ ἡμετέρᾳ πόλει] ὡς θεοὶ θεοῖς πολεμοῦσι τε καὶ ἐπιβουλεύονται καὶ μάχονται. οὐδὲ γὰρ ἀλήθη ... "Ἡρας δὲ δέσσιν ὑπὸ ὤς καὶ Ἡραδοῦρ μῆψες ὑπὸ πατρὸς, μέλλοντος τῇ μητρὶ τυπτομένη ἀμνεῖν, καὶ θεομαχίας οὕσα "Ομηρος πεποιήκας οὐ παραδεκτέον εἰς τὴν πόλιν, σύτ' ἐν ὑπονοοῖς ἐπεισεμένας [i.e. composed as allegories] οὔτε ἀνευ ὑπονοοῦν. ὁ γὰρ νῦς σὺχ σὺχ τε κρύνειν διτ τε ὑπόνοια καὶ ὁ μὴ ... 161 What is of interest here is not merely the fact that Plato criticizes such theomachies but the implication that allegorical interpretations of them were not uncommon in his day and his judgment that the existence of such interpretations did not free the passages concerned from the danger of being morally harmful. Finally, two late examples of elaborate allegorizing of the passage II. 15. 18-31 itself which may well have had much earlier forerunners may be found in Schol. B (Dindorf) ad loc., one of them from the Homeric allegorist Heracleitus of the first century p.C., and there is similar material in Eustathius ad loc. Murray, Von der Mühll and Van der Valk all regard the passage as a genuine one excised from expurgatory motives. However, we cannot overlook the fact that there is some weighty internal evidence against it. The late and unusual linguistic features are discussed by Leaf ad loc., Bolling (EE 151) and G.P. Shipp, who declares that "the language contrasts so violently with that of the context that it [sc. 18-31] cannot have been composed by the same poet", while Bolling (loc. cit.) comments, "The linguistic oddities of the passage ... cannot be pressed too hard, as they can be paralleled from
late but genuine sections of the poem. Still, the accumulation is remarkable ... " It certainly is, and it tells strongly against the authenticity of the passage. What of the charges that the transition from 31, τῶν ο' αὖτις μνῆσω, ἃν' ἀπολλήξεις ἀπάτῶν, to 32, ὁφα ὑν τοι χραίσμη φιλότης τε καὶ εὐνή, is impossible? Hentze's objection 164 - "... die Erinnerung an die frühere, unter anderen Verhältnissen verhängte Strafe kann doch unmöglich der Here die Erkenntnis verschaffen, dass die jetzt vorhergegangene Liebesscene sie nicht vor Misshandlung schützen werde" - would arguably be pedantic even if the tacit premise on which it is based - that τῶν ο' αὖτις μνῆσω = τῶν σε νῦν μμηνήσκω - were correct, the more so if 11. 15. 33, omitted by Zenodotus and Aristophanes, is an interpolation, as Bolling assumes, since without this line the reference in 32 could be to love-making in general (including future love-making), not just "die jetzt vorhergegangene Liebesscene". It is therefore surprising that Bolling thought Hentze had "[shown] the sequence 31-2 [to be] impossible". Moreover, Bolling was surely wrong to judge that "Finslner's attempt ... to make τῶν σ' αὖτις μνῆσω = 'I will repeat your punishment' is ingenious rather than convincing": it seems to me to be the natural meaning in the context (cf. αὖτε in line 16 in similar surroundings) and it is also, I notice, the interpretations offered by Van Leeuwen ad loc. However, the transition from 31 to 32 is extremely awkward - though for a reason which neither Hentze nor Bolling has mentioned: the succession of two final clauses, both dependent on μμηνήσω, without any copulative particle to link them involves a very harsh asyndeton for which I know of no adequate parallel. Conversely, a sequence in which 32 immediately follows 17 gives perfect sense - we may note inter alia how the idea of "benefit" in an ironic sense introduced in ἐκαύρνας (17) is immediately continued in χραίσμη (32). Thus Leaf seems justified in claiming that 31 "has all the appearance of an awkward transition from an addition to the original text". In all probability, then, the passage is an interpolation. In that case it is natural to suppose that Zenodotus knew a MS. or MSS. representing a branch of the tradition to which the interpolation had not yet spread; but on the other hand it is possible (though Bolling does not allow for this) that the interpolation was already present in the official sixth-century Panathenaic text, that Zenodotus knew of no MS. which lacked it, but that on the basis of the internal evidence alone he correctly judged it to be an interpolation and excised it: he would
then probably have been motivated not only by the argument of theological δικαιοσύνη but also by some of the contextual and linguistic considerations which we have discussed. An alternative but similar hypothesis would be that Zenodotus knew a MS. without the passage but one which at this point reflected diplomatically unsupported excision rather than a branch of the tradition which had never been contaminated by the passage. Nevertheless, in spite of these uncertainties, Bolling's position on this passage has turned out to be, though overstated, basically justified: the passage, though "shocking", is in all probability a post-Homeric interpolation, and Zenodotus probably knew a MS. or MSS. representing a branch of the tradition to which the interpolation had not yet spread.

Let us next consider the famous passage Od. 8. 333-42 within Demodocus's Lay of Ares and Aphrodite. We have already discussed the scholium which refers to the absence of the passage from some MSS. That this absence is due to excision, and that the passage is in fact genuine, is assumed, with little discussion, by Murray and Van der Valk. Of course the song of Demodocus as a whole, with its theme of divine adultery, would have been a target for attacks from an early period. One thinks immediately of μουχέσων in the excerpt from Xenophanes already quoted; Plato explicitly criticizes the story at Rep. 390C; Schol. Ar. Pax 788 refers to τόσος άδεστούντας τήν ἐν 'Óνοισσεία "Ἀρεώς καὶ Ἀφροδίτης μουχέταν; the two allegorical interpretations offered by Schol. E on Od. 8. 267 (one of them again from Heracleitus) may well have had much earlier counterparts reflecting an embarrassment at the content of the song; and the defence of the song in Schol. ἩQT is, quite explicitly, a reply to an attack (κατηγορία, ἔγκλημα). As for the omitted passage itself, 333-42, there is an explicit attack on it within the criticism by Zoilus (whom we have already mentioned) of lines 326-42 as recorded by Schol. Τ: ἐκτιμή δὲ αὐτούς ὁ Ζωίλος, ἀτόκον, αὐτοῖν, μὲν ἔγινε, μέν ἀκολούθως τοὺς θεοὺς ἐπὶ τοὺς τολμώτους, τὸν ἐκ ἔμμην εὐχεσθαι ἐναντίον τοῦ πατρὸς καὶ τῶν ἄλλων ἠθῶν ὀρθῶν δεδέσθαι σὺν τῇ Ἀφροδίτῃ. There can be no doubt that the content of the passage 333-42 could have been regarded as offensive enough to warrant its excision. One of the various ways in which this might have happened is the deliberate omission of the passage by a rhapsode in the course of dictation from memory, and in this connection it is interesting to
compare a recent parallel. Maartje Draak has described how she attended a Mod (annual festival of Gaelic culture) in Oban in 1953 at which the story-teller Duncan Macdonald, in the course of telling a traditional (prose) tale, "passed over the motif of the hero sleeping with the sleeping princess, and the princess seeking the hero because he was the father of her child. ... Afterwards ... I said to Duncan that I thought the story was like this and mentioned the motif. Did he not have [i.e. know] the story like that? Duncan replied in the affirmative, but added that he could not tell such a thing before an audience at the 'Mod'." Thus the content of the section omitted by Macdonald had something in common with that of the passage Od. 8. 333-42. However, Bolling (EE 239) regards this passage as an interpolation and trundles out his stock arguments against bowdlerizing: "An expurgator would certainly have cut deeper. We have no right to assume that interpolators were always clean-minded people. Why should not some Athenian wit have sought to render the scandal still more spicy?" But the admission contained in Bolling's "still more spicy" provides us with an answer to his argument that "an expurgator would certainly have cut deeper": someone may well have found 333-42 substantially more offensive than the rest of the tale and would thus have had adequate motivation for excising only these lines: after all, the story as a whole has a moral, οὐκ ἄρετᾶ κακὰ ἔργα (329): the embarrassment of the trapped lovers at becoming a spectacle and a laughing-stock for the gods constitutes their punishment (or rather part of it: there is also the μουχάρια, 332, 347-56); but the enthusiasm with which Hermes contemplates the possibility of committing a similar act of adultery himself, and the gay nonchalance with which he allows for similar but intensified attendant circumstances - note especially 341 ὑμεῖς δ' εἰσορῶμε γε θεοὶ πᾶσαι τε θέαναι as against 342 ἐφιλήτεραι δὲ θεαὶ μένον αἴδοι οὐχοὶ ἐκδοτὴ - is gratuitous and could easily have been regarded as outrageously de trop. Bolling's attempt to show that the lines were in all probability absent from Plato's text of the Odyssey is a blatant piece of special pleading and fails dismally: while there is no positive evidence that Plato knew the lines, there is not a shred of evidence that he did not - and of course even if Bolling were right the evidence could be interpreted simply as indicating a terminus ante quem for an excision. Moreover, there is in reality no internal evidence against the passage: the playful exchange between Apollo and
Hermes perfectly suits the mood of the scene. In fact we can go further than this, for while the ten lines which the scholia say were omitted, 333-42, could well have been excised by an expurgator, their removal produces an impossible sequence, with the ὄφε δὲ τις εἰπεσκεῖν...

speech of 328-32 then immediately followed by 343 ὅς ἔφατ', ἐν δὲ γέλως ἄρτ' ἀδανάτωσι θεοῦσιν: everywhere else in Homer, as we should expect and as logic requires, the concluding formula after a speech introduced by...

τις εἰπεσκεῖν(ν)...., ἄλλος... εἰπεσκεῖν... or τις...

αὐδήσασκεν is either another εἰπεσκεῖν(ν) formula, viz. ὅς ἄρα τις εἰπεσκεῖν(ν), or a non-frequentative plural formula, e.g. ὅς ἄρ' ἔφαν, but never a non-frequentative singular formula. Moreover, even if we were to emend ὅς ἔφατ' in 343 to ὅς φᾶσαν, the remainder of the line, with γέλως ἄρτ', would remain odd in the resulting context in the light of 326 ἀσβεστος ἄ' ἄρ' ἐνυρτο γέλως μακάρεσσο θεοῦσι immediately before the τις εἰπεσκεῖν speech, which presumably represents the kind of comment with which their prolonged (ἀσβεστος) laughter was interspersed.

Bolling (EE 209, 239f.) is aware of these difficulties, and tries to evade them by proposing two alternative hypotheses, for one of which he expresses a preference. Before proceeding to consider these hypotheses, however, we must stress that prima facie the evidence of the scholia supports the view that the passage is a genuine one whose omission is due to excision: we do not need to cast around for ingenious "solutions" to a "difficulty" unless we insist, on inadequate a priori grounds, that the passage must be an interpolation. To proceed: the hypothesis which Bolling regards as second best is as follows (EE 209): "Verses 333-43 were the missing lines, one of them having been interpolated after the time of this scholium. [The scholium refers to οὗ δέκα στίχοι.] For the absence of a formula [after 332] to summarize the speech σ 400-4 can be compared. The interpolated line would probably be verse 335 - a needless vocative; it is omitted by [Ludwich's] G, though this may well be nothing more than haplography" (there is the homoiarchon 334 ἔρμην, 335 ἐρμεῖα and the homoioteleuton 334 'Ἀπόλλων, 335 ἔδων). Various considerations combine to render this hypothesis highly unlikely. The most serious objection is probably the absence of the concluding speech-formula mentioned by Bolling: some such formula is virtually universal in Homer where the quotation is not immediately followed by an answering-formula, and I know of no precise parallel to the formula-omission
which Bolling’s theory posits: Od. 18. 400-404, cited by Bolling, will not do, as the formula τούτο δὲ καὶ μετέευφ’ (405), which immediately follows the direct speech, is, in the circumstances, equivalent to an answering-formula, and is again used immediately after direct speech at 11. 14. 109 and Od. 11. 342. Secondly, οὗ δὲ Ποσειδόνα γέλως ἔχε (344) is rather awkward coming so long after ἐνῶρτο γέλως (326) without any renewed reference to laughter in the immediately preceding line: this is a matter to which we shall presently return. Thirdly, the internal evidence tends to favour the retention of 335, Bolling’s "needless vocative", since Hermes likewise uses a formal vocative in his reply (339) and since the line is by no means a διψοροδιμενος. Fourthly, as Bolling himself admits, the external evidence against this line is weak: Bolling could have strengthened his case somewhat by pointing out that Ludwich’s G (= Allen’s L4) is the earliest miniscule MS. of the Odyssey which we possess – and there is still no published papyrus covering the passage; but the lack of support for the reading of L4 combined with the ease with which the line, through the operation of either or both wings of the double homoiographon, could have been accidentally omitted favours the thesis that it is not later than its context. Finally, there is no other line within the passage 333-43 which is at all likely to be an interpolation.

The hypothesis which Bolling prefers (EE 239) is "to push the scholiuim down one line ... and make lines 334-43 the verses in question". This avoids most of the difficulties inherent in his other hypothesis, but actually intensifies one of them: the formula which concludes the τοῦς εὔπροέκειν speech, 333 ὃς οἷς μὲν τολµάτα πρὸς ἀλλήλους ἀγορέων, by emphasizing the actual conversation without making renewed reference to the laughter, takes us even further away from the thought of 326, and the sequence 333 ὃς οἷς μὲν ... ἀγορέων | 344 οὗ δὲ Ποσειδόνα γέλως ἔχε is distinctly odd: a negative line qualifying or standing in contrast to an earlier more general positive statement would normally follow that earlier statement immediately, and this is in fact the transition which we find in the vulgate; and if an expurgator chose the limits of his excision so as to leave this sequence intact (as it is left intact if the omitted passage was, as the scholia say, 333-42) his instinct would to this extent have been sound. Bolling claims (EE 240) that in the vulgate line 343, after we have already had the similar 326, "is very awkward", but this objection
cannot be sustained: as we have seen, the speech introduced by ὅσε ὅ
τις εἶπεσκεν ... (328) presumably represents the typical remarks which
accompany the gods' laughter; the formula (333) which concludes this
speech corresponds closely to the one which introduces it: the
generalizing force of the opening τις εἶπεσκεν ὅδων ἐς πλησίου άλλον
is well taken up by the closing τοιαύτα πρὸς ἄλληλους ἄγρευον; the
generalizing section being now neatly rounded off, we pass on to a
particularized conversation consisting of a particular question
addressed by a particular god, Apollo, to another, Hermes, and the
latter's reply; and the humour of that reply gives rise to a new bout
of laughter. There is nothing at all awkward about this.

The objections to Bolling's preferred hypothesis are admittedly
not so overwhelming as to render it completely impossible, but are
weighty enough to render it unlikely. The view that the passage is
an excision is much more attractive, and Bolling's emphatic rejec-
tion of this view is unjustified.177

Thus 11. 15. 18-31 has considerable linguistic evidence against it,
and its presence leads to a seriously awkward sequence which disappears
once the passage is removed; whereas Od. 8. 333-62 has no linguistic
or other internal evidence against it, and its removal leads to a
seriously awkward sequence, and even if we attempt to redefine the
limits of the omitted passage there remain serious objections to its
removal. Our study of the former passage has disproved Pasquali's view
that the "shocking" nature of a passage guarantees its authenticity
and has to this limited extent vindicated the general position of
Bolling; but conversely our study of the latter passage has shown that
the facts do not justify Bolling's confidence that no pre-Aristarchean
omissions of "shocking" passages are due to expurgation.

We are now brought face to face with the question whether
Aristarchus omitted any genuine passages against which there was
misleading manuscript evidence reflecting not interpolation but
excision in category (ii). In this connection there is, in my opinion,
only one passage whose claims to authenticity deserve to be taken
seriously, 11. 9. 458-61.178 Phoenix, having explained how his mother
entreated him to have intercourse with his father's concubine so that
the latter should ἐχθρέει γέροντα, continues (in the Wolfian vulgate):

177
178
Lines 458-61 are absent from all our MSS. and were clearly also absent from the text on which the glossary of Pap. Pack 1189 (saec. i-ii p.C.) was based; obviously, then, they were absent from the edition of Aristarchus. They are known only from Plutarch, who says at Moralia 26F: καὶ μὴν ὁ φοίνιξ ὁδὰ τὴν πολλαπλάσια κατάρατος ὑπὸ τοῦ πατρὸς γενόμενος "τὸν μὲν ἐγώ ", φησίν, "βούλευσα κ.τ.λ." [= 458-61]. ο μὴν σὺν Ἀρίσταρχος ἐξελε ταῦτα τὸ ἐπὶ φοβηθεῖς. Plutarch also cites 459 and the first half of 460 (as far as φάτων), with the variant τρίφεν φόνοις for παύσεν χλόον, at Vita Coriolani 32 (= 229B), and 461 alone at Moralia 72B. (Porphyry also seems to have been acquainted with the lines - see Van der Valk, RTSI I 462 - probably, I would say, through Plutarch.) Just what are we to make of Plutarch's story that Aristarchus "excised" the lines? Opinions range from one extreme - that the passage is a genuine one which Aristarchus excised on his own initiative without any diplomatic backing - to the other - that the passage was an interpolation, present in only a few pre-Aristarchean MSS., which Aristarchus omitted from his text primarily because of its very weak attestation. Van der Valk's position is at the former extreme (RTSI II 483-6). Gilbert Murray takes up the same position in the text of his book, but in a later footnote allows for the possibility that Aristarchus had some manuscript authority reflecting an earlier excision. Pasquali alleges that the "shocking" character of the passage guarantees its authenticity (a claim which, in general terms, we have already discussed and refuted); he says that in spite of ἐξελε Plutarch could not have known that the passage was unanimously attested by Aristarchus's sources, and that it is possible that the excision was first made before the time of Aristarchus, but that if it was it could not have spread to more than a small part of
the tradition, for otherwise it would have been known to, and, "without doubt", followed by, Zenodotus and Aristophanes. Wilamowitz also believes the passage to be genuine, but for him the hypothesis that Aristarchus was following an earlier excision is not, as it is for Murray and Pasquali, a mere possibility, but a necessity. On the other hand Leaf, Van Leeuwen and Bolling believe that the lines are an interpolation and had little support in Aristarchus's MSS. Leaf says ad loc. that "the statement that Ar. 'took them out' can only be true in the sense that they may very likely have been found in some of the editions current in the book-trade, such as the early papyri recently discovered, which were displaced under Alexandrine criticism by corrected copies of the old Attic vulgate". Similarly, Van Leeuwen says ad loc., "Immo Ar. in quibusdam libris olim inventos non recepisse censendus est" (my emphasis). Bolling (EE 121) says, "From Plutarch's statement we must infer that (1) he had observed that the lines were not in the vulgate MSS. of his day; (2) he believed that these MSS. depended on the edition of Aristarchus; (3) he knew, directly or indirectly, of a pre-Aristarchean text or texts in which these lines were contained; (4) he generalized and believed this of all early texts; (5) he drew the conclusion that Aristarchus had removed the lines, and proceeded to guess at his motives." On (4) Bolling comments, "The generalization is rash and opposed to what we know of Aristarchus; had he known of any MS. evidence worthy of consideration he would have written the lines, athetizing them had it seemed desirable." Whether or not one accepts this last comment, one must grant that Bolling's deductions on the workings of Plutarch's mind are in their main outline eminently plausible, though one could propose alternatives for some of the details, and it is possible that the thought-process described by Bolling was not that of Plutarch himself but of some grammarian on whom he was relying. It follows that Pasquali's unargued assumption that Zenodotus and Aristophanes did read the lines is unjustified and the argument which he has based on that assumption fallacious, as Bolling has himself pointed out (ALI 26 n. 9). But are we to agree with Bolling's comment on (4)? First, the view that Aristarchus would have been prepared to omit the passage even if he had found it in all his MSS. can confidently be rejected: we have already demonstrated the great caution with which he proceeded in establishing his numerus versuum, and there is no reason whatever to suppose that he may have found the
objections against the passage so overwhelming as to induce him to make such a radical departure from his normal modus operandi. There could have been three objections to the passage on moral grounds: (a) that it was unfitting for a Homeric hero to even contemplate the terrible crime of parricide; (b) that it was doubly unfitting that Phoenix, who was entrusted with the upbringing of Achilles, the greatest hero in the Iliad, should do so; (c) that what restrained Phoenix in the end was not conscience but merely fear of what people might say. We cannot deny that these objections may well have carried considerable weight with Aristarchus, but that weight would have been limited by the fact that Phoenix's thoughts of parricide are never put into effect but pass quickly from his mind; and, as we shall see, there is no linguistic evidence, and no overwhelmingly strong contextual argument, against the passage. Thus at the most we may grant that Aristarchus may have regarded the internal evidence against the passage as strong enough to allow him to omit it with slightly less external evidence against it than he would normally have required; but he would surely have required much more evidence against it than the omission by a very few MSS. which is posited by Pasquali and, apparently, by Wilamowicz. It then becomes a question of how likely it is that the passage would have been excised in pre-Aristarchean times and that such an excision would have led to the absence of the passage from the majority of Aristarchus's MSS., and what conclusions can be drawn from the internal evidence.

There can be no doubt that the passage could have provided adequate provocation for excision in the pre-Aristarchean era. Our quotations from Plato's Protagoras and Xenophon have illustrated the tendency to idealize Homer's heroes as moral exemplars, and the three possible moral objections against the passage which we have listed show that it may well have been seen as seriously conflicting with such idealizing attitudes; we can have no confidence that the countervailing consideration that Phoenix does not in fact kill his father would have been regarded by everyone as sufficient to redeem the passage. Bolling contends that "the supposed bowdlerizing" is "too sporadic" - "If these lines in I are shocking, so is much else in the tale of Phoenix." We have already dealt with this argument in general terms, but we can add here that in fact at another point in the Phoenix story the text certainly has been tampered with in order to eliminate a "shocking" element: we know from Schol. A and Eustathius that Sosiphanes emended
Tē πλάθμην καὶ έρεξα at 9. 453 to τῇ σῷ πλάθμην οὔ δ' έρεξα 189 and that this emendation was accepted by Aristodemus of Nysa, a γραμματικός and βήτωρ who taught in Rhodes, Rome and Nysa and was tutor to Pompey's sons: 190 we can thus plausibly see Aristodemus's motive as concern for the morals of his pupils. 191 In fairness to Bolling, however, one should add that making an intralinear alteration, albeit a radical one, would constitute a less radical form of bowdlerizing than actually deleting a passage.

Given, then, that the content of the lines 9. 458-61 would have made their excision possible, how likely is it that the excision would have spread to the majority of Aristarchus's MSS.? We may perhaps suggest that the lines may have produced such strong revulsion in fifth-century Athens that there developed a widely-held view (perhaps particularly among schoolmasters) opposing their inclusion in texts of the Iliad; but this hardly seems plausible. Less unlikely would be the hypothesis that the lines, though genuine, were absent from the Peisistratean Panathenaic text, having been deliberately omitted either by the reciter who dictated it (or its exemplar or archetype) or by some influential aoidos or reciter who passed on his version orally to his disciples. Then the general tendency for weakly-attested lines to spread through contaminatio may well have been partially checked in this instance by the "shocking" character of the passage. On the other hand it is at least doubtful whether the passage would have been likely to strike a reciter as early as the mid-sixth century as offensive. Of course it is not impossible that its omission was due not to deliberate expurgation but to a lapse of memory. We may conclude that the hypothesis that the lines were absent from the majority of Aristarchus's MSS. but are nevertheless genuine is possible but not particularly attractive unless we decide that the internal evidence strongly favours their authenticity. We shall now proceed to assess this evidence.

There can be no doubt that the passage is thoroughly Homeric in language, style and sentiment. So Leaf and Van Leeuwen ad loc.: ["The lines] are by no means un-Homeric in thought or expression"; "Colorem habent Homericum". One's impression that the lines are composed in the Homeric formulaic style can easily be confirmed by consulting the Concordances. For τὸν μὲν ἐγὼ βοûλευσα κατακτάμεν ὃξες χαλκῷ: ἀλλὰ ... Leaf well compares Od. 9. 299-302 τὸν μὲν ἐγὼ
Phoenix's attribution of the abatement of his anger to τὰς ἀθανάτους is typically Homeric, as is the concept of νέμεσις inherent in 459f.: in the latter case both thought and language are reminiscent of II. 6. 351 (mentioned by Leaf), ὅς ἦσθον νέμεσιν τε καὶ ἀναχνέα πάλι' ἀνθράκων. 194 Shipp points out that φάτος (9. 460) does not occur elsewhere in the Iliad (it occurs at Od. 6. 29, 21. 323, 23. 362), but adds that "with ι before ω [it is] an old form": thus its occurrence here can hardly be used as evidence against the authenticity of the passage.

Moreover, the passage can easily be seen as a variation on the common "homicide and flight" theme such as we might well have expected an oral poet of the Homeric era to introduce at this point. There are at least eleven passages in Homer where a man who has killed another flees his homeland to escape the vengeance of the dead man's kinsmen; the following collection of examples is my own and I cannot guarantee that it is complete: (1) II. 2. 661-70, (2) II. 9. 632-6 (by implication), (3) II. 13. 694-7 = 15. 333-6, (4) II. 15. 430-32, (5) II. 16. 570-76, (6) II. 23. 83-90, (7) II. 24. 480-82, (8) Od. 13. 256-86, (9) Od. 14. 378-85, (10) Od. 15. 223f., 272-8, (11) Od. 23. 118-20. To put this another way, a man who has permanently left his homeland and settled elsewhere is often depicted as one who has done so because he has killed someone. Sometimes the homicide seems to be introduced merely as a convenient narrative device to explain the presence of a foreigner: in (8) Odysseus uses it in one of his false tales to account for his presence in Ithaca, and in (10) it is employed to explain Theoclymenus's appearance on the scene and to get him accepted, as a suppliant, into the house of Odysseus, where the poet requires him for two prophetic performances (17. 151-61, 20. 350-94); (9) can be placed in the same category, and on (6) see Leaf on II. 23. 87 ad fin. Where the killer is received into the house of someone in the place to which he flees, he tends to stand in a special relationship of dependence and clientship to his host: thus in (4) Lycophron becomes the ἑράτων of Aias, in (5) Peleus and Thetis send Epeigeus to Troy ἀπ' Ἀχιλλῆς, and in (6) Peleus appoints Patroclus as the ἑράτων of Achilles. 196 Often the dead man is himself a relative of the killer: so (1), (3), (5) and (10). Now though Phoenix has not killed his father, all the other elements in
the "homicide and flight" theme which I have mentioned corresponded closely to the story of his life which he tells in 9. 438-95: after a serious quarrel with a relative he flees his homeland and is welcomed by Peleus, whom we have already come across as a host of refugee killers in (5) and (6), and is later sent by him to Troy with Achilles as the latter's tutor - in the meantime he has acted as his nursemaid: we may compare in particular the other associations with Achilles in (5) and (6); Phoenix's quarrel with his father is a narrative device to explain how he came to Phthia as an exile cursed with perpetual childlessness and thus plausible as an enthusiastic foster-father to Achilles; and the relatives (€ΤΤΥ xaL ₶νεψυο) of Phoenix, and thus of his father, who keep him under virtual house arrest after the quarrel and from whom he eventually escapes (462-78) may be compared with the kinsmen of the dead man from whom the killer flees in the homicide-and-flight theme. Now the use of traditional themes has for some time been recognized as an important element in the oral poet's method of composition, and it is inherently plausible to regard 11. 9. 458-61 as an integral part of that particular variation on the homicide-and-flight theme which the story of Phoenix's life constitutes: actual homicide would be inappropriate here, because it would be the particularly terrible crime of parricide and because it would seriously impair Phoenix's qualifications for the job of tutor to Achilles; but, we could argue, the traditional element of homicide nevertheless surfaces in an attenuated form, and in 458-61 Phoenix's consideration and rejection of homicide reflects the poet's. That the homicide-and-flight theme is a traditional and familiar one is evident not only from the frequency with which it occurs but also from two examples which in themselves imply that the idea is a familiar one: first, the simile of (7), "As when calamity overtakes a man who has killed someone in his own country and comes to the land of others, to the house of a wealthy man ...", and secondly, (11), "For even someone who has killed only one man goes into exile, leaving behind his kinsmen and his native land, even if the dead man is survived by only a few supporters to avenge him". Moreover, the attenuation of the homicide element in 11. 9. 458-61 can be paralleled: in (6) the poet seeks to diminish the guilt of Patroclus by making him a mere child (85 τυτδν έδντα, 88 ούχ έτελυν).
We must grant that the internal evidence so far considered is, at the very least, compatible with the hypothesis that the passage is genuine; but before deciding precisely how much weight we should attach to it we must proceed to consider the relationship of the passage to its immediate context.

Leaf declares ad loc. that "the lines are neither essential to nor inconsistent with the context"; but Monro ad loc. says that they "do not fit very well into the context", and Van Leeuwen goes further: "... hoc loco molesti sunt potius quam necessarii". That the lines are not essential to their context no one will dispute: the question is rather whether they are intrusive. The reference of τὸν μὲν (458) is clear enough; but the transition from 461 to 462 is awkward. The thought-sequence in 457-78 must be: "I contemplated killing my father, but through divine promptings abandoned the idea. Nevertheless, the situation at home, with my father furious, was now intolerable for me, and I decided to leave." There is nothing in the Greek corresponding to my "nevertheless", but I have inserted it because logic requires that the relationship of 462f. to 459-61 be an adversative one: lines 459-61 describe the abandonment of thoughts of murder, and thus a mitigation of Phoenix's antipathy towards his father (note esp. 459 παθομεν χόλον); 462f. imply that the antipathy nevertheless remains strong. Thus if we retain 458-61 the absence of an adversative particle in 462 is odd; whereas if 462 immediately follows 457 the relationship between 453-7 and 462f. is simply one of sequence and an adversative particle would be inappropriate.

This last argument is admittedly not strong enough to completely rule out the hypothesis that 458-61 are genuine, but in my opinion it is strong enough, in conjunction with the built-in odds against any given passage omitted by Aristarchus being genuine, to render an alternative hypothesis much more attractive. We may suppose that the lines are spurious but were added in one branch of the oral tradition at an early stage by a predominantly reproductive aoidos who had not, however, lost his capacity for original composition and who wished to make the quarrel between Phoenix and his father more dramatic: the idea for the lines would doubtless have sprung, by a process of association, from the elements of the homicide-and-flight theme which were already present in lines 438-95; and we may further suppose that the lines were absent from the Peisistratean text and from the vast
majority of Aristarchus's MSS. but found their way into some post-
Peisistratean MSS. through the agency of some reciter who represented
that branch of the oral tradition into which the lines had been
interpolated. A similar but more complex and perhaps somewhat less
plausible hypothesis would be that the lines were originally composed
not for II. 9 but as part of an independent Phoenix-lay, from which
they were later interpolated into the Iliad. 203

Even if we were to grant that the lines are genuine, the
consequences for other passages absent from Aristarchus's text would
be slight, because there are very few such passages where expurgation
would be even a theoretically possible explanation for their omission
from a substantial proportion of Aristarchus's MSS., and in all such
cases the opposite thesis of interpolation is much more likely. Among
the passages known to be pre-Aristarchean there are only two or three
where expurgation is even a remote possibility. One of these is II.
14. 241ab, 204 which has been well discussed (EE 145f.) by Bolling,
who remarks that "no one, as far as I know, has ever suggested that
our text has here been bowdlerized", and as far as I know this remains
true today: certainly Van der Valk does not attempt to defend the
lines. Another of these passages is II. 9. 119a, which Wilamowitz
thought might well be genuine, but I know of no one else who has gone
even as far as this; 205 even Van der Valk rejects the line. 206 It
is doubtful whether the innocuous II. 15. 5a 207 (= 2. 42) qualifies
even as a remotely possible example of expurgation, though Gilbert
Murray 208 strangely regards it as genuine, listing it among his
examples of expurgation and referring to it as "decidedly ἀπερατός, and
dropped from our texts": I know of no one who has expressed the same
or a similar opinion. Among the weakly-attested lines of our MSS. I
can find only three where it is theoretically possible that the weak
attestation indicates a line which Aristarchus omitted as a result of
earlier expurgation and which has subsequently returned to part of the
post-Aristarchean tradition via a lost scholiu m which cited it or a
pre-Aristarchean text which contained it. However, the 'built-in odds
against any given passage omitted by Aristarchus being genuine" to
which I referred in my previous paragraph must now be multiplied by
the built-in odds against any given addition to the numerus versuum
of Aristarchus going back to pre-Aristarchean times. There are other
reasons too why these three lines should be regarded as spurious:
each is a ὄφροδορμένος or a virtual ὄφροδορμένος, none is likely to have been regarded as seriously offensive, none is essential to its context and in fact the internal evidence against two of them, (a) and (c), is strong. I now list and discuss the lines concerned, in each case giving first, in brackets, the theoretically possible motive for expurgation. (a) II. 14. 269 Παολέθην, ἡς αἰὲν ἤμετρα (v.l. ἐκλείπει) ἡματα πάντα (lust in a god): the line is found in a substantial minority of our MSS., though those which have it in the text (as opposed to the margin) are all later than the 13th century with the possible exception of Allen’s M11 (saec. xii-xiii). It is based on the shortly following line 276, Παολέθην, ἡς τ’ αὐτὸς ἐκλέκτω ἡματα πάντα, and Leaf ad loc. well remarks, "The scribe who first interpolated this line from 276 appears to have aimed at originality by writing ἤμετρα for ἐκλείπει, quite unconscious of his false quantity." (b) II. 19. 177 θ’ Θέους ἐστίν, ἄναξ, ἡ τ’ ἀνδρῶν ἢ τε γυναικῶν (mistaken impression that this implied a sanctioning of relatively casual sexual relationships): the line is omitted by Pap. 9 (saec. vi p.C.) and by a substantial minority of our minuscules. It can easily be seen as a typical example of concordance interpolation: II. 19. 176 = 9. 275, 19. 177 = 9. 276. (c) Od. 7. 203a ἄργαλος γὰρ τ’ ἐστὶ θεὸς βροτῷ ἀνδρὶ δαμήναι (impiety, on the grounds that it is actually impossible for a mortal to overcome a god): the line is found only in Allen’s T (saec. xiv); it is repeated from Od. 4. 397 but is totally unsuited to its new context.

To conclude this Chapter:- The chances that any given line which Aristarchus found so weakly attested that he was prepared to omit it from his text is genuine must be judged infinitesimal; and the chances that a non-Aristarchean line present in some of our MSS. but not known to be pre-Aristarchean is genuine must be judged very much slighter even than this. Perhaps the non-Aristarchean passage with the best credentials is II. 9. 458-61, but the fact that its language, style and sentiment are compatible with authenticity does not mean that it is in fact authentic: in the ultimate analysis, as we have seen, the external evidence against it is supported by the internal evidence, because it does not fit well into its immediate context.
NOTES ON CHAPTER IV


2. For these texts see e.g. Ludwich (above, n. 1) Vol. I 46f.; Monro, Appendix 433-5; Allen, Origins (above, n. 1) 271-82.

Plural nounless adjectives referring to a class or group of texts qualify an understood ἐκδοσεως (editions), ὅραματα (recensions) or γραφαζ (manuscripts) when feminine and ἀντίγραφα (copies) when neuter.

3. In the light of what follows later in this paragraph of my text, I should point out that here T's πᾶσαζ is confirmed by A's "ξεκυον" πᾶσαζ.

4. A similar point has been made by Van der Valk, RTSI I 423f., though by means of different examples whose evidential value I regard as dubious.


7. I use the term "passage", here and subsequently, in the sense of "a line or group of lines".

8. On such cases see Ludwich (above, n. 1) Vol. II 136f.

9. For the former line see Bolling, EE 127f., Ludwich (above, n. 1) Vol. II 137f.; for the latter EE 150, Ludwich loc. cit.

10. See Bolling, EE 176f.

11. The limits of this omission have never (as far as I know) been so defined before, although this seems to me the most probable solution to the problem presented by the scholia at this point: it is therefore incumbent on me to justify this judgment, especially as we shall be returning to this passage later and as the question of the limits of the omission is related to the question of its cause.

There is an A scholium immediately after a lemma citing the first half of Il. 17. 133 (the lemma printed in Erbse's text of the scholia is a conjecture), and this scholium runs as follows: παρα Ẓηνοδότῳ κατ' ἐν τῇ Χείρι ὤκ ἤκουν οἱ γ' στίχον. (There is a very similar T scholium.) Scholars have understandably refused to believe that the scholium correctly designates the lines omitted, because the resulting text would be arrant nonsense. Nevertheless, Bolling, after a lengthy discussion, judged the most plausible hypothesis to be that the scholium as it stands is correct and that the omission "is a mechanical blunder, haplography starting from the similarity of ΑΙΑΣ and ΑΝΑΕΕ at the beginning of the lines. It will have happened in sources common to Zenodotus and the Chia ... . If Zenodotus so read, it was not
merely περιττή εὐλάβεια but sheer heroism to let the text stand without any Verschlimmbesserung in spite of its obvious defectiveness" (EE 173, cf. 174, and also ALI 26-8, TAPA 71 (1940) 43f.). Bolling's reference to Verschlimmbesserung implies that he believes Zenodotus knew no text which contained the missing lines, but this supposition is highly improbable, for the nonsense of a text without the lines would surely have led Zenodotus to suspect a lacuna and to hunt among his other MSS. for the missing lines, and he would presumably have had little difficulty in finding them; and if he knew the lines we cannot believe that he would have carried his general preference for the shorter text to the point where it produced nonsense. A further difficulty for this hypothesis is that Bolling's homoiarchon is very slight. Thus Bolling has judged the situation badly.

The conventional solution (adopted by, among others, Leaf and Allen ad loc., Ludwig [above, n. 1] Vol. II 138, Wecklein 59, Pfeiffer 114 and Erbse in his edition of the scholia) is to move the scholium one line down so that it would refer to lines 134-6. But a concluding summary (137) after a one-line simile (133) would be un-Homeric, so that if 134-6 were the lines omitted the omission could not be regarded as evidence of interpolation; as Bolling has put it (EE 173), "134-7 are inseparable". This consideration led Bolling to an outright rejection of the possibility that 134-6 were the omitted lines. Admittedly transcriptional error would hardly be a plausible alternative explanation of an omission of 134-6, since there is no relevant homoiographon worth speaking of and the passage is (for an omission without such temptation) a long one; and excision by Zenodotus without diplomatic backing can be ruled out, if not on Bolling's general grounds (the conservatism of Zenodotus), then at any rate by (a) the fact that the omission was also found in the Chia and (b) the lack of an adequate motive. However, an omission of 134-6 could conceivably have sprung from a dictating reciter's forgetfulness - we cannot be sure that Zenodotus would have been deterred from following such an omission by the literary and contextual consideration adduced above. However, this solution, though possible, does not strike me as particularly likely.

Bolling has viewed with some sympathy a third possible solution: attach the scholium to line 134, read δ' for γ and assume that 134-7 are an interpolation; the simile, he concedes, is a good one, "but we have no right to assume that the Iliad had a monopoly of good similes," and we may suppose that "somebody ... added to the text an excellent simile from another poem" (EE 173f., and cf. his ALI and TAPA treatments cited above; he would presumably also suppose that the concluding summary [line 137] was probably composed by the interpolator rather than found in his source). I would regard this solution as just possible, although - apart from other considerations - the double corruption in the scholia which it presupposes is not in its favour, as Bolling himself is aware (EE 174).

The solution which I myself regard as most probable is as follows: leave the scholium at line 133, emend γ' to σ' and suppose that Zenodotus was following an accidental omission in one or two of his sources caused by the very strong homoiarchon ἐτσηκει in 133 and 139: in other words some scribe, after
copying 132, read έστήκει at the beginning of 133 in his exemplar, wrote έστηκε in his apograph, and then, on returning to his exemplar, was trapped by the έστήκει at the beginning of line 139 into continuing with that line. The resultant text would yield good enough sense, since both the simile and the reference to Menelaus are dispensable, Ἀδας and καλώς would be followed by an appropriate finite verb, μέγα πένθος ἐν τῇ στῶσαν ἄξιων (139) could be suitably applied to Αἰας, and it would be open to Zenodotus to regard the shorter text as authentic and the additional lines as an interpolation. Moreover, on my assumptions about the probable date of the πολιτικά (to be explained later in this Chapter), it would be quite natural to suppose that the text of Zenodotus at this point was based, either solely or partly, on the text of the Chia itself.


13. For these two passages see Bolling, EE 218f.


15. E.g. the scholia (HPQ) on Od. 4. 569 say ἐν ἐννοίας δὲ οὐ φύτευται ὁ στῦχος δὲ τὸ ἀνάρως ἐχετίν τὴν ἀντωνυμίαν, and the scholia on the other three passages listed follow the same pattern. In each case the consideration mentioned in the scholia as a reason for the omission of the line in some of Aristarchus's sources is likely to reflect a reason actually given by Aristarchus himself as partial justification for his athetesis: in other words, a note from Didymus reporting the absence of the passage from some texts has probably been combined with a note from Aristonicus giving a reason for an athetesis by Aristarchus: cf. Bolling on Od. 11. 428 and 454-6, EE 241f. The plausibility of this hypothesis becomes abundantly clear when we compare the excerpt from Aristonicus on Od. 14. 188-90 (= 1. 171-3) preserved in Schol. HQ with the note in Schol. HM on Od. 1. 171-3 (both notes are quoted by Bolling at EE 218): the latter scholium says ὅπο ἐν τοιον οὐκ ἐφύρωντο after citing an argument which we know from the former scholium was used by Aristarchus in support of his athetesis. I now append some specific comments on each of the four passages:—

(1) II. 5. 786: see Bolling, EE 89. It is perhaps worth pointing out that whereas the scholia (ABCE3E4T) report ἐν τοῖον οὐκ ἦν ὁ στῦχος, Eustathius (607. 31) uses a stronger term: ἀλλοὶ δὲ φασίν ἐν πολλοῖς τῶν ἀντωνυμίαν μὴ κείσατα τὸν ... στῦχον.

(2) Od. 8. 333-42: I shall discuss this passage in detail later in this Chapter. For the main (H) scholium see Bolling, EE 239; but there is also a brief note in Schol. Vind. 56 (ignored by Dindorf and Bolling but quoted by Merry and Riddell ad loc.), ἐν ἐννοίας ἄσετονται: if this is not simply a late scholiast's would-be summary of the version of Schol. H, then probably, like the H scholium itself, it combines a reference to athetesis by Aristarchus (ἀσετοῦνται, δὲ ... - Aristonicus) with a reference to omission by some pre-Aristarchean MSS. (ἐν ἐννοίας οὐ φύτευται — Didymus).

(3) Od. 4. 569: cf. Ludwich (above, n. 1) Vol. II 137f. n. 117. I have quoted the relevant scholium at the outset of
this note. Bolling's judgment "I think it probable that a scholiast is speaking of the ἀντίγραφα of his own time" (EE 207) is inconsistent with his treatment of the similar scholia on Il. 5. 786, Od. 8. 333-42, 11. 428 and 454-6 and with the scholium on Od. 1. 171-3 (see above); he thinks the line was probably absent from the edition of Aristarchus, but (like all the other passages just listed) it is present in all our MSS. His position therefore has nothing to recommend it.

(4) Il. 2. 141: the T scholia state: ὥστε ὁ στίχος ἀναλαμβάνει τὴν ἀμφιβολίαν· ὥστε ἐν τισίν σφέρεται. Bolling (EE 69) says simply that it "cannot be determined" whether the τῶν are pre- or post-Aristarchean, but this judgment is, once again, inconsistent with the other evidence and with Bolling's own treatment of most of that evidence. It is true that the line is omitted by a single mediaeval MS., V32 (1304 AD.), but this is probably an isolated and independent transcriptional error springing from the strong homoioteleuton 140 πατρόκλα γαίαν, 141 εὐρυδίκην; at any rate, there is no need to connect it with the scholium — except in so far as it illustrates the possibility that the pre-Aristarchean omission was itself the result of mere transcriptional error: the temptation provided by the homoioteleuton is strikingly illustrated by the behaviour of our MSS. at Il. 9. 27f., where the two lines recur: 28 is omitted by Ludwig's X (= Allen's Vλ2, saec. xv) and is annotated with the remark λέξεις in Allen's M11 (saec. xii-xiii) and R1 (saec. xiii), which are both members of his family g.

Further, something approaching confirmation that Aristarchus both read and athetized the line is provided by Eustathius (188. 4-8): besides attesting the pre-Aristarchean manuscript omissions in much the same way as Schol. T (φατ ... ὥστε ἐν τισίν τῶν ἀντίγραφον ὥστε ἐφέρετο τὸ τολοῦτον, ὡς ...), he says ... τὸ "ὁ ν γὰρ ... ἐφερόμενον" [Il. 2. 141] παρασημειώνει το 0 τολοῦτον ὥστε ... (a longer version of the argument given by Schol. T follows). Erbse, who quotes the whole Eustathius passage in his commentary on the scholia ad loc., suggests that the section which I have quoted second may come ultimately from Aristonicus; in my opinion there can be little doubt that it does. παρασημειώνει το 0 here must mean "they mark (the line) with a marginal sign or signs" as in Schol. A on Il. 10. 398 Ἀμμάνδης δὲ ὁ Ἀριστάρχειος ... στιγμαῖς φιλαὶ τῶν Ἀριστοτέραν παρασημειώσασθαι αὐτοὺς (sc. τοὺς στίχους). In all probability, then, Aristarchus marked Il. 2. 141 with an obelus (and presumably also with an asterisk, since the line recurs at Il. 9. 28) — primarily, I assume, because of its absence from some of his sources, since the argument on the basis of the internal evidence can carry little weight (see Leaf ad loc.). Unfortunately, however, the note in Allen's apparatus ad loc., "post versum signs — adi. P37 (= I 28)", with his notes on pp. 77 and 197 of his Prolegomena, "P37 ... has ... dotted asterisks with obelus to B 141, 164", " — P37", turns out to be a red herring: Pap. 37 (Pack 632, late second century B.C.), in so far as it is not lacunose, contains all the σημεῖα which we know to have been present in Aristarchus's edition over the section of the text covered by the papyrus (see Allen, CR 17 [1903] 4f.), but the asterisk and obelus adjacent to line 141 appear to the right of the line, unlike all the other critical signs in the papyrus, which are placed at the left of the line concerned (the normal position), so that we must accept the judgment of B.P. Grenfell,
A.S. Hunt and J.C. Smyly, The Tebtunis Papyri Vol. I (London 1902) p. 16: "The asterisk and obelus in the margin refer to the corresponding line in the next column, of which the lower half is lost"; and their conjecture that the line concerned is 164 (= 180), of which Aristonicus (Schol. A) says ἀδετέτων καὶ ἀδυτοῦσις παράκειται, is almost certainly right. However, at least the papyrus does not testify against an Aristarchean obelus and asterisk to 141, since it preserves only the end of the line, viz. ἐν εὐρωπήτων. Further, the presence of the line in this very early post-Aristarchean papyrus is virtual proof that Aristarchus included it in his edition - a conclusion not challenged by Bolling, who concedes that if the scholium's τύχα are post-Aristarchean the omission referred to "is a case of haplography" (EE 69).

17. Schol. T says τυνες δὲ οὐ γράφουσι τὸν στίχον, but other sources provide more precise information: see Bolling, EE 188f.
18. See Bolling, EE 224f.
19. See also above, n. 15, on (1) II. 5. 786.
20. Schol. H: ἐν πολλοῖς οὐ φέρεται, ὡς ...: see above, n. 15, and Ludwich loc. cit. (above, n. 16). Lest anyone be tempted to suggest that the πολλά here may be post-Aristarchean (cf. above, n. 15, (3) and (4)), it is worth pointing out that the line is unanimously attested by the post-Aristarchean tradition, which includes two early papyri, Pap. 121 (Pack 1098, saec. i p.C.) and Pap. 78 (Pack 1099, saec. i-ii p.C.).
21. Schol. HOVind. 133 ἐν πολλοῖς οὐκ ἐφερέτο. See Bolling, EE 244, Ludwich (above, n. 1) Vol. II 137f. n. 117.
22. Cf. Ludwich, loc. cit. (above, n. 16). ἰ is Porson's emendation for Schol. H's λ', and this solution strikes me as more plausible than Bolling's, which is to accept λ' and posit an unidentified post-Aristarchean interpolation among the 30 lines so designated or to emend to λα" (EE 247f.). Confusion of uncial Δ and λ is common enough.
23. See above, n. 15.
24. See Bolling, EE 234f. The scholia on this line continue: Ἀριστοτέρχος δὲ περί μὲν τῆς ἀδετήσις ὑποτάξεις, on which Bolling (EE 235) comments, "[The use of the term ὑποτάξει] probably signifies that the scholiast had more detailed information than he could conveniently carry. One might conjecture that Didymus reported that the line was not contained in Aristarchus's first edition, but read and athetized in the second." However, Bolling's conjecture must be unhesitatingly rejected. Since athetesis is a term primarily denoting rejection, and the presence of the athetized passage in the text is only an implication of the term, to doubt or hesitate concerning athetesis must mean to be uncertain whether a passage in the text should be marked with an obelus or not; it cannot mean to be uncertain whether a
passage should be retained (and obelized) or totally omitted: that would be to doubt concerning the line's omission from, or presence in, the text. This interpretation is supported by the only other place in the Homeric scholia where the term ὁστάζειν is used in connection with an athetesis, Schol. HQ on Od. 6. 244f.: ἂν ἢ μὲν ἄντετε 'Ἀρωταρχος, ὁστάζειν ὃ ἐπὶ τοῦ πρῶτου, ἐπὶ καὶ Ἀλκιάδαν αὐτὸν μετέβαλε [μετέλαβε coni. Lehrs] παράδειγμα λεγοῦσα εὐλάβως Ζεὺς πάτερ, αὐ γὰρ ἔμοι κέρας εὖν."
Thus there is no reason to believe that Aristarchus omitted Od. 5. 337 from either of his editions.

25. See Bolling, EE 232, Ludwich (above, n. 1) Vol. II 136. Bolling's statement "The plus verses ... had according to the scholia almost no support in the tradition" goes slightly beyond the evidence, since the scholium's σχεδὸν ἐν πάσαις presumably refers only to the χαράδοςτερα, and the lines may have been present in a fairly substantial proportion of the εἰκαϊϊστερα.

26. See Bolling, EE 220, Ludwich loc. cit. (above, n. 16).

27. See Leaf ad loc.; Bolling, EE 197f., All 179; West 171.

28. The reference is to G.A. Gerhard, Griechische literarische Papyri: I Ptolemäische Homerfragmente, Veröffentlichungen aus der Heidelberger Papyrussammlung Band 4 Heft 1 (Heidelberg 1911) p. 87.

29. Thus Bolling attributes the shorter text to "certain unnamed editors" at EE 198, "τενές" in fl. Ath. ad loc.

30. I have noticed that it is frequent in English, both in students' essays and in the press. A single example must suffice: I quote from a front-page report on a coup in Thailand in the Adelaide Advertiser of 7th October 1976: "... all radio stations, except Radio Thailand, cannot produce their regular programmes" (for "No radio station except Radio Thailand may produce its regular programmes" or "All radio stations except Radio Thailand have been prohibited from producing their regular programmes"). In Greek, one may note that Aristotle, Plutarch, Lucian, Artemidorus and Stobaeus all quote Theognis 177f. as πάς γὰρ ἀνάρ πενήν ἐξωμιμήνος οὔτε τι εἴπετο | οὔτε ἔρξαν σύναται (for references see D.C.C. Young's Teubner edition of Theognis [Leipzig, 2nd ed. 1971] ad loc.) where strict grammar would require οὐδές γὰρ πενήν ἐξωμιμήνος ... Actually our MSS. of Theognis have καὶ where the quotations have πάς, and the usual explanation of this discrepancy - that καὶ is the original text and was altered to πάς to make the passage more gnomic when it was isolated for separate quotation - may well be right, but this does not weaken my point here, which is merely that the πάς version was obviously felt to be possible Greek.

31. See West 171f.; the quotation by Aeschines ends with line 91, but the fact that he has χρυσός ἐν ἀμφворотι τὸν τῶν πόρων πόλιν μὴτρο has as his 83b makes it virtually certain that his text omitted 92.

32. Eustathius must have had the scholium, in this or a very similar
form, before him when he wrote (1506. 40-42) τοῦτον δὲ τὸν στόχον
φασίν οἱ καλοί ἐν οὐδέμιο ἐκδόσει φέροντες, διὰ τὸ λέαν εὔτελές.
οὗ θαυμάζοντο πῶς ἔλαθεν Ἀριστοτέλης αὐτὸν.

33. E.g. in the T scholium on 11. 1. 598, which can be compared with
the A scholium on the same line: I have quoted both above, p. 48.
For other examples see Ludwich (above, n. 1) Vol. I 118-21.

34. Monro, Appendix 433; he also adduces other arguments (432f.).

35. The presence of the line in the edition of Aristarchus is
attested by (1) its presence in the entire post-Aristarchean
tradition and (2) Aristonicus's report of Aristarchus's
athetesis (Schol. A).

36. See above, p. 7.

37. Cf. n. 15 above, and note that the version of Eustathius (above,
n. 32) precisely fits the pattern discussed in n. 15.

38. For the internal evidence motivating Aristarchus see Schol. A
(Aristonicus) ad loc. in Erbse's edition of the scholia.

39. For the internal evidence see Leaf ad loc.

40. For the internal evidence see Bolling, EE 218.

41. For the internal evidence see Merry ad loc.

42. For the internal evidence see Bolling, EE 220, though his
citations of the scholia need to be supplemented by reference
to Merry and Riddell ad loc. or Dindorf's edition of the scholia.

43. For the internal evidence see Leaf ad loc.

44. In every case except the last (Od. 19. 130-33) the scholia give an
argument against the passage based on the internal evidence but
it is not absolutely certain that the argument was used by
Aristarchus: on all these passages except Od. 5. 337 my n. 15
above is relevant. On Od. 19. 130-33 the scholia record an
athetesis but no argument based on the internal evidence, but it
would be characteristic of Aristarchus to regard the lines as
wrongly transferred (with minimum adaptation) from Od. 1. 245-8
and 16. 122-5.

45. See Bolling, EE 218, though his citations of the relevant scholia
need to be supplemented by reference to A. Kirchhoff, Die
This objection by Aristarchus has been supported by:
P.D.C. Hennings, Jahrbücher für classische Philologie Suppl. 3
(1858) 163; E. Lentz, De versibus apud Homerum perperam
iteratis (Bartenstein 1881) 9f. (though he fails to mention the
omission of the lines by pre-Aristarchean MSS., and his discussion
also suffers from an apparent unawareness of the external evidence
against Od. 1. 185f.); F. Blass, Die Interpolationen in der
Odyssee (Halle 1904) 32f. (a commendably thorough discussion);
Bolling loc. cit.; and also Kirchhoff loc. cit. and A. Ronconi,
One needs to consult the full text of the T scholium in Maass's edition, as the excerpt given by Bolling (EE 77) and West (61) is enigmatically brief. For the "Ammonius" commentary see B.P. Grenfell and A.S. Hunt, The Oxyrhynchus Papyri Part II (London 1899) pp. 52-85, esp. 62, 78. Eustathius covers the same ground in a note on 11. 21. 155ff. (1228. 32-7).

The phrase ολ δε ψαυτων was inserted by Wilamowitz, and is required to balance the preceding ολ μεν υποτέκωνε ἡττά. The ψαυτων after φέροντα is my own insertion: it will both remove the harsh ellipse and explain the omission of Wilamowitz' supplement (haplography of ψαυτων, after which the surviving ψαυτων disappeared as a result of the similarity between ΦΑΣΙΝ and the immediately preceding ΦΕΡΕΣΘΑΙ).

This supplement by Blass is almost certain: see T.W. Allen, CR 14 (1900) 17; Grenfell and Hunt (above, n. 46) p. 78 (on line 17).

I.e. "was read": Allen's own supplement, first proposed loc. cit. (above, n. 48) and repeated in his edition (on 11. 2. 848). It must at least correctly represent the sense.

Cf. Bolling, EE 61; Jachmann (above, n. 52) 130-34; West 61.

With this argument cf. Jachmann (above, n. 52) 132, though he speaks of "die Alexandriner" in general rather than Aristarchus in particular, and the parallel with 11. 2. 558 is my own.

See below, pp. 154-9.

See Bolling, EE 221f.

See Bolling, EE 127.

See Bolling, EE 79; Van der Valk, RTSI I 188f.; Erbse's commentary in his edition of the scholia ad loc. It is only fair to add that we cannot prove that Aristarchus used the edition of Euripides (see Bolling, EE 38f.) - though Van der Valk (loc. cit.) thinks Eustathius' reference to it goes back to Didymus.

See, respectively, above, pp. 4-6; Bolling, EE 141f.; EE 144f.

The possibility that absence from the majority of the χαρέςτερα could sometimes have been due to critical excision will be considered (and rejected) later in this.
Chapter.

61. See also Gerhard (above, n. 28) 88.

62. Examples may be found among the lists in Allen's Origins (above, n. 1) 271-98 and in Monro, Appendix 419f.

63. See e.g. Allen, Origins (above, n. 1) 249-70; Van der Valk, RTSI II 264-369. However, where a quotation contains a variant unknown from other sources there is usually a distinct possibility that the author concerned has made a slip of memory: see e.g. Van der Valk, ibid. 270f., and below, n. 179.

64. For the limited influence of Aristarchus on the text of the subsequent tradition within his numerus versuum, see above, pp. 37f.

65. Bolling is here using the term "variants" in the widest possible sense, so as to include differences in the numerus versuum.

66. AJP 70 (1949) 368; cf. ALI 5.

67. HSCP 43 (1932) 46f. Cf. Parry's question, HSCP 41 (1930) 75f., "How have they [viz. the Unitarians] explained the unique number of good variant readings in our text of Homer, and the need for the laborious editions of Aristarchus and the other grammarians, and the extra lines, which grow in number as new papyri are found?" Cf. M.W.M. Pope, "The Parry-Lord Theory of Homeric Composition", Acta Classica 6 (1963) 1-21, esp. 6f., where he acutely points out that this argument of Parry's has been ignored by the latter's disciple A.B. Lord in his theory that Homer dictated his poems (e.g. "Homer's Originality: Oral Dictated Texts", TAPA 84 [1953] 124-34), though Pope probably exaggerates in characterizing Parry's desire to explain "the state of our text of Homer" as "the purpose for which the whole of Parry's theory of Homeric composition was originally invented". See also Monro, Appendix 419f.

68. Odysseus's point is surely that great physical effort (implied in υδ' χεν τις ... ρέτα μετοχλόσεξεν) would be needed to move his bed. The task would be χαλητόν ... καὶ μάλ' ἐπισταμένῳ (184f.), presumably because brawn would be needed as well as brains. The bed-post was the trunk of an olive still rooted in the ground, a trunk as thick as a pillar (191), which one would first need to cut through at its base if one wanted to move the bed (204; note again τις ... ἀνδρῶν, with no mention of women, in 203f.). In this context not only would the addition of γυναίκῶν in 187 be out of place, but the removal of μάλ' ἡδῶν would considerably weaken Odysseus's point. It is worth noting that in the only other places in the Homeric epic where the phrase υδ' μάλ' ἡδῶν occurs, 11. 12. 382 and 24. 565, the context again consists in or includes the difficulty involved in moving something, and that the latter passage contains the only other occurrence in the Homeric epic of the compound μετοχλόσεξεν, again in the phrase ρέτα μετοχλόσεξε (11. 24. 567); moreover, all three passages contain the words υδ' χεν (ρέτα 11. 12. 381), and with ἀνδρῶν δ' οὐ ... τις ἐκδός βροτός in Od. 23. 187 compare 11. 12. 381-3 υδ' ... ἀνήρ ..., οὐκ τινως βροτος εἶναι and 24. 565.
ou ... Νυνακων (no mention of women in either passage!); and the opposition between god and man in Od. 23. 184-8 mirrors that in II. 24. 564f. In defence of Νυνακων it might be argued that Penelope has instructed her maidservants to move the bed (177-9); but the movement whose difficulty Odysseus is describing is not the movement just proposed by Penelope but the earlier, radical, movement (involving the cutting off of the bed-post) which Penelope's present instruction appears to presuppose: τος ου μοι Νυνακω μεγα λέχος; (184), Νυνακω μεγα, ημων ειπο πυθενε' έλαθε (204).

69. Νυνακω ... γυναλκων (genitive), Od. 14. 308, 21. 323, 23. 147, II. 9. 134, 276, 19. 177, 24. 698; other cases, Od. 15. 163, 19. 408, II. 7. 139, 15. 683; the conjunctions are ηδε, τε, ητε, μήτε.

70. See Van der Valk, TCO 163 n. 2.

71. Van der Valk (TCO 162f.) seems to believe that Aristarchus objected to the reading on the grounds that it exaggerated the danger to the suitors from Telemachus (given their limited knowledge?), but this notion is patently preposterous, and not only because Antinous has in any case just said Νυνει κατ προτέρω κακων έμεναν (667).

72. So A. Pierron in his edition of the Odyssey (Paris 1875) ad loc. and Merry ad loc.

73. One could posit such a parallel in a lost epic, but this would savour of special pleading.


75. Θεσεσολο: II. 1. 591, 2. 457, Od. 9. 434, 13. 363, 24. 6; Θεσεσολων: Od. 12. 158.

76. I say "at any rate on the whole" because, as West (13) has pointed out, "there remains a large number of [additional] lines [in the 'eccentric' papyri] for which no close parallel can be found"; she comments that "some of these may have been composed for interpolation, but it is equally possible that they come from lost hexameter poetry".


79. Kirk touches on this difficulty in his article "Homer's Iliad and Ours" (above, n. 78: Proc. pp. 55f., = repr. pp. 139-41), but his only argument is to concede that Homer's various performances would have differed slightly from one another.

80. Striking examples of such repetition are Od. 17. 124-41 = 4. 333-50 (Telemachus quoting Menelaus to his mother) and Od. 17. 427-41 = 14. 258-72 (a passage common to two of Odysseus's false tales). Now there are numerous instances of non-formulaic repetition - e.g. where a messenger has been entrusted with a message and the poet immediately proceeds to narrate its delivery - where we could imagine the poet, in the process of composition, saying to himself, "Now I must make a special point of remembering this passage accurately, because I shall presently need to repeat it." However, it does not seem likely that it would occur to him to accord such special treatment to e.g. Od. 4. 333-50 or 14. 258-72. For this reason I am inclined to regard the repetition of such passages as evidence in favour of the view that the Homeric poet memorized the whole of his own monumental creation with much greater word-for-word accuracy than many Parryites would believe possible. Cf. A. Hoekstra, Homeric Modifications of Formulaic Prototypes (Amsterdam 1965) 18-20; M.W. Edwards, TAPA 97 (1966) 177-9; D.C.C. Young, "Never Blotted a Line? Formula and Premeditation in Homer and Hesiod", Arion 6 (1967) 279-324, esp. 311. Further, Young (ibid. 284f.) cites the case of Duncan Macintyre (1724-1812 A.D.), an illiterate Scottish Gaelic creative poet who premeditated and then dictated his compositions; but the parallel with Homer is far from complete, since Macintyre's poems were non-formulaic; and while it is a reasonable assumption that two dictations by this poet of one of his longer poems would have been virtually identical, the evidence does not enable us to be certain about this. On the other hand the almost verbatim correspondence between the 1944, 1950 and 1953 recordings of Duncan Macdonald's version of a traditional Gaelic tale (Young 296f.) can be demonstrated, but because of the highly traditional nature of the tale Macdonald is the counterpart of the Homeric reproductive aoidos or rhapsode rather than Homer himself, and we must not overlook the difference that Macdonald's story was told in prose, not verse. Similarly, Young's "Russian bylina collected in almost identical terms from the same singer at 40 years' interval" (ibid. 297) seems to have been sung by the equivalent of a reproductive aoidos. Nevertheless, these last two examples do help to support Young's general point that "the concept of a fixed text is not incompatible with illiteracy" (ibid.).

81. So, apparently, Bolling (EE 33) in the statement already quoted, "... that [written] tradition can have, on account if its small divergence, but a single fountain-head", and West 13, "The relatively minor scale of the interpolations [in the 'eccentric' papyri] argues against the view that there is a connection between the eccentricities of the early texts and the long oral tradition of the poems ..." (emphasis in both places mine).

82. E.g. by Merry on 565; Monro, Appendix 320; R. Merkelbach, Untersuchungen zur Odyssee (Zetemata Heft 2, Munich 1951, 2nd ed. 1969) 177; D.L. Page, The Homeric Odyssey (Oxford 1955) 24-7.
Kirk, Songs (above, n. 78) 236f. (he refers to the passage as 568–627, but cf. n. 83 below); M. Bowra, Companion 45; and even the inveterate Unitarian Stanford concedes ad loc. that 568–627 is "probably ... an interpolation" and that 565–7 "may be taken as part of the following long interpolation".

83. Schol. HT νοθευταὶ μέχρι τοῦ "ὡς εὐτυχ ... εὐω" [627] ... ὑπὲρ δὲ τῆς ὀστισμεὶς αὐτῶν λέγεται ...; and Schol. Pindar Ol. 1. 91, in discussing the nature of Tantalus's punishment, cites Od. 11. 583f. "καὶ τὰ ἔξης", i.e. refers to 583–92, and adds πάλιν εἰ μὴ κατὰ τὸν Ἀρισταρχον νῦν ἐξαλ τὰ ἔξη ταῦτα (sic Schol. Pind ACDEH, sed verba καὶ τὰ ἔξης om. DEH). It is apparently not clear from the MSS. of the Odyssean scholia to precisely which line their note should be attached, and it has thus been disputed whether the passage condemned by Aristarchus started at 565 or 568 (see Dindorf's apparatus ad loc. and K. Lehrs, De Aristarchi studiis Homericis [3rd ed. Leipzig 1882, repr. Hildesheim 1964] 156); the former seems the more likely starting-point, since 568 could hardly have directly followed 567, though it could easily have followed 564, and this would not have been likely to escape Aristarchus's notice. Be that as it may, the interpolation must in actual fact have started at 565, both for the reason just referred to and because 565 destroys the effect of the preceding lines (a fact which may well also have been observed by Aristarchus): see e.g. Stanford ad loc. and, for a devastating polemic, D.L. Page (above, n. 82) 26f.

84. See e.g. D.L. Page (above, n. 82) 21–51.

85. There would have been a few transitional lines in the original version: see my discussion in Acta Classica 17 (1974) 2lf.


87. D.L. Page (above, n. 82) acknowledges that the judgment of the two Alexandrians on the ending of the Odyssey may have been based on the internal evidence alone (p. 101) and that the internal evidence is in fact insufficient to condemn the passage 23. 297–343: in fact he goes out of his way to show the inconclusiveness of the arguments which have been brought to bear against it (pp. 115f.). He thus seems inconsistent in concluding that "the Alexandrians placed the end of the Odyssey before this passage, not after it. It would be very imprudent of us to contradict them without strong reasons; and we may as well admit that we have no such reasons" (p. 116, cf. p. 129): I would say that on Page's own preceding assumptions about the passage (which I share) the burden of proof rests with him who asserts that it is spurious. To the literature cited by Page one may add J.A. Notopoulos, "Continuity and Interconnexion in Homeric Oral Composition", TAPA 82 (1951) 81–101, esp. 91–5: Notopoulos sees Odysseus's summary of his adventures (23. 301–41) as typical of
the repetition proper to oral poetry in general and "the Homeric oral style" in particular, assigns it to the species "recapitulation" within the genus "retrospection", compares it with other recapitulations within the Odyssey and praises its artistic appropriateness and internal structure. However, even if the passage 297-343 as a whole is genuine it may well have suffered some interpolation over and above the intrusion of 320 into part of the post-Aristarchean tradition - e.g. 322-5, the summary of Odysseus's visit to the Underworld.

88. Schol. M V Vind. 133: 'Αριστοφάνης δὲ καὶ 'Αριστοφάνης πέρας τῆς 'Οδυσσέας τούτο ποιούσατο; Schol. HMQ: τούτο τέλος τῆς 'Οδυσσέας φησιν 'Αριστοφάνης καὶ 'Αριστοφάνης; Eust. 1948. 48 - 1949. 2. The only major collection of scholia to omit the report is Dindorf's D, which however according to Dindorf (p. xiii) contains "almost no" scholia after Od. 10. (I have omitted from the reckoning Dindorf's B, E, R and S, whose scholia in each case stop completely well before 23. 296.)

89. Cf. e.g. Kirk, Songs (above, n. 78) 311: "The tradition that the Doloneia was added at this stage may well be true ... ."

90. For a careful and properly cautious consideration of the evidence see Kirk, Songs (above, n. 78) 185-8, 307, 316-18, 325f.


92. From c. 700 B.C. (the appearances of the poems on the mainland) to c. 600 B.C. (Page's terminus post quem for Od. 24. 205-411).

93. Schol. Pindar Nem. 2. 1: ἢν δὲ ὁ Κύναθος τὸ γένος Χεδο ... πρῶτος ἐν Συράκουσίᾳ ἐρρημόθησε τα ὑμήρου ἐπὶ κατὰ τὴν ἔθο' 'Ολυμπιάδα, ὡς 'Ιππόστρωτος φησιν. Kirk (Songs [above, n. 78] 272) favours acceptance of the date but rejection of πρῶτος: "This last piece of information is improbable as it stands; but it is less likely that the date is seriously wrong than that the nature of Kynaithos's service to the Syracusans is misconstrued." H.T. Wade-Gery favours acceptance of both πρῶτος and the date in "Kynaithos", Greek Poetry and Life (Festschrift Gilbert Murray, Oxford 1936) 56-78, repr. in the author's Essays in Greek History (Oxford 1958) 17-36: see esp. pp. 72-8 = 31-6. Moreover, Wade-Gery apparently favours a date about a century later than that of Kirk for the introduction of the Homeric poems to the Greek mainland - though one must bear in mind that much of the relevant archaeological evidence available to Kirk in 1962 was not available to Wade-Gery in 1936.

94. Plato Ion 530 A-B.

95. Some evidence for the existence of groups of rhapsodes consisting of a master and disciples in the late sixth century B.C. is provided by the phrase ὁ πρὸς Κύναθον in Schol. Pindar Nem. 2. 1c and e: see pp. 23f. of Z. Ritoćk, "die Homeriden", Acta Antiqua 18 (1970) 1-29.

96. For the "Peisistratian recension" see e.g. Kirk, Songs (above, n. 78) 306-12.
97. For the Homeridae see e.g. Davison, Companion 219; Kirk, Songs (above, n. 78) 272; and, for an exhaustive treatment, Ritoček (above, n. 95).

98. Said of Hipparchus in the pseudo-Platonic Hipparchus at 228B, but for the view that the statement should probably be applied to Peisistratus see Kirk, Songs (above, n. 78) 307-10 and A. Dihle, Homer-Probleme (Opladen 1970) 107f.


100. Cf. Eustathius 6. 40-42 ἔλυμησαντο δὲ, φασάν, αὐτῆς σάμπολλα οἱ περὶ τὸν Κώναλδον καὶ πολλὰ τῶν ἐκῶν αὐτοῖς ποιήσαντες παρευθηλον. διὸ καὶ δωράδαις ἀν Ὀμηρικαὶ βῆβλοι ....

101. I shall not attempt to assess which of these three hypotheses is the most plausible. However, two points may be worth making:—

(a) Hypothesis (3) would be compatible with Dihle's theory (op. cit. [above, n. 98]) that the Peisistratian text contained some episodes which were composed in writing—though, like J.B. Hainsworth (CR 22 [1972] 316f.), I am sceptical about the validity of Dihle's arguments for non-orality.

(b) A situation where all our sources agree in genuine in-trialinear corruption (as distinct from mere orthographic modernization), which I have supposed to be possible at Od. 10. 193 (Acta Classica 18 [1975] 135-7) and virtually certain at Od. 10. 455 (Acta Classica 17 [1974] 11-23), is easy to account for on hypothesis (1), only slightly less easy to account for on hypothesis (3), but fairly difficult to account for on hypothesis (2), though somewhat less difficult where we suppose that the corruption arose before the poems' dispersal.

102. The possibility of the omission of genuine lines through a different process will be considered presently.

103. That some form of contamination operated even in the pre-Aristarchean era is a basic supposition behind Bolling's work, and the idea receives support from those situations where (a) we are told merely that "some" pre-Aristarchean texts omitted a line, and (b) the internal evidence tells strongly, or at any rate fairly strongly, against that line. Bolling often exaggerates the weight of the internal evidence against lines in whose attestation there is some weakness, but there are occasions when criteria (a) and (b) are both fulfilled: I would regard Od. 1. 171-3 as a good example (see above, n. 45).

104. Above, p. 56.

105. Cf. above, p. 35 and p. 43 n. 5.

106. Cf. above, p. 35 and p. 43 n. 3.

107. I cite a single example, chosen almost at random. A note attached to Od. 1. 279 in both the H and the M scholia reads: οὗτος δὲ οὐκ ἔτι κατὰ Ῥωμον οὐκ ἂν. Bolling (EE 220)
follows others in transposing the note to line 278, commenting, "The omission of line 279 could be nothing but an accident." But why should it not have been a mere accident? The temptation to omit 279 in the form of the homoioteleuton in 278 (ἐκ τῶν) and 279 (ὡς τῶν) means that such a transcriptional error could easily have occurred, and this explanation receives support from the fact that no source other than Rhianus is cited for the omission. (However, this is not to deny that the usual transposition is possible.) In the case of a second example, II. 18. 39-49, omitted by the Argolikes, Bolling has himself (ALI 158) criticized his own earlier treatment (EE 177ff.): "... I should have given more weight to the possibility that ... the disturbance of the tradition may be simply the result of a 'blunder'" (there is a very strong homoiographic temptation: 38 τάσις ὡς κατὰ βέβης ἄλος Νηπτύω τέσσεραν, 49 ἄλλα κα τὰ βέβης ἄλος Νηπτύω τέσσεραν); see further Van der Valk, RTSI II 437-9, and below, n. 137.

108. See Pfeiffer 72 n. 4; Bolling EE 38f., 77, 79; and above, n. 58 and pp. 53f.

109. Allen, Origins (above, n. 1) 297f., lists 13 citations. N.C. Wilson, CR 19 (1969) 369, criticizes Pfeiffer for failing to mention that Bolling (EE 39) thought the edition of Antimachus might well be the text of someone other than the poet Antimachus of Colophon, but Wilson's criticism is hardly warranted: Bolling's theory was spawned by his special interest in arguing that the texts used by Aristarchus were late, and has little to recommend it, since (i), as Bolling himself concedes, the scholia elsewhere cite Antimachus of Colophon, but "they [do not] distinguish between two men of the same name"; (ii) no other suitable Antimachus is known from other sources; and (iii) Antimachus of Colophon is known to have had a special interest in Homer and to have written on his life (Pfeiffer 94f.).

110. I deliberately ignore the alleged "edition" of Aristotle: I agree with Pfeiffer 71f. that it is "very improbable" that Aristotle ever prepared a διάθεσις of Homer, and even if a text associated with his name survived into later times (his own copy, or one which he gave to Alexander, or a copy of one of these?), there is no evidence that it was used by Aristarchus.

111. Origins (above, n. 1) 283-96.

112. Ibid. 291-4. Further literature on the πολιτικαί is cited by Van der Valk, TCO 14 and RTSI II p. 1. Van der Valk's attempt to show that most of the readings ascribed to the πολιτικαί are Hellenistic conjectures (TCO 14-21, RTSI II 1-9) is unconvincing.

113. Above, n. 11.

114. I cannot allow myself the luxury of arguing this position here: the question is an extremely complex one. I merely state my opinion that even when due allowance has been made for the loss of information through various causes along the long road from the ἴππομονήμαta of Aristarchus to our surviving scholia, the frequency with which Zenodotus is the sole source cited for an omission remains remarkable.
115. Bolling's attempt (EE 41) to avoid the natural implication of the terms Sinōpikē, Massaliōtikē, etc. - viz. the implication that "the name [is] that of the city from which the MS. came to Alexandria" (loc. cit.) - and especially his suggested alternative explanation of the terms, that "some editions were prepared [in Alexandria] for the export trade, and named accordingly", will surely strike most readers as absurd.

116. This formulation will exclude the Krētikē if Bolling's suspicion (EE 39, ALI 172) that it was not used by Aristarchus is right.

117. See above, n. 11.

118. Cf. Bolling, EE 173; ALI 27f.; "Zenodotus' Dehorning of the Hornèd Hind, and the Text of Homer", TAPA 71 (1940) 40-44, esp. 43f. However, Bolling is misleading in stating that "the scholiast ... proceeds to ascribe a motive to Zenodotus" (my emphasis - EE 173, and similarly ALI and TAPA locc. cit.): both Schol. A and the similar but briefer Schol. T clearly mention the motive as an explanation of the omission in both Zenodotus and the Chia.


120. Below, pp. 215f.

121. See e.g. Blass (above, n. 45) 29f.; Bolling, EE 217f.

122. Pap. Oxy. Part II No. 221 (above, n. 46 and p. 53) Column XV lines 6-27, pp. 71, 83f.; the relevant section is also transcribed and discussed by Bolling, ALI 172.

123. See above, n. 116.

124. For the influence of such homoiomesa see below, Chapter V n. 58.

125. A scholium (AT) on II. 18. 376 ascribes to the εἰκαστέρας a version of that line which, if the scholium is not seriously corrupt, must imply the absence of 377. However, the scholium probably is seriously corrupt: so Dindorf on Schol. A ad loc. and Bolling, EE 181, while Leaf and Van Leeuwen ad loc. regard such corruption as possible; and this hypothesis is supported by the fact that neither Schol. A nor Schol. T mentions the omission of 377. However, if we do suppose that the line was omitted by some MSS., this cannot possibly have been the result of a diplomatically unsupported excision, for which there could have been no conceivable motive.

126. See above, p. 16.

127. West 129; homoiomeson 402 ΑΕΙΔΟΣ, 403 ΠΑΙΔΟΣ: for the influence of homoiomesa such as this and those pointed out in the next three notes see above, n. 124.

128. West 255; homoiomeson 68 τοσι, 70 μαλακοται.

129. West 255; Payne Knight's theory on the origin of the line, which West views as probable, may be right, but she should not have
completely ignored the strong possibility that the absence of the line from this papyrus is merely an accident stemming from the very pronounced homoiomeson 74 ἀποικος ἀπεχθηται, 75 ἀπαντοῦσαν ἀπεχθόμενος.

130. West 269; homoiomeson 363 TINEI, 364 TINEI.

131. See e.g. Bolling, EE 60f.

132. See e.g. Bolling, EE 85f., though in his citation of Schol. A (Aristonicus) he has omitted the opening explicit attestation of the δικλαί περιπετευμένα affixed to II. 4. 88f.

133. See e.g. Bolling, EE 165-9.

134. See e.g. Bolling, EE 188f. For Aristarchus's defence of the line see Schol. AGε and the "Ammonius" commentary, Pap. Oxy. Part II No. 221 (above, n. 46 and p. 53) Column IX lines 5-8, pp. 64, 79.

135. See e.g. Bolling, EE 66f., ALI 65.

136. See e.g. Bolling, EE 174f., ALI 157f.

137. Viz. II. 18. 39-49: its inclusion is based on Bolling's gratuitous emendation of the scholia (EE 177f., ALI 158, apparently accepted by Van der Valk, RTSI II 35). Schol. A (Didymus) tells us: ὁ τῶν Νηρείδων χαρᾶς προσέθηται καὶ παρὰ Ζηνοδότη; Bolling wants to punctuate after προσέθηται and continue καὶ παρὰ Ζηνοδότη <οὔτε ἦσαν οἱ στίχοι>. But the text of Schol. A is entirely satisfactory as it stands: προσέθηται καὶ παρὰ Ζηνοδότη - i.e. "it was pre-athetized [not only by Aristarchus but] also by Zenodotus" - is precisely parallel to Didymus's frequent use of expressions such as ἡθέτη καὶ παρὰ Ἀριστοφάνει (Schol. A on II. 8. 284), i.e. "it was athetized [not only by Aristarchus but] also by Aristophanes" (cf. also e.g. Schol. A (Didymus) on II. 8. 557 and 12. 175). Erbse was right to judge (in his edition of the scholia ad loc.) that Bolling had emended "vix recte".

138. See above, n. 9. There is one line - II. 14. 114 - merely athetized by Zenodotus which Aristophanes omitted, if we are to believe Schol. AT, but we should probably accept the assumption of Nauck, Duenter and Wecklein that a corruption has led to an interchange of the names and emend to the usual om. Zen., ath. Aristoph.: see Bolling, EE 143f.

139. Cf. Bolling, EE 182f., though he thought the couplet was probably an interpolation. Actually the homoiographic temptation to omission is stronger than indicated by Bolling: not only are there close similarities between lines 595 (τῶν δ' αἱ μὲν λεπτὰς ὀθάνος ἔχουν, οἱ δὲ χιτώνας) and 597 (καὶ δ' αἱ μὲν καλὰς στεφάνας ἔχουν, οἱ δὲ μαχαίρας), but there is also a homoiarchon in 596 and 598 (ἐκατ', ἔχουν, more obvious as EI-): the omission could then be seen as an almost exact parallel to the omission of Od. 19. 331-3 by one of Ludwich's MSS.: see my discussion in Acta Classica 17 (1974) 28f., esp. 29 n. 66.

140. See C. Mayhoff, De Rhiani Cretensis studiis Homericis (Leipzig 1870) 29-89, or the list of W. Aly, RE 2e Reihe [R-Z] IA 1 (1914) 788f.
Mayhoff (op. cit. [above, n. 140] 30-35) regards them all as excisions, Bolling (EE 176ff., 219ff., 230ff.) as interpolations; I have already suggested (above, n. 107) that one of them may well be due to a mere slip in copying.

See Ludwich (above, n. 1) Vol. I 3; F. Susemihl, Geschichte der griechischen Literatur in der Alexandrinerzeit Vol. I (Leipzig 1891, repr. Hildesheim 1965) 374 n. 118. Susemihl suspects that in Schol. A and Eustathius on 11. 9. 453 ἐωσφάνης may be corrupt for ἵωςφένης: if so, this would give the latter a sixth intra-linear reading, but I think the text of the scholium is probably sound: see below, n. 189.


See above, notes 17 and 134.

See above, n. 11 and p. 76.

See above, n. 132.

See above, pp. 1f.

Thus Allen (in his editio minor ad loc.) and Van der Valk (RTSI II 397) err in saying that Zenodotus "athetised" the lines.

Thus, rightly, Leaf ad loc. "om. Zen. and others", Wecklein 58 "Es gab ... noch andere Ausgaben [sc. in addition to that of Zenodotus], in denen die Verse fehlten".

The long scholia on these lines are best read in full in Erbse's edition (Vol. IV p. 436 line 50 - p. 438 line 91) in conjunction with his commentary. The ὑπλὴ affixed to line 10 is explicitly attested by Aristonicus (Schol. A: ἡ ὑπλῆ, πρὸς τὸ ἀμφιβολὸν κ.τ.λ.), and that Aristarchus defended the lines is explicitly stated by Didymus (Schol. A: ὥστε δὲ φησιν ὅ 'Αρσαταρχὸς οὐτὸς αὐτοῦς παραδόχεσθαι κ.τ.λ.: I have expanded the compendium αὐ in A to αὐτοῦς, referring back to αὐτῷ [sc. στέχου], as this seems most natural, though the editors have printed either αὐτὸν or [Erbse] αὐτό).

See above, p. 13 n. 33.

G. Pasquali, Storia della tradizione e critica del testo (2nd ed. Florence 1952) 231.

Op. cit. (above, n. 152) 231 n. 3.

On allegorical interpretations of Homer see e.g. Pfeiffer 9-11, 35f., 237-40.

See (besides the statement of Bolling already quoted) EE 122, "Had there been a successful bowdler at work on the Homeric poems, it is obvious that we would never have got the story of Phoenix in its present shape", and EE 239, "An expurgator would certainly have cut deeper".

157. See Kirk and Raven (above, n. 156) Fragments 164 and 169.

158. For Theagenes see e.g. Pfeiffer 9-11.

159. For further examples, and a wide-ranging discussion of this whole question, see W.J. Verdenius, *Homer, the Educator of the Greeks* (Amsterdam 1970) 3-11.

160. The general interpretation of the passage 29-41 as a whole is hotly disputed: L.R. Farnell, *The Works of Pindar* (3 vols.), *Translation* (London 1930) pp. 48, 51f., *Commentary* (London 1932) pp. 68-70, 472, is opposed by G. Norwood, *Pindar* (Berkeley 1945) 236 n. 37, but (in general) supported by C.M. Bowra, *Pindar* (Oxford 1964) 54-6. However, all three interpretations (see also Norwood, ibid. 80) are compatiable with the point made in my text, and the same is true even of the interpretation of J.E. Sandys in his Loeb edition of 1915 (p. 99 with n. 1), which departs more radically from the Farnell-Bowra line than Norwood's does and for the most part follows B.L. Gildersleeve, *Pindar: the Olympian and Pythian Odes* (London 1885) 205; but, unlike Sandys, Gildersleeve, to judge from his notes on lines 38 and 40, seems to take ἐὰν τὸλμουν μάχαν τε πᾶσαν | χωρὶς ἀδανάκτων to mean "Do not vie with the gods": this, however, is extremely forced and highly implausible.


165. Above, n. 15 ad init. and section (2).

166. Murray (above, n. 162) 123; Van der Valk, *TCO* 260. Similarly, Kirchhoff (op. cit. [above, n. 45] p. 213) has no doubts that the absence is due to excision, and one gets the impression that he regards the passage as in all probability early, though on this latter point he expresses himself cautiously: "Ich ... vermag keinen Grund abzusehen, der uns berechtigte, die Möglichkeit [my emphasis] zu bezweifeln, dass das ganze Stück [267-369] einschliesslich jener zehn Verse [333-42] einen integrenden Bestandtheil schon der älteren vom Bearbeiter benutzten Vorlage bildete."

167. The defence takes the following form: the story has a good moral, for Demodocus ὀδύμον ... ἔλεγς ἐν ἀυτοῖς νεκροῖς καὶ τοὺς ἄνδρας λέγοντας "οὐκ ἀρετὴν κακὰ ἔγρα " [329]; Odysseus's subsequent request to Demodocus, ἀλλ' ὅτε ὲ ἐπὶ μετάβηται καὶ εἰποῦ κάλον ἀνέβουν
implies that Demodocus should not sing of adultery among the gods but profitable and praiseworthy actions" [- contrast the actual context of 492!]; Homer was only repeating a story which was already well known; his personal disapproval of adultery is made clear at Od. 1. 47; and it is Demodocus rather than Homer who tells the story: Homer himself believes that Charis, not Aphrodite, is the wife of Hephaestus [II. 8. 382].


169. On Macdonald see also above, n. 80.

170. Cf. EE 54: "If the jesting between Hermes and Apollo offends, then the whole lay of Demodocus is offensive."

171. There is also a subsidiary moral: the brainy tortoise beats the brawny hare (329-32, cf. 308-11).

172. Bolling alleges (EE 239) that "Plato could hardly have failed (Rep. iii. 390) to allude directly to this part of the episode had it stood in the text". Now Plato is here giving some examples of lack of σωφροσύνη in Homer's gods and heroes, and, having described in considerable detail how Zeus was overwhelmed by Hera's charms in II. 14, he goes on to briefly mention the story of Ares and Aphrodite as a similar example: οὐδὲ Ἀρείως τε καὶ Ἀφροδίτης ὑπὸ Ἡρακλείου δεσμὸν ὤλ' ἕτερα τοιαύτα: the underlined phrase clearly gives this example the character of a brief adjunct to the previous one in Plato's scheme, and we thus have no right to expect him to elaborate. One example described fully, the other merely mentioned: such a scheme is still often used today. It would also be futile to argue that if Plato had known the conversation between Apollo and Hermes he would not have adopted this scheme: his aim throughout the section 376C-392C was to give specimens of unedifying passages in Homer and the other poets rather than a comprehensive criticism of Homer, and within the limits imposed by such a plan there was bound to be a certain arbitrariness involved in the selection and treatment of examples.

173. After II. 17. 414-19 and Od. 2. 324-30 and 21. 396-400 the τις εἰπε... speech is not followed by any concluding formula at all but by a line introducing another such speech (e.g. ἄλος ... εἰπε...); after Od. 18. 400-404 there is again no concluding formula - we have instead the formula τοῦτο δὲ καὶ μετέλειφ' to introduce Telemachus's rebuke.


175. For the three passages mentioned at the beginning of n. 173 above see my discussion in Acta Classica 17 (1974) 24 n. 47 and F.M. Combellack, University of California Publications in Classical Philology Vol. 12 No. 4 (1937) 48f.

176. Cf. e.g. Od. 15. 4-7, II. 1. 22-4, 2. 1-2, 12. 108-10, 24. 677-9.

177. For the sake of completeness one should add that one cannot rule out the possibility that one of Bolling's hypotheses correctly defines the limits of the omission but that the omission is nevertheless due to excision; but, as we have seen, the thesis
of excision enables us to accept the statement of the scholia at face value.

178. This passage thus also falls within the ambit of the next Chapter as defined in its title, but it seemed preferable to consider it here.

179. We cannot argue with any confidence that the existence of these variants implicitly increases the testimony in favour of the passage, since the natural explanation of their origin is that one of them is due to a lapse of Plutarch's memory. Plutarch's tendency to quote Homer from memory is strikingly illustrated by his discussion of Question 13 in Quaest. Conv. (741E-743C): his quotation of Hector's words to the Trojans and Achaeans (742E-F) consists of II. 3. 88-91 + 255, the last line being in fact spoken by Idaeus to Priam; in his next quotation (742A) Agamemnon's speech is quoted as 281, 282 (with ἔχειν for the ἔχειν of all our MSS., the latter being obviously correct, as the opposite of 285 ἀπὸ ὀλυμπίας and in any case the more appropriate word), 284, 72, the last line being in fact spoken by Paris to Helen; and his next quotation but one (742C ad fin.) attributes to Zeus a line, 3. 457, which is in fact spoken by Agamemnon.


181. Ibid. 124 n. 1.


183. Die Ilias und Homer (Berlin 1916) 66 n. 2.

184. See also EE 47f., and cf. Monro ad loc.: "This [sc. Plutarch's story] is very improbable, as Aristarchus in such a case would have only marked the lines with the obelus."

185. These deductions are supported by the parallel case of Athenaeus on II. 18. 604/5, which will be discussed in Chapter V.

186. E.g. in (1) for "the vulgate MSS. of his day" one could substitute "his own copy of the Iliad": Plutarch would not have needed to carry out any collation if he knew the lines from a report which quoted them and stated or implied that they were absent from the edition of Aristarchus.

187. For (a) and (b) see Pasquali (above, n. 152) 231; for (c) cf. Van der Valk, TCO 87; and in general cf. Van der Valk, RTSI II 483-5.

188. EE 54; see also the first quotation in n. 155 above.

189. As we have seen (above, n. 142), Susemihl thought that Ἐποιημένης might be corrupt for Ἐποιηγένης, whose edition of Homer is cited elsewhere in the scholia; but the text of the scholium is probably sound, not only because Schol. A and Eustathius agree but because it makes sense to assume that the reference is to the fourth-century B.C. tragedian Sosiphanes: a fragment of his Meleagros is preserved (see A. Nauck, Tragicorum Graecorum Fragmenta [2nd ed. Leipzig 1889] p. 819), and he may well also
have written, among his 72 other plays (see the Suda), a Phoenix — Euripides had written both a Meleagros and a Phoenix — and have followed the version of Euripides in making Phoenix innocent: it may well be significant that Schol. A and Eustathius mention Euripides's Phoenix immediately after naming Sosiphanes as the originator of the emendation: καὶ Εὐριπίδης δὲ ἀναμάθητον εὐσθέγε τὸν Ἡρώα ἐν τῷ Φοῖνικι (Schol. A), i.e. "Euripides, too, ...". R.J. Walker (Addenda Scenica [Paris 1923] pp. 249f., 252) assumes that Sosiphanes wrote a play about Phoenix but misunderstands the scholium.


191. Cf. Van Leeuwen ad loc.: "Bonorum morum ... curam habens", and, for a more detailed discussion, his Enchiridium dictionis epicae (2nd ed. Leiden 1918) 34f. However, he curiously attributes the emendation to Aristodemus without even mentioning Sosiphanes.

192. The similarities between the two passages in language and thought-sequence may well reflect a formulaic pattern common to them both, though on the other hand it is possible that one passage was modelled on the other: in the latter event the internal evidence gives us no indication of which is prior.

193. For divine intervention in similar circumstances one may compare the much more elaborate episode in II. 1. 188-221, where Athene prevents Achilles from killing Agamemnon.

194. Cf. Van der Valk, RTSI II 484f: "... Homer's characters are usually prompted by practical considerations. Now this passage completely answers this mentality, for Phoenix says that he only refrained from killing his father because of the rumours and censures of other people." Van der Valk expresses the same view at TCO 87.


196. Cf. Leaf on (7) ad loc.: "The comparison is to the familiar scene of a homicide exiled from his own land and taking refuge with a chieftain among whose retainers he will enrol himself in return for sustenance and protection."

197. See e.g. A.B. Lord, "Composition by Theme in Homer and Southslavic Epos", TAPA 82 (1951) 71-80, Singer (above, n. 99) 68-90; Kirk, Songs (above, n. 78) 72-80.

198. Cf. n. 196 above.

199. This example is probably late, belonging as it does to a passage which seems designed to prepare the way for the "continuation" of the Odyssey (see e.g. D.L. Page [above, n. 82] 114f.), but it may legitimately be regarded as representative of a tradition with deep roots both in Homeric poetry and in Homeric society, and in any case the Phoenix episode of II. 9 has often itself been regarded as late: see e.g. D.L. Page, History and the Homeric Iliad (Berkeley 1959) 297-300.
200. My discussion of II. 9. 458-61 in relation to the homicide-and-flight theme has been partially anticipated by Van der Valk, RTSI II 484. However, he does not discuss any of the examples of the theme or see it in terms of oral composition. His treatment runs as follows: "Homer has to give a representation in which Phoenix is forced to leave his country and to take refuge with Peleus. To this end he invents a quarrel between Phoenix and his father. In ordinary circumstances we might have expected that Phoenix would have killed his father and fled from home. Homer, however, is loth to present facts which are very offensive. This time he has the more reason to be cautious, because Phoenix is Achilles' preceptor. Therefore he only plans the murder but does not carry it into effect."

201. Three recent translators of the Iliad who have retained 458-61 in their texts have rightly sensed this adversative relationship: E.V. Rieu (Penguin, 1950), R. Lattimore (Chicago 1951) and R. Graves (London 1959) respectively start 462 with "Yet", "But" and "None the less".

202. 456 θεοὶ ...-457 is parenthetic and the use of ἔνθα in 462 (without 458-61) is perfectly legitimate and well within Ebeling's sense I 2) a) β, "interpositis nonnullis referetur ad ea quae paulo sunt remotiora, illo tempore" (H. Ebeling, Lexicon Homericum [Leipzig 1880-85, repr. Hildesheim 1963] Vol. I 418f.). If we ask whether 458-61 have necessarily been inserted in our texts in the right place, the answer is that Plutarch's wording best suits the position after 457 and that the only other possible place for them would be after 463, where their disturbance of the context would manifestly be even greater.

203. In that case the lines could originally have been in the third person, with καλέσαμεν for καλεούμην in 461 and something like καλ μὲν τὸν βούλευσα at the beginning of 458 (καλ μὲν at the beginning of a line immediately followed by a pronoun is common in Homer: e.g. καλ μὲν τὸν II. 6. 27, 23. 174, Od. 10. 13, καλ μὲν οί II. 6. 194, Od. 19. 244, καλ μὲν ἔγω Od. 10. 16); but the parallel passage Od. 9. 299-302, which we have already discussed, is in the first person and starts with the identical first-person phrase, τὸν μὲν ἔγω βούλευσα, and in the posited independent lay the lines could easily have been spoken by Phoenix himself, e.g. on his arrival at the court of Peleus.

204. Schol. T quotes the lines with the introduction τυνὲς ἐπόγουσιν.

205. Wilamowitz, Ilias (above, n. 183) 66 n. 1. Bolling (EE 118) makes Wilamowitz go too far in referring to his "inclination ... to accept the line": Wilamowitz's wording in fact suggests that he regards the line as slightly less likely to be genuine than spurious: "Es ist schwer zu sagen, ob der Dichter seinen Helden mit der eignen Trunkenheit rechnen liess ..., oder erst ein Rhapsode darauf kam. Dass ich das letztere nicht zuversichtlich behaupte, bewirkt die zimperliche Streichung von 458-61, einerlei wer sie vernahm."

206. TCO 85f., RTSI II 458f. However, Van der Valk strangely raises a non-existent difficulty in saying (TCO 86), "It is remarkable that the original text, given by the mss., is represented by Athenaeus
as a conjecture of Aristarch": in reality Athenaeus does not even mention Aristarchus here, and I can only suppose that Van der Valk has become confused with a discussion of some other Homeric passage by either Athenaeus or Plutarch.

207. Schol. T quotes the line: προστιθέασο δὲ καὶ τὸ ἔξετο ... χτίσωνα".


According to a note published 315 was present in some proarithemene texts but absent from the edition of Aristarchus; it is omitted by some of our MSS. Now if Aristarchus found the line in one of his MSS. It is natural to assume, even on the basis of the scholiast alone, that most of these omitted it. Moreover, it follows from our earlier demonstration of Aristarchus's reluctance to omit lines that he would not have omitted this inoffensive line unless it was evident to the vast majority of his MSS. Thus it would appear certain, on various and the external evidence would in itself have provided adequate justification for his omitting it from his text. Further, the line can easily be explained as an instance of concordance interpolation. Yet Van der Valk, however, this is an example of the Aristarchean "existing original lines . . . (through) a misinterpretation of each style . . . in favour of a more common style . . . . It is a clear attempt at shortening the text" (MD 171). I have already adequately refuted this position, but let me add that, as far as "style equals" in constancy, the continuance of Od. 30. 313 is not proved by the fact that the line also occurs after Od. 10. 344 (MD 14). Od. 11. 363 (a similar line with the same second half) and, with the epithets in
CHAPTER V

OMISSIONS WHICH GIVE RISE TO DIFFICULTIES FOR THE HYPOTHESIS THAT ARISTARCHUS DID NOT OMIT GENUINE LINES

Although Van der Valk defends numerous lines weakly attested by the post-Aristarchean tradition, attributing their omission to subjectively-motivated Alexandrian excision, very few of these omissions give rise to any real difficulty for the hypothesis that Aristarchus did not omit genuine lines. The reason for this is that normally Van der Valk's arguments establish no more than that the internal evidence is compatible with a line's authenticity. However, before moving on to consider some omissions which do create difficulties for the hypothesis, I shall discuss, exempli gratia, one of the lines where Van der Valk's case is, as usual, very weak. The example I have chosen, Od. 10. 315, has some relevance to three passages to be discussed later in this Chapter, II. 18. 380f., 426f. and 604-6. I print Od. 10. 314f.:

"Εἴος ὁ δὲ μ' εἴσαγαγόθων ἐπὶ θρόνου ἀργυροκόλου
καλοῦ δανδαλέου· ὑπὸ δὲ θρῆνος ποσιν ἔνν.

According to a Q scholium 315 was present in some pre-Aristarchean texts but absent from the edition of Aristarchus; it is omitted by some of our MSS.¹ Now if Aristarchus found the line in some of his MSS. it is natural to assume, even on the basis of the scholium alone, that most of them omitted it. Moreover, it follows from our earlier demonstration of Aristarchus's reluctance to omit lines that he would not have omitted this inoffensive line unless it was absent from the vast majority of his MSS. Then it would almost certainly be spurious, and the external evidence would in itself have provided adequate justification for his omitting it from his text. Further, the line can easily be explained as an instance of concordance interpolation.² For Van der Valk, however, this is an example of the Alexandrians' "excising original lines ... [through] a misinterpretation of epic style ... in favour of a more concise style. ... It is a clear attempt at shortening the text" (TCO 271). I have already adequately refuted this position; but one may add that, as far as "epic style" is concerned, the genuineness of Od. 10. 315 is not proved by the fact that the line also occurs after Od. 10. 366 (=314), II. 18. 389 (a similar line with the same second half) and, with the epithets in
the accusative, after the somewhat similar Od. 1. 130: the line adds a minor and dispensable detail which the poet was not obliged to insert on every possible occasion. As it happens, we can see a special reason for its absence after Od. 10. 314: the mood here is tense, the narrative terse and rapid, and at this stage Odysseus has no special status in the eyes of Circe, who is anxious to get on with the business of turning him into a pig; this is hardly the moment for the poet (or Odysseus, who knows [317] what is in Circe's mind) to linger over the beauty of the chair or the presence of a footstool - if indeed Odysseus is even given one. However, in the context surrounding Od. 10. 366f. the pace is much more leisurely; the ordeal is over, Odysseus is secure; Circe has been impressed by his immunity to her witchcraft and rightly guessed who he is, and now bestows lavish hospitality on him: the line καλοῦ δνδαλέου κ.τ.λ. (367) occurs towards the end of this elaborately-described hospitality-scene (347-73, minus the interpolated 368-72\(^3\)), where the detail which it adds is much more natural than it would be after 314. In fact Od. 10. 366f., 11. 18. 389f. and Od. 1. 130f. show that it is in contexts of generous hospitality that the line is at home: its absence from the grim travesty of hospitality at Od. 10. 314 is therefore perfectly understandable. On the other hand, given that the poet omitted the line after 314, the temptation to concordance interpolation would be strong.

One may compare the four sequences Od. 1. 331ff., 16. 413ff., 18. 207ff., 21. 61ff. In each passage Penelope makes her way to the megaron. The first line of each passage tells us that she was accompanied by ἀμφικτιόνως (1. 331 = 18. 207). In all four passages we have next, or (in the last passage) soon after, the line ἡ δ' [16. 414 ἀλλ', ν.1. ἡ δ'] δὲ δὴ μνηστήρας ἀφῄκε τὸ θα γυναικῶν. Then, in all four passages, comes the line στῇ ἀρα καρὰ σταθὼν τέγευς πόλις πολιτοῦ. Next, in the first three passages, comes the line ἀντα παρεδάνων σχομένη αὐταρὰ κρήδεμνα; in the fourth passage the line is found in all our minuscules but is omitted by the only extant papyrus which covers the passage, Pap. 28 (Pack 1106, saec. iii-iv p.C.): it is probably an early and particularly successful interpolation.\(^4\) The final line at 1. 335 and 18. 211 is ἀμφικτιόνος δ' ὅμα σοι κεδυν ἐκάτερες παρέστῇ; after 16. 415 this line is absent from all our MSS., and there is no evidence that it ever stood here
in any pre-Aristarchean text; at 21. 66 it is found in the majority of our MSS. but is omitted by a substantial minority and by Pap. 28: it is therefore almost certainly spurious. Thus these four occurrences of a "typical scene" show both that the poet did not feel obliged to make each instance equally elaborate and that there was a temptation to expand the shorter instances by interpolation so as to assimilate them to the longer instances. The parallel with Od. 10. 314f. and its congeners is obvious.

Proceeding now to the omissions which do give rise to problems, I shall start with the puzzling case of II. 23. 804. I print 802-7 (Achilles is speaking):

\[
\begin{align*}
\text{άνδρε ὃδω περὶ τῶνδε κελεύομεν, ὥ περ ἄρβοτω,} & \quad 802 \\
\text{τεῦχεα ἐσσαίνω, ταμεσίχρα χαλκὸν ἐλάντε,} & \quad 803 \\
\text{ἀλλήλων προπάροιθεν ὀμίλου πειρηθήκατ.} & \quad 804 \\
\text{ὀπίσθερὸς κε φήσου ὀρεξάμενος χρῶα καλῶν,} & \quad 805 \\
\text{φασοθ ὅ ἐνόςων ὅδα τ' ἐντετα καὶ μέλαν αἴμα} & \quad 806 \\
\text{τῷ μὲν ἐγὼ ὄσσω τὸδε φάσγανον ἄργυρόθολον} & \quad 807
\end{align*}
\]

804 is omitted by the only papyrus which covers the passage, Pap. 13 (Pack 998, saec. i a.C., post-Aristarchean in type as well as in date) and by a fairly substantial minority of our minuscules, including the two tenth-century MSS., A (in its text) and D. Furthermore, an A scholium excerpted from Nicanor makes it perfectly clear that he did not read the line: he explains 802ff. as follows: συναπτέουσι πάντα μέχρι τοῦ "χαλκὸν ἐλάντε", οὐ λείστοντος τοῦ μάχεσθαι ή τινος τοιοῦτοι. καὶ γὰρ μόνον αὐτῷ τὸ κελέβειν εἴωθεν τιθέναι καθ' ἐαυτὸ ὁ ποιητὴς ... [He cites two Homeric passages, then continues:] καὶ [? μετ' ἄλγον] ἄφ' ἐτέρας ἄρχης ἀναγωστέου "ὀπίσθερος κε φήσου ὀρεξάμενος" ... ἐὰν μὲν γὰρ συνδιπομεν τὸ "ἄνδρε ὃδω, ὀπίσθερος κε φήσου", σολοκλωσίδος γίνεται. \text{7} In 1914, 1916 and 1925 Bolling simply listed the line as an interpolation in the post-Aristarchean tradition. \text{8} In fact, however, this line - or something like it - is absolutely essential to the sense: περὶ τῶνδε, κελεύομεν and the two aorist participles all cry out for an infinitive like πειρηθήκατ. κελεύομεν can be used without an infinitive in certain circumstances, but not here: the alleged parallels cited by Nicanor are illusory; one can add to them, but without effect. \text{9} As Leaf says ad loc., "The line is ... indispensable ..."; likewise Monro ad loc., "it is necessary to the construction of the sentence", Van Leeuwen
ad loc., "Sine vitio ... omitti non potuit ...", C. Hentze, "Gleichwohl ist der Vers ... kaum zu entbehren", W. Aly, the line "ist in der Konstruktion nicht zu entbehren". Thus one cannot accept without qualification Bolling's assertion (EE 12), "For none of the lines I have designated as interpolations can it be maintained that its omission produces an evident lacuna." Van der Valk (RTSI II 495f.) believes that the line is a genuine one excised by Aristarchus for subjective reasons - an instance of his tendency "to make the Homeric text more concise and poignant"; but this explanation is so preposterous that it must be unhesitatingly rejected: it is out of the question that Aristarchus, faced with 804 in all his MSS., should have arbitrarily excised this unobjectionable and indispensable line so as to produce a shorter but nonsensical text; the line is not even a ὀφοροῦμενος, and it is far from ἀπολυτός. How, then, are we to account for its weak attestation in the post-Aristarchean tradition?

The statement by Aly quoted above comes from a review of Bolling's EE, and in reply to this criticism Bolling in 1928 refined his position on the line somewhat. While maintaining that "für diesen Teil des Gedichts liege das schroffe Anakoluth innerhalb des Möglichen", he was at least prepared to consider what would follow if the alternative view - that the line is "unentbehrlich" - were correct. In that case, he conceded, the text of Aristarchus - and of the sixth-century B.C. Athenian text on which Aristarchus's MSS. were based - was at this point bad. After all, he reminds us, the recensio will take us back to this sixth-century text but not beyond it, and there is no guarantee that this text contained the ipsissima verba of Homer. Thus line 804, he says, should be regarded as an emendation - probably a pre-Aristarchean one (since the line is not a ὀφοροῦμενος) which has returned to the text via the scholia - which may have hit on the original text, though Bolling himself would prefer to omit the additional line, and, with Wecklein, read ἔλπησα in line 803 - if any emendation at all is necessary. However, Leaf had already commented in 1902 (ad loc.) that the omission of 804 "would hardly be compensated even if we read ἔλπησα for ἐλπίς... ἔλπησα would be an odd periphrasis for "to compete with each other in swordsmanship for these prizes" - I know of no adequate parallel to support such an expression - although I do not think it would be completely outside the bounds of semantic possibility (and
Leaf's "hardly" implies the same). On the other hand, if we read 804 (with ἔλοντε in 803) the language is thoroughly Homeric: for περὶ ... περρηθήναι cf. Ι. 23. 553 περὶ δ' αὐτῆς περρηθήτω; for the genitive of the person (ἀλλὰ ... περρηθήναι) cf. Ι. 21. 580 περρηθάσατ' Ἀχιλήος; the phrase προκάρακεν ὀμῆλον recurs at ΙΙ. 3. 22, in the same part of the line as here; the form περρηθήναι occurs only at the end of a line in Homer, and ἀλλὰ ... begins three other lines in the ΙΙΙαδ. However, Bolling went out of his way to stress his opinion that line 804 has no diplomatic value - that it should be regarded as no more than an emendation, in spite of its age. This opinion stems from his more general view that none of the Alexandrians' MSS. contained any lines absent from the Peisistratean text but present in and descended from the pre-Peisistratean tradition (or part of it) — see EE 34-7. I have argued in Chapter IV, however, that this view was based on inadequate foundations. But even without the hypothesis of some oral transmission after Peisistratus, it must surely be obvious that any transcriptional blunders present in the original Peisistratean MS. (and its earliest descendants) of such a kind as to produce conspicuous nonsense could have been corrected easily enough by reference to the rhapsodes, and in all probability would have been so corrected. On my own view, of course, it is probable that such accidentally-omitted lines would have returned to the text through other dictations. Anyhow, let us suppose that the original Peisistratean MS. was faulty in lines 803f. because it either (a) omitted a genuine line 804 or (b) read ἔλοντε instead of ἐλέσθαν in 803 (there being no 804). It is inconceivable that such nonsense should have been allowed to stand in the mainstream of the later tradition. Even if we allow for conjectural emendation as well as, or instead of, consultation of the rhapsodes and the influence of other dictations, we are still faced with the following alternatives: either (1) the main (or only) remedy applied was the insertion of a line after 803 - but in this case the line would have been strongly enough attested to be included in Aristarchus's edition;15 or (2) the main (or only) remedy was the substitution of ἐλέσθαν for ἔλοντε, and this was the reading of Aristarchus (without 804) - but in this case we should have expected at least some of our extant MSS. to preserve the infinitive ἐλέσθαν, whereas in fact there is no trace of such a reading in any of our sources, even those which omit 804.16 Moreover, even if Nicanor did not have access to the edition and
knowledge of Aristarchus's readings on the relevant works of
Didymus and Aristonicus, and even allowing for the fact that
these works were selective rather than comprehensive in scope,
it is virtually certain that Nicanor, faced with the illogical text
of 802ff. in his own MS., would have turned to these works for
enlightenment, and fairly likely that one or the other (if not both)
would have dealt with Aristarchus's text of 803-4 had he read ἐλέσσοντο,
omitted 804 and discussed the matter in his ὑπομνήματα: in other
words, Nicanor's citation of 803 with ἐλάντες adds some further weight
to the evidence that Aristarchus did not write an infinitive for the
participle.

I think the facts can best be accounted for by the following
hypothesis. The line is genuine; Aristarchus found it in all his
MSS., and he included it in his edition. Now in Chapter I I put
forward a theory to explain the mechanics of Aristarchus's influence
on the numerus versuum of the post-Aristarchean tradition. My present
hypothesis is that an early would-be verbatim transcript of
Aristarchus's edition accidentally omitted II. 23. 804, and that it
was this transcript, or a copy or descendant of it, which was made
available to the publishers to consult, with the result that initially
the omission was taken over by all or nearly all the texts whose
numerus versuum was based on the edition of Aristarchus. (It is not
necessary to posit an original 804, different from our present line,
which contained a homoiographic temptation to omission: it is clear
both from the papyri and from the mediaeval MSS. that copyists quite
often accidentally omitted single lines without any such temptation.)
The influential MS. in which the omission first occurred could have
been the very first apograph of Aristarchus's edition - the copy
deposited in the Library of the Museum. It may be objected that
in this case the omission would immediately have been queried by
one of Aristarchus's pupils, brought to his notice and corrected;
but this would not necessarily have happened: before Aristarchus's
dition was complete, his pupils would probably have based their study
of Homer on copies of the edition of Aristophanes, as Aristarchus did
himself - in fact his first set of ὑπομνήματα took the form of a
commentary on the edition of Aristophanes (see Pfeiffer 217). When
Aristarchus's own edition of the text was complete, his pupils may
well have been content to continue to use their copies of
Aristophanes's text as their basis, recording the apparently relatively
few places where Aristarchus's *numerus versuum* differed from that of
his predecessor (Aristarchus could have supplied them with a list) and per-
haps altering their own texts accordingly, then copying the critical ομετα from the margin of the Library's copy of Aristarchus's own edition—a process which could easily have failed to show up the omitted 804; attention would have been drawn to Aristarchus's own noteworthy intralinear readings by his διπλα, and his pupils could have consulted his ουμονήματα and made up their own minds about such readings: a slavish word-for-word collation of their own texts with the edition of Aristarchus may well have struck them as quite unnecessary. Moreover, the error in the Library's copy of Aristarchus's edition could easily have remained undetected by the pupil(s) who, after his death, prepared the revised edition incorporating the new readings contained in his latest set of ουμονήματα, since this task would simply have entailed reproducing the first edition with the appropriate alterations, attention being paid only to those sections of the text to which the new ουμονήματα drew attention, and the basis for the second edition would probably have been the Library's official copy of the first edition, since Aristarchus would probably have taken his own copy with him when he fled to Cyprus.

Alternatively, 804 could have been present in the Library's copy or copies of Aristarchus's first edition but accidentally omitted from the second edition, the production of which, as we have just seen, would have required the mechanical copying, without any alteration, of substantial sections of the first edition. We happen to know that it was this second edition, not the first, on which the publishers based their popular editions, because we know from the scholia that the first edition omitted 11. 16. 613 whereas the second contained it, and it is contained also by all our MSS. and by Pap. 228 (Pack 937, saec. iii-iv p.C.). Since it was apparently Aristarchus's pupils who themselves prepared this edition, on the basis of his most recent ουμονήματα, they would not have needed to consult it for the purposes of revising or annotating their own texts—it would have been deposited in the Library "for the record", for later scholars and other authorized persons to consult. In such a situation it is easy enough to see how the accidental omission of 804 could have escaped the notice of Aristarchus's pupils—at any rate for a considerable time: if the Library's copy of the second
edition was eventually corrected, this may well have been done only after the initial and decisive contact between the publishers and the Museum had already taken place and the "Aristarchean" edition had been well and truly launched on the market.

We cannot hope to identify the influential transcript in which the omission first occurred with certainty, but I think it likely that it was one or other of the two candidates I have suggested.

The return of the line to the post-Aristarchean tradition could be explained in various ways: e.g. at the stage when the "eccentric" texts were being corrected some commercial copyist may have been alert enough to notice that the apparently Aristarchean omission of 804 produced nonsense and independent enough to refrain from deleting the line; or by the time 804 had already disappeared from most of the current texts - i.e. from all the "Aristarchean" texts - someone who was puzzled by the resultant defective text of 802ff. and deduced that there was probably a line missing after 803 may have retrieved 804 from a text of the pre-Aristarchean type and inserted it in his own copy; or one publisher may have made his collation with the Library's copy after the error had been detected and corrected by the Museum. Once the line had returned to a small section of the post-Aristarchean tradition the normal process of contamination would have been reinforced by a specific desire, arising from puzzlement, to consult other manuscripts' versions of 802ff.

Finally, one must stress that there is no need whatever to suppose that Nicanor's interpretation of the passage was based, either directly or indirectly, on a comment in Aristarchus's ὑπομνήματα: though Nicanor often followed the opinions of Aristarchus, his work on the punctuation of Homer was not simply a rehash, selection or elaboration of interpretations offered by Aristarchus: its subject-matter was the punctuation of Homer itself, not the opinions of Aristarchus on this question, as both the title, preserved by the Suda,23 and the extant fragments indicate: in this respect it was thus in quite a different class from Didymus's Περὶ τῆς Ἀρισταρχείου ὄνοματος and Aristonicus's Περὶ σημείων Ἰλιακῶν καὶ Ὀδυσσείας, which both aimed explicitly at dealing with the opinions of Aristarchus. Moreover, Nicanor's independence is illustrated by the fact that his views quite often disagree with those of Aristarchus.24
Are there any other lines whose omission by our MSS. and papyri can plausibly be attributed to the same cause, viz. accidental omission from an influential transcript of the edition of Aristarchus? At the outset one needs to make the general point that the Library's own transcripts would surely have been made with more than usual care, and indeed if any other would-be verbatim copy or copies of the whole of Aristarchus's edition of Homer were made it is reasonable to suppose that whoever commissioned it or them, obviously out of esteem for the learning and authority of the great critic, would have done all in his power to secure accuracy in the copying - otherwise the whole object of the exercise would have been endangered. It thus seems likely that only a very few lines would have been accidentally omitted from the influential transcript(s). It follows that the plausibility of the hypothesis of this particular kind of omission as a way of accounting for the behaviour of our MSS. in the case of any given line will be in inverse proportion to the plausibility of alternative explanations. In practice this means that we shall need to ask ourselves: (1) Is the line essential to the sense? (2) Is there a homoiographic temptation to omission? (3) Is the line a non-δημοφωνήμενος? Where the answer to (1) is unmistakably "Yes", there will be no real need to put the other questions (except in order to secure possible confirmation); but where the answer to (1) is "No" or "Non liquet" the other questions will become important.

Let us start by considering Od. 4. 432; I print 431-4 (Menelaus is speaking):

\[\begin{array}{c}
\text{\textit{Hμος ὁ ἤρωγενεα φάνη ῥοδοδάκτυλος Ἕλδας,}} \\
\text{καὶ τὸτε δὴ παρὰ ἕτων θελᾶσσε κύρικοροι}} \\
\text{Ἡπα πολλὰ θεοῦς γονοδήμενος οὐτὰρ ἑταίρους}} \\
\text{τρεῖς ἄγον, ὃς μάλλον τεπολύει πάσαν ἐκ Ἀδεν. 434}
\end{array}\]

There are still no papyri covering the passage, but 432 is omitted by seven of Allen's minuscule families, including our two oldest Odyssean MSS., L4 and L8. As far as I know, however, no editor has omitted or even bracketed the line, presumably because all have felt the sense would be awkward without it. Bolling, however, regards it as a certain interpolation. Van der Valk (TOC 271) attributes the omission to an arbitrary shortening of the text by Aristarchus, and comments, "The omission of such a line, however, is completely at variance with the formular, epic style of the Odyssey." He presumably
means that in a sequence like this 431 must be followed by a line beginning with a resumptive temporal formula such as καὶ τότε ὅτι – but this is disproved by the parallel passage Od. 4. 570-76 (425f. = 570f.; 427-31 = 572-6), which is followed by no such formula. The real objection to the omission of 432 is the rather strange absolute use of ἡ γὰρ in 433 which would result. Without 432, the only meaning ἡ γὰρ could have here would be "I set out". Now one would naturally expect a particular destination to be implied with what I would call this "forward-looking" absolute use of εἰμι (to be distinguished from the predominantly "backward-looking" use – inapplicable at Od. 4. 433 – where the emphasis is on the place left and where the meaning comes closer to "leave" than to "set out"): cf. e.g. Od. 17. 190, 194, ἀλλ' ἔκμεν, where the destination has been clearly expressed in 185 πόλινδρ' ἐκνα. But the difficulty with Od. 4. 433 (if we omit 432) is that at the moment when ἡ γὰρ is used the destination aimed at remains unclear: Eidothea has said she will lead Menelaus to Proteus's siesta-spot (407), but he actually sets out before there has been any mention of Eidothea. Unless we are content to say simply that the poet has not fully visualized the scene, I suppose we can assume that Menelaus immediately sees Eidothea in the distance, or that he wanders uncertainly till he catches sight of her, or that her mention of ὑπερ' ὀκταοθόων in 403 is sufficient to mark the general area, towards which he proceeds until he catches sight of her; but in any case the text (without 432) is disconcertingly unexplicit. The addition of our 432 would do more to suggest Odysseus's course, and the sense of ἡ γὰρ, construed now with παρὰ δὲνα θαλάσσας, would alter sufficiently to remove the syntactical demand for a clearly-understood destination; and I think it would then become easier to supply the necessary logical bridge (such as Menelaus catching sight of Eidothea in the distance) between ἡ γὰρ in 433 and μᾶλα σχέδον ἡλάθομεν αὐτῆς in 439, since παρὰ δὲνα θαλάσσας ... ἡ γὰρ in 432f. would be taken up by εὖνας δ' ἐν φανάραοι, ὀνέγαθαρ' ἄλλην | ἢτο μένουν' in 438f. – though some slight vagueness would still remain. One might suggest simply that 432, in the form in which we know it, may have been accidentally omitted from the influential transcript of Aristarchus's edition, in spite of the absence of any homoiographic temptation; but I am inclined to prefer a more complex hypothesis which would both introduce such a temptation and allow for the possibility that 432 originally lent
greater clarity to the sequence of events. This hypothesis is as follows. After 431 there originally stood a line beginning with τήμος - perhaps something like τήμος δὲ μετὰ Ἑλλοθεν παρὰ θεῶν θαλάσσης; the homoioarchon 431 ἦμος, 432 τήμος led to the accidental omission of the line from an influential transcript of Aristarchus's edition; and this time the original line was never recovered: the lacuna would have been less conspicuous than that after Il. 23. 803, and by the time someone became convinced that the text was defective there may no longer have been any extant copies of the pre-Aristarchean type with the original version. However, the interpolator's feeling that the text needed expanding was, for once, right. The lines Il. 24. 788f. provide us with a parallel both for the sequence ἦμος δ' ἡμείνεια φάνη βοθοδόκιτυλος 'Ηώς, | τήμος ... and for the accidental omission of the second line: Il. 24. 788 ἦμος ... 'Ηώς = Ὀδ. 4. 431, Il. 24. 789 begins τήμος ἀρ' ... and is accidentally omitted by τ as a result of the homoioarchon (it is read by all our other minuscules and by Pap. 14 [Pack 1013, saec. ii p.C.]). This particular ἦμος-formula is not followed by τήμος elsewhere in the Odyssey. However, (1) the poet has just used an ἦμος-clause, again marking the time of day, followed by a τήμος-clause at Ὀδ. 4. 400f., and this may have inclined him to use a similar sequence here. (2) If the poet had wanted to start the second foot of Ὀδ. 4. 432 with a word beginning with a vowel, the τήμος ἀρ' form of Il. 24. 789 would have been metrically convenient and, presumably, normal: a similar metrical situation does not occur among the remaining seven instances in the Odyssey, and one in the Iliad, where the formula ἦμος δ' ἡμείνεια κ.τ.λ. is immediately followed by a resumptive temporal expression, but cf. Ὀδ. 4. 400f. ἦμος ..., | τήμος ἀρ' ἐξ ..., Il. 7. 433f. ἦμος ..., | τήμος ἀρ' ἀμφ' ..., where in both cases the ἦμος-clause marks the time of day. A possible alternative to τήμος ἀρ' would be καὶ τότ', but this phrase, though it is used after ἀλλ' ἀτε (see e.g. Il. 24. 31f.), occurs nowhere in Homer after ἦμος. (3) We find τήμος ἀτ' after clauses marking the time of day at Ὀδ. 12. 439-41 and 13. 93-5: in the former passage the clause is introduced by ἦμος.

Though a text without a line at Ὀδ. 4. 432 would be awkward, I am not convinced that it would be impossible, and so I am not prepared to rule out the possibility that the prima facie implication of the external evidence - viz. that 432 is a post Aristarchean interpolation whose removal leaves us with the version of the Odyssey-poet himself -
is in fact the correct one. However, I think it more likely than not that we have to do with an accidental omission here.

We can now move on to II. 18. 381; I print 380-82:

\[\delta\varphi\prime\, \delta\, \gamma\tau\dot{a}t\prime\, \acute{\epsilon}k\nu\nu\varepsilon\tau\mathrm{t}\omega\, \lambda\omicron\upsilon\nu\lambda\eta\varsigma\, \tau\rho\acute{a}\nu\varsigma\, \omicron\upsilon\, \dot{\gamma}\gamma\dot{\theta}\dot{\theta}\varepsilon\nu\varsigma\, \acute{\varepsilon}\, \dot{\theta}\acute{e}\varsigma\, \dot{\gamma}\rho\upsilon\dot{r}[

\text{381 is omitted by Pap. 11 (Pack 953, saec. i-ii p.C.) and a small but fairly weighty minority of our minuscules, including the tenth-century A (in its text). It was probably also omitted by Pap. 6 (Pack 952, saec. ii-iv p.C.)}, which is lacunose at this point, but has the stichometric marks \(\delta\) opposite line 100 and \(\varepsilon\) opposite line 505, which strongly suggests that the five lines between these two points listed by Bolling (EE 21) as post-Aristarchean interpolations, including 381, were absent from its text. Further, the line has been added in the margin of A with the comment \(\epsilon\nu\, \acute{\alpha}l\lambda\nu\varsigma\, \kappa\acute{a}i\, \circ\omicron\upsilon\omicron\upsilon\varsigma\, \epsilon\upsilon\phi\nu\varsigma\cdot\ \acute{\alpha}p\acute{e}\varsigma\tau\rho\alpha\tau\varepsilon\pi\tau\alpha\delta\varsigma\), which suggests that it was found in a MS. which itself showed evidence of collation with a MS. which lacked it.

The question to be decided is: Is the omission of 381 accidental - a result of the homoiarchon \(\delta\varphi\prime\), \(\tau\delta\varphi\alpha\) - or is it an indication that the line is a post-Aristarchean interpolation? Bolling at first regarded it as "absolutely necessary" to the sense and its omission as accidental, but later (EE 12, 21, ALI 18f.) came to agree with P. Cauer and Wecklein (p. 18) that "the removal of \(\Sigma\) 381 does not reduce the text to nonsense" (ALI 18). I sympathize with Bolling's change of mind, because I have undergone the same process myself. The problem is that, once one has read the text of the passage in the printed vulgate, the presence of 381 tends to mesmerize one into accepting it as indispensable, because without it both syntax and sense have to be taken very differently. With the printed vulgate, we have a sequence precisely parallel to that at the beginning of Book 18: the \(\delta\varsigma\) of \(\omicron\upsilon\, \mu\dot{e}n\) transition (1, cf. 368), the approach or arrival (2, cf. 369-71), the \(\tau\omicron\upsilon\, \acute{\delta}\, \acute{\varepsilon}\phi\varsigma\) line (18. 3, cf. 372), the preoccupation of the person approached (3-14, cf. 372-9), the "while" line summarizing this preoccupation (15 \(\dot{\kappa}\omicron\upsilon\varsigma\, \dot{\omicron}\tau\dot{a}d\theta\prime\, \omicron\upsilon\mu\mu\mu\alpha\nu\varsigma\), 380 \(\delta\varphi\prime\, \delta\, \gamma\tau\dot{a}t\prime\, \acute{\epsilon}k\nu\nu\varepsilon\tau\mathrm{t}\omega\)), then the line summarizing the approach, in each case \(\tau\delta\varphi\alpha\, \omicron\upsilon\, \dot{\gamma}\gamma\dot{\theta}\dot{\theta}\varepsilon\nu\varsigma\) plus the subject (16 and 381). Moreover, a similar sequence occurs at the beginning of Od. 4 - though it concludes with the paratactic \(\delta\varsigma\) of
μὲν ... (15), τῶ δ' αὖτ' ... (20)\(^3\) instead of the hypotactic ἄφρο/ὕς ... τόφρα. The parallelism - especially that between the two passages in Book 18 - is so precise, elaborate and striking that it could be argued that it tends to favour the authenticity of 381. On the other hand, if the line was originally lacking it is easy to see how its accession could have been due to concordance interpolation, reinforced (1) by the fact that ἄφρα in contexts like this is usually followed by τόφρα and perhaps (2) by a temptation to assume a lacuna after 380 through a misinterpretation of the δ€ in 382 (of which more anon); and the second half of 381 is a common formula for Thetis which would not have taxed even a post-Aristarchean interpolator's ingenuity. Moreover, the existence of the parallels should not mislead us into thinking that such a summary at the end of an approach- or arrival-sequence of this type is normal: on the contrary, it is the exception rather than the rule. Admittedly this is largely because in such arrival-scenes the section describing the preoccupation of the person(s) approached is normally shorter (e.g. II. 3. 125-8, Od. 7. 136-8) than in the three cases we have just examined; but in the arrival-scene Od. 1. 102ff. we have six and a third lines (106-12) devoted to the activities of the suitors and their hangers-on, and yet they are followed by no such summary. (This compares with seven and five-sixths lines devoted to the activities of Hephaestus, II. 18. 372-9.) Of course at II. 18. 380 we do have the activities of Hephaestus summarized: what is lacking, without 381, is a further explicit reference to the approach of Thetis. But once we have rid ourselves of any notion that a double summary, for its own sake, is de rigueur in such an arrival-scene, we can begin to entertain the possibility that in 380-83 the poet was aiming to do something other than merely recapitulate his arrival-scene: why should he not (with 381 omitted) have been trying to explain why Hephaestus did not notice Thetis's arrival? - "Hephaestus was preoccupied in his workshop - it was his wife, Charis, who noticed Thetis and came forward to greet her." In fact once we start to consider this interpretation, 381, with its οἱ ἐγγόνεν ξάθε, begins to look distinctly odd: can Thetis really be said to "come near to Hephaestus" when it is clear from the subsequent narrative (382-422) that at this point she must still be a considerable distance from him? This was a difficulty which worried Ameis-Hentze (even though they felt the line to be necessary)\(^3\) and which led Wecklein
(p. 18) and Cauer\textsuperscript{37} to reject the line; as in the case of Od.
10. 314, discussed at the outset of this Chapter, we can see a
special reason for the poet to vary a familiar pattern by omitting the
final element. The presence of δ€ (382) in a temporal apodosis should
cause no difficulty: δ€ after a temporal protasis "is by far the
commonest form of apodotic δ€ in Homer, occurring more than seventy
times" (J.D. Denniston).\textsuperscript{38} It is at best misleading to claim, as
Leaf does ad loc., that "there appears to be no other case of ὕφα, in the purely temporal sense, succeeded by δ€ in apodosi", since it is
followed by ὕφα δ(€) at II. 4. 220f., 15. 343, Od. 9. 56f., 10. 125f.
Moreover, there are exact syntactical parallels where ἦς is followed
by a simple δ': II. 1. 193f., Od. 4. 120-22 (without Bently's
needless deletion of the δ', strangely followed by Allen), Od.
5. 365f.: in all three places the semantic, verbal and contextual
parallels to the sequence II. 18. 380, 382 should also be noted.
Finally, Leaf himself cites four other places in the Iliad (5. 788-90,
9. 352f., 18. 61f., 442f.) where a temporal ὕφα of this type is not
followed by ὕφα or any other demonstrative adverb of time. The
simple pronoun τήν in 382, when Thetis was last mentioned at 372, is
perfectly acceptable in spite of the interval: compare the use of τήν in Od. 1. 113 in a very similar situation.\textsuperscript{39}

The line, then, is not essential to the sense or syntax; but is
it positively intrusive? That depends on how much we make of the
ἐγγὺς ἐν difficulty already mentioned. I do not think this objection
can be pressed too hard, especially in the light of the parallel
passage 18. Iff. (which must have been fresh in the poet's mind while
he was composing 368ff.) and τὸν δ' εἰρ' in 372 (though ὅτι ἐγγὺς ἐν
ἤλθε does go further than this). We could perhaps see 381 as an
instance of the poet's being betrayed by a familiar pattern into a
slight illogicality.

However, even if the internal evidence does not prove the line
to be impossible, it still shows it to be dispensable, and that is
an important point, because in itself it encourages us to take the
external evidence at its face value. It is mainly the homoiarchon
which must make us hesitate. We have just seen how a very similar
homoiarchon (ἤμος, τῆμος) led the scribe of T to omit II. 24. 789.
Thus the hypothesis that II. 18. 381 is genuine and was accidentally
omitted from an influential transcript of Aristarchus's edition is
not totally implausible. However, in the light of the line's
dispensability and the εγγυότευν difficulty, I think it is far more
likely to be a post-Aristarchean interpolation. But even if the
ultimate verdict must remain non liquet, what we clearly cannot do is
to attribute the omission to arbitrary excision by Aristarchus, as
Van der Valk does (RTSI II 51f.: he suggests that Aristarchus
regarded the line as a superfluous repetition of what had already
been said in 372): if Aristarchus knew the line at all, he would not
have omitted it without very strong manuscript evidence against it.
(I shall be returning to this line on pp. 152-4 below.)

II. 18. 427 is a very similar case; I print 426f. (Hephaestus
is addressing Thetis):

αὖδα δὲ τῷ προνέμε, τελέσαι δὲ μὲ θυμὸς ἄνωγεν 426
εἰ δύναμαι τελέσαι γε καὶ εἰ τετελεσμένον ἔστεν. 427

427 was probably omitted by Pap. 6 (Pack 952, saec. ii-iv p.C.)
and is certainly omitted by Pap. 9 (saec. vi p.C.), Pap. 11 (Pack 953,
saec. i-ii p.C) and ten of Allen's minuscules. Bolling listed
the line as a post-Aristarchean interpolation, noting that it = II. 14. 196, but Aly, while conceding that it was syntactically
dispensable, argued that without it the greeting of Hephaestus would
be lame (I do not know whether the pun was intended), that the
previous line (II. 18. 426) was also taken over from Book 14 (= 14. 195) and that one could see no reason why the poet of Book 18
should not have taken over the second line with it. Bolling's main
reply to this was a vivid re-presentation of the external evidence;
his enigmatically brief comments on the internal evidence seem to
mean "Yes, 18. 427 would improve the text, Hephaestus's greeting does
end abruptly without it - but can we expect anything better from the
author of the Ὀπλοποιεύα, which is after all a late component of the
Iliad?" In my view, however, this reply (if my exegesis is correct)
concedes far too much. If A expresses a willingness to do B an
unspecified favour which B may request, the willingness may be
expressed either conditionally or unconditionally. A conditional
expression would be the more cautious statement, an unconditional
expression the more friendly and generous. The couplet II. 18. 426f.
(= II. 14. 195f., Od.. 5. 89f.) has a formulaic ring about it, but
nothing like it occurs anywhere in Homer outside these three places, so
that we do not have enough evidence to insist that the first line cannot
occur without the second: it may well be that there was a longer
formula and a shorter one, and that the context determined which was used in any given case. Alternatively, we could acknowledge that the only true formula was the two-line version, but defend the poet's right to curtail it in a special case by omitting the second line. In any event, we can easily see what could have led the poet to choose the longer form at II. 14. 195f. and Od. 5. 89f. and the shorter form at II. 18. 426. In II. 14 Aphrodite has every reason to be cautious in her reply to Hera, since, as Hera has just pointed out, the two goddesses support different sides in the war. Likewise in Od. 5 Calypso has reason for caution, since she is suspicious of the Olympians and launches a bitter attack on them (116-29) as soon as Hermes has spoken; moreover, εἰ τετελεσμένον ἔστιν (90, i.e. "if it is a thing capable of accomplishment") has a special relevance in the light of Calypso's subsequent πέμψε μὴ μοὸ τῇ ἔγγυς γε. | σὺ γάρ μοι πάμα νήσες κ.τ.λ. (140-42). In II. 18, on the other hand, the situation is quite different: Thetis saved Hephaestus when he was thrown out of heaven, and the very mention of her arrival stirs in him profound feelings of gratitude effusively expressed (394-408) and a strong desire to repay the debt of kindness in full: 406f. τῷ μὲ μάλα χρεία | πάντα θέλει καλλικλοχάω τρικύρτω τίνειν. These are strong words, and after them a simple 426 (without 427) - "Tell me what you have in mind - I am eager to do it" - would well reflect the god's enthusiastically generous mood (which is carried over into his reply, 463-7, to Thetis's request). The addition of the cautious qualification of 427 would arguably sort ill with the spontaneity, emotion and abandoned generosity of his attitude. While one certainly cannot go so far as to claim that the context renders the longer text impossible, there are strong arguments (as in the case of Od. 10. 314) for the aesthetic superiority of the shorter text; and of course if the second line was originally lacking the parallel passages would have provided a strong temptation to concordance interpolation. Once again the main reason for hesitating is a homoiographon - this time a homoiomeson, the word τελέσω in both 426 and 427. This would give the hypothesis that 427 was accidentally omitted from an influential transcript of Aristarchus's edition a certain degree of plausibility, but my own assessment of the internal evidence leads me to regard the prima facie interpretation of the external evidence - that 427 is a post-Aristarchean interpolation - as far more likely.
II. 18. 441 is read by Pap. 9 (saec. vi p.C.) and all our minuscules except Ge (saec. xiii) but is omitted by Pap. 11 (Pack 953, saec. i-ii p.C.), Pap. 239 (Pack 958, saec. ii-iii p.C.) and probably Pap. 6 (Pack 952, saec. ii-iv p.C.).

There is virtually no homoiographic temptation to omission: the homoiomesa 440 ... μαχησθησθαι τον ... , 441 ... νοσθησαντα δομον ... are very slight, and the homoioteleuton 439 Ἕλεω, 441 Πηλήσων εἶσω would be most unlikely to lead to the omission of 441 alone. Thus the evidence points very strongly to the conclusion that the line was absent from the edition of Aristarchus but interpolated in the post-Aristarchean tradition at a stage early enough to enable it to capture a place in Pap. 9 and all but one of our minuscules. It will then be most natural to interpret the scholium in A (one of Ludwich's "Textscholien") ἐν τοις οὐ κεῖται as a report of a post-Aristarchean collation, and if we are looking for a sign of lateness in its wording we may perhaps point to the use of κεῖσθαι where εἶναι or φέρεσθαι would be normal; compare the Byzantine collation and the use of κεῖσθαι in conjunction with it in the A "Randscholion" on the post-Aristarchean interpolation II. 20. 447, ἐν ἀλλοις ὁ στίχος οὗτος οὐ κεῖται. However, it is possible that Didymus, finding II. 18. 441 present in some of the MSS. of his own day, consulted the editions of the three great Alexandrians and then wrote something like οὗτε Ἀριστοφάνης οὗτε Ἄρσταρχος οὗτε Ζηνόδοτος ἐκείσταντας τοῦ τῶν στίχων (= Schol. H on Od. 8. 142), and that the epitomator abbreviated this to ἐν τοῖς ό στίχων: the fact that this is an A οὐκ οἷς Σχολ. H on Od. 11. 525 or ἐν ταῖς Ἀριστοχέων οὔτε φέρεται (= the other H scholium on Od. 8. 142). But either hypothesis would be compatible with the supposition that the line was absent from the edition of Aristarchus.

However, although Bolling lists the line as a post-Aristarchean interpolation, I know of no editor (except Bolling himself) who omits or even brackets it, and we shall now need to consider the case for a radically different hypothesis - that the internal evidence shows the line to be genuine, that it was present in the edition of
Aristarchus and in all earlier texts, and that the behaviour of the post-Aristarchean tradition is due to the accidental omission of the line from an influential transcript of Aristarchus's edition. An argument for the line's authenticity would take the following form: in 11. 18. 437-43 Thetis is repeating to Hephaestus what she said earlier in this Book to her fellow-Nereids; in fact 437-43 = 56-62 verbatim; and the omission of 441 (=60) would thus be extraordinary, the more so since besides 440f. (=59f.) ... τὸν δ' οἷς ὑποδέξομαι ἄτις | ὦ καθός νοστήσαντα δόμον Πηλάιον εὕσω we have 89f. (Achilles to Thetis) τὸν δ' οἷς ὑποδέξειαι ἄτις | ὦ καθός νοστήσαντ' and Od. 19. 257f. (Penelope speaking of Odysseus) τὸν δ' οἷς ὑποδέξομαι ἄτις | ὦ καθός νοστήσαντα φίλην ἐς πατρίδα γαῖαν. Prima facie this argument may seem persuasive, but on closer consideration it will turn out to be very shaky. E. Pfudel has shown that where there is repetition of a fairly extended passage which is not itself a formula, the passage is often not repeated in full but abbreviated by the omission of a line or lines. For example (Pfudel pp. 12f.), the proposal which Paris has put to Hector in 11. 3. 68-75 is repeated by Hector to the assembled Trojans and Achaeans in 88-94, but while 96f. = 90f. and 71-3 = 92-4, 74f. are omitted from the repetition. Pfudel says (p. 34) that such abbreviations "haben ihren ganz natürlichen Grund in der sehr begreiflichen Besorgnis des Dichters, durch allzu weit ausgesponnene Wiederholungen ... die Geduld des Hörers zu ermüden". While Pfudel probably underestimates the poet's and his audience's tolerance of repetitions, I think his explanation is on the right track for some examples, including the one I have just cited, though I would see the poet's motivation as probably rather less conscious than, apparently, Pfudel does. On the other hand there is sometimes another factor at work, a factor which Pfudel has completely ignored, viz. the effect of differences between the context of the earlier passage and that of its later echo. Thus, at the most obvious level, Helenus's suggestion to Hector at 6. 90-97 is repeated by Hector to Hecuba virtually verbatim at 271-8, but the fact that Hector does not go on to repeat 98-101 as well hardly amounts to a "Verkürzung" (as Pfudel calls it on pp. 11 and 34) at all, since the content and wording of these four concluding lines strongly suggest that they do not, or at any rate do not necessarily, form part of the "message" which Helenus wants Hector to pass on to Hecuba. At a more subtle level, Circe's instructions to Odysseus on
the procedure he must adopt if he wants to listen to the Sirens' song (Od. 12. 49-54) are retailed by Odysseus to his companions (160-64) with a good deal of virtually verbatim repetition, but, as Pfudel has pointed out (p. 14), there is nothing in the second passage corresponding to line 52 in the first, δίστα κε τερπόμενος ἄκοδος Σελήνου. This could easily have been turned into the first person and inserted after 162: why did the poet not do so? Perhaps this is merely abbreviation for the sake of abbreviation; but it is more likely, I suggest, that the line was omitted because of a subtle change of emphasis in Odysseus's version of Circe's instructions. Circe had made it quite clear to Odysseus that there was no obligation on him to listen to the Sirens' song: on the contrary, it was simply an option which he was free to choose (ἄκοδος καὶ κα' ἑλελησθα, 49) for the sake of the pleasure involved (τερπόμενος, 52) provided he took the proper precautions. However, he says to his companions, "οἶον ἐμὶ ἡμῖν δι' ἄκοδος." To be fair to Odysseus, this may be simply a short cut for "She forbade you to listen but allowed me to", but the natural meaning is that Odysseus has as firm an instruction to listen as the companions have not to listen; and if Odysseus is here deliberately disguising indulgence as duty - perhaps in order to prevent further jealousy (cf. 10. 34-45) and disaffection (cf. 10. 429-37) among his companions - he has a special reason for not saying "δίστα κε τερπόμενος δι' ἄκοδος Σελήνου." Finally, I choose an example which corresponds to the two speeches of Thetis under consideration in that once again we have A repeating to C a speech which A has earlier delivered to B. In Π. 14. 200-210 Hera tells Aphrodite that she intends to visit Oceanos and Tethys in order to restore amity between them; in repeating this to Zeus (200 = 301, 201f. = 302f., 205-7 = 304-6) Hera omits 203f. and 208-10. Both omissions could simply be cases of abbreviation for its own sake; but more probably the poet omitted 203f. because he thought it would be incongruous for Hera to repeat to Zeus this information about his own past behaviour, even if the wording were adapted to the second person; and while 208-10 would not be out of place if addressed to Zeus, the lines seem to have a special function in the speech to Aphrodite which they would lack when transferred to the speech to Zeus, viz. to provide a concluding appeal, complementing the opening appeal (198f.), for the φιλάτητα καὶ ἠμερον which Hera wants Aphrodite to give her. To return to the omission of Π. 18. 441: one
might choose to explain this as abbreviation for its own sake, and one should point out that 441 is not essential to the sense, as the meaning of τὸν ἀ' ϑύχ ὑποδέξωμαι ἀδικες in 440 is adequately defined by the preceding τὸν μὲν ἐγὼ ... | νησιν ἐπιπροέηκε κορωνοῦν Ἰλιοῦν ἔσω (439f.); and one might add that there is a rather similar abbreviation a little later in Thetis's speech: 444f. = 16. 56, 58 (change of pronoun): 57 is omitted where it could easily have been retained with very slight adaptation (δοῦρ ἀ' ἐν κτεῖταισα could have been altered to δοῦρ ἀ' ἔʃ [i.e. ἕʃ] - or ἀ' ἔʃ - κτεῖταισα) - though one could argue that it would be less appropriate in the mouth of Thetis than in that of Achilles. However, I submit that the most probable reason for the omission of 18. 441 is a certain difference in the wider context in Thetis's second speech produced by the accession of some new material near its beginning: in 432-4 she explains to Hephaestus that one reason for her unhappiness is that Zeus forced her to marry a mortal man, Peleus, and that she endured his bed much against her will: ἐκ μὲν μ' ἀλλὰς ἄλλας ἀνδρὶ δώμασεν, καὶ ἔτην ἄνερος εὐνὴν | τολλὰ μᾶλ' ὡκ ἐθέλουσα: these are strong words, and after she has expressed such extreme distaste for Peleus we can understand that to explicitly name Peleus's house as the destination which Achilles will fail to reach and the venue of the post-war reunion with her son which will never take place would detract from the pathos which she is seeking to produce. Thus not only is the line dispensable but, once again, there is a good case for the aesthetic superiority of the shorter text; and once again we can see that there would have been a strong temptation to concordance interpolation of that text. For these reasons, and also because there is no real homoiographic temptation to omission, I regard it as almost certain that the prima facie interpretation of the external evidence is the right one and that the line is an interpolation which was absent from the edition of Aristarchus.

Let us next consider II. 13. 316. 315f. run:

οἳ μὲν ἄδην ἐλδοῦν καὶ ἐσσόμενον πολέμου, 315

"Εκτορα Πριαμίδην, εἰ καὶ μάλα καρτερὸς ἔστιν. 316
Line 316 is absent from the text of all but one of the minuscules earlier than the twelfth century and is also omitted by Pap. 10 (Pack 899, saec. i p.C.), Pap. 36 (Pack 906, saec. ii-iii p.C.) and Pap. 60 (Pack 870, saec. iii-iv p.C.). However, a relatively early terminus ante quem for the line's début in the post-Aristarchean tradition is provided by Pap. 435 (Pack 904, saec. v-vi p.C.), published in 1949, which contains it. Bolling lists it as a post-Aristarchean interpolation. Monro ad loc. curtly dismisses it as "not found in good MSS., and ... clearly superfluous"; but the "clearly superfluous" judgment is unjustified: cf. Leaf ad loc.: "Though this line is omitted by five important MSS. and is of a familiar type of interpolation, a couple of explanatory words plus padding, it can ill be spared. The omission of Hector's name would be very strange, as he has never been mentioned since 205." So also A.G. Laird in his review of Bolling's EE: "If 316 goes, who is the μυν in 315? Hector has not been mentioned for more than a hundred lines"; and the same point was made much earlier by A. Pierron in his edition of the Iliad (Paris 1869) ad loc. Is it likely, then, that 316 was accidentally omitted from an influential transcript of Aristarchus's edition? We may note that there is a fairly slight but not completely negligible homoiomeson which could have acted as a temptation: 315 ... μυν ἀδόνι ἐλδόει καὶ ..., 316 Πρωμηχόνι, εἴ καλ ...; and the epexegetic addition of the name would be not un-Homeric in manner. A glance at Allen's apparatus will reveal that, although nearly all our later minuscules contain the line, all but two of them read the unmetrical καλ εἴ for εἴ καλ. This may at first sight appear to suggest that the line is spurious, but this evidence is counterbalanced by the fact that our earliest source for the line, Pap. 435, reads the metrical εἴ καλ: in other words, the numerical preponderance of the unmetrical version in our surviving sources could well be due to a miscopying of the line at a crucial stage during its spread by contaminatio. Thus the hypothesis of accidental omission from the influential transcript of Aristarchus's edition must be judged a real possibility here. On the other hand, I am inclined to believe that the prima facie interpretation of the external evidence - viz. that Homer went straight from 314 to 316 and that 315 is a post-Aristarchean
interpolation - is somewhat more likely to be the right one. It is true that Hector has not been mentioned since line 205, but he has been very much to the fore in the fighting from the beginning of Book 11, and in 13. 39-205 he is not only a very active commander-in-chief but is himself the spearhead of the Trojan advance, while the two Aiantes and Teukros, whom Idomeneus mentions in line 313, are the chief Achaeans involved in the resistance to him. Thus in spite of the long digression of 206-305, Hector may still be so much to the fore in Homer's mind that after the immediately preceding mention of the Aiantes and Teukros in 313, which recalls their (pre-digression) resistance to Hector, Homer may have carelessly referred to him simply as μὲν. A similar use of a personal pronoun without any expressed antecedent occurs in 11. 19. 176 μὴ κοτε τῆς εὐνῆς ἑπιβήμεναλ ἢ δὲ μυγήναι, where τῆς = "her", referring to Briseis; as Leaf comments ad loc., "τῆς [has] no reference, as Briseis has not been named or even remotely alluded to": she was last referred to in 19. 89 as Achilles's γύρας (and before that in 58 as κοῦρης). Line 176, though a διρροῳδές (= 11. 9. 133 and 275), cannot be dismissed as an interpolation, as it is indispensable to its context (19. 175-91) and is presupposed by 19. 250-68. Presumably the line was originally composed for Book 9 (where τῆς does have an antecedent, viz. κοῦρη Briseis in the previous line, 132=274), but this consideration will at best only partially account for its occurrence without an antecedent in 19: the important point to be made is that in a sense Briseis is what the whole reconciliation episode of Book 19 is about, so that the wider context makes the reference in τῆς clear enough. The parallel with 11. 13. 315 need not be laboured.

I now print 11. 21. 71-4, as found in the Wolfian vulgate:

αὐτάρ ὁ τῇ ἐτέρη μὲν ἐλῶν ἐλλίσσετο γοβνών, 71
τῇ δ' ἐτέρη ἔχεν ἐγχος ἀκαχελόνον οὐδὲ μεθέλε, 72
καὶ μὲν φωνῆσας ἔκει περιδεντα προσηνόμα: 73
"γουνοῦμαι σ', Ἀχιλεῦ ..." 74

Didymus (Schol. AT) says of line 73: τὸῦτον προστιθέασε τινες οὗ
φερόμενον ἐν ταῖς Ἀρωταρχο. The line is read by Pap. 9
(saec. vi p. C), Pap. Pack 977 (saec. ii-iii p. C., published in
1957) and all our minuscules except V32 (1304 A.D.). Leaf ad loc.
doubts the reliability of the statement in the scholia that the line
was not read by Aristarchus, and argues that "the beginning of the
speech without it is hardly Homeric"; Van Leeuwen ad loc. claims that "sine vitio omitti vix potest"; and Van der Valk (RTSI II 489-92) argues that although the line was omitted by Aristarchus it is nevertheless genuine: he sees the omission as an excision. However, all three views are misguided. In reality the line is certainly not necessary to its context: that ἔλὼν ἐλλάσσετο γοῦνων constitutes an adequate introduction to a speech is shown by II. 6. 45f. ... λαβὼν ἐλλάσσετο γοῦνων. | "εἰσεύρετ..." and Od. 10. 264-6 (without the interpolated 265, omitted by the vast majority of our MSS. and by Pap 116 [Pack 1087, saec. i p. C.]) αὐτὰρ ὦ γ' ἀμφοτέρου λαβὼν ἐλλάσσετο γοῦνων. | "μή ..." (and cf. II. 11. 130f., 15. 660f., mentioned by Van der Valk loc. cit.); and the intervention of a line between the verb of speaking (II. 21. 71) and the commencement of the speech itself (74) without a further speech-formula (73) can be amply paralleled: cf. e.g. II. 5. 784-7, 9. 252-4, 13. 215-19, 17. 553-6 (minus the post-Aristarchean interpolation 585), 23. 17-19, 24. 723-5, Od. 2. 399-402. Admittedly the intervening material in the examples so far cited, unlike II. 21. 72, does not constitute a co-ordinate main clause; but two such clauses intervene in the case of II. 16. 231-3 εὖχετ' ἐπειτα στὰς μὲσῳ ἔρχετ' , λείβε δὲ οὖν | ὑπανόν εἰςαναλόν. ἄλα δ' οὐ λαδετερηκέραυνον. | "Ζεῦ ..."; and in two further cases, Od. 8. 474-7 and 14. 459-62, the intervening material, though not a co-ordinate main clause, is syntactically complex - in the former case it actually includes two ὦ- clauses. Finally, for the combination of ἔλὼν ἐλλάσσετο γοῦνων (II. 21. 71) and an intervening co-ordinate main clause cf. Od. 10. 481-3 (without the interpolated 48264) γοῦνων ἐλλατάνευσα, ἄλα δὲ μοι ἔκλυνεν σοῦδῆς. | "Ὃ Κῆρη ...", though admittedly the transition there is rather smoother than in the case of II. 21. 71-4 (without 73). The presence of II. 21. 73 in virtually all the extant post-Aristarchean texts is not inconsistent with its absence from the edition of Aristarchus: our earliest extant text, Pap. Pack 977, was written about 350 years after Aristarchus, and, as we shall presently see, other speech-formulae of this type were interpolated soon after Aristarchus; if an earlier papyrus were discovered, it might well substantiate the scholium: compare what has happened in the case of Od. 11. 525.65

We must next consider the relevance of the variants in line 73, for besides καὶ μὲν φωνῆσας we have καὶ μὲν λασθομένος and καὶ ὦ.
I do not propose to rehearse the full evidence for these three readings here: it may be found in Allen's apparatus and in Van der Valk loc. cit. However, I should state that the AT scholium which I have already quoted continues: ἐν ἐξ ἑκ "καὶ ὁ ὀλοφυρδέμενος". Van der Valk attributes this continuation to Didymus too - probably rightly - and then, in arguing against Erbse's view that the line made its début in the post-Aristarchean era, finds a difficulty in the assumption "that in this [short] period [between Aristarchus and Didymus] a line was interpolated and that, moreover, a different version of this line came into existence" (p. 491).

There is some force in this argument; but if we assume, as Van der Valk does, and as I believe to be probable, that the original reading was καὶ μὲν λυσσῆμενος (which is read by the majority of our MSS.), the tautology ἐλλόσειτο (71), λυσσῆμενος (73) would provide a strong motive for an alteration in the Alexandrian era (when variety was prized), as Van der Valk himself admits, and the virtual unanimity of the extant MSS. and papyri in reading the line supports the hypothesis that it made its début in the post-Aristarchean tradition very soon after Aristarchus. ψωνήσας could be explained either as an alternative attempt to eliminate the tautology (so Van der Valk, and also Wecklein [69], who regards the line as an interpolation) or as a simple transcriptional slip caused by the greater familiarity of the much more frequent ψωνήσας in this position: for the latter explanation cf. II. 15. 48, where two late MSS., 09 (saec. xv) and W3 (saec. xiii-xiv), have καὶ μὲν ψωνήσας ἔτηα περδεντα προσῇδα for the remainder's καὶ μὲν ἀμεληδέμενος, and Od. 22. 311, where all our sources read καὶ μὲν λυσσῆμενος ἔτηα ετ. πρ. except Pap. 28 (Pack 1106, saec. iii-iv p.C.), which has καὶ μὲν ψωνήσας. However, in view of the strong temptation to interpolation and the strength of the diplomatic support for the line, it is quite possible that it was interpolated independently in two different forms during the period between Aristarchus and Didymus, once in the form καὶ μὲν λυσσῆμενος and once either in the form καὶ ὁ ὀλοφυρδέμενος or in the form καὶ μὲν ψωνήσας: cf. II. 11. 218a, where the few MSS. which contain the line are evenly divided between προσῇκη κρεζων ἐνοςχεῖων and ἔτηα περδεντα προσῇδα, and perhaps Od. 10. 430 (see Allen ad loc.). Finally, I think a possible interpretation of the scholium is that its τὰνες and τὸσε are pre-Aristarchean; and if Aristarchus had discussed the line in his ὑπομνήματα, its early entry
into the post-Aristarchean tradition would be easily intelligible. However, to concede that the line may have originated before Aristarchus is not to concede that Aristarchus may have omitted it without very strong manuscript evidence against it.

Further, the view that the line is an interpolation is strengthened by a fact already touched on, viz. that many other lines of this type have been interpolated in the post-Aristarchean tradition, some of them soon after Aristarchus. The motive was obviously to introduce an explicit verb of speaking immediately before the commencement of the direct speech. The matter was dealt with at some length by Bolling in 1922. Among the most recent of such interpolations are those with so little manuscript support that they failed to gain admittance even to the edition of Wolf: II. 9. 224a, 12. 162a, 13. 218a, 22. 330a, Od. 10. 225a, 18. 111a. In the same class as these one may place the very weakly attested lines II. 21. 480 and Od. 10. 265. Then there is an intermediate group—interpolations more firmly entrenched in the mediaeval tradition but with strong early minuscule evidence against them: II. 10. 191, 17. 585, Od. 10. 430, 482; II. 17. 585 is also omitted by Pap. 111 (Pack 941, saec. iii p.C.). Bolling somewhat hesitantly put II. 17. 219 in the same category, but the papyrus evidence against the line has expanded since 1922: then it was omitted by the single papyrus covering the passage, Pap. 48 (Pack 925, saec. v p.C.), but since then two more papyri covering the passage have been published, and they too omit the line: Pap. 111 (Pack 941, saec. iii p.C.) and the newly-published Pap. Köln I. 36 (saec. v-vi p.C.). Finally, there is a group of introductory speech-formulae present in all or nearly all our minuscules but omitted by one or more papyri: I have already argued (in Chapter II) that of the six lines in this category listed by Bolling as interpolations one is genuine, another probably genuine and a third of dubious status, but that still leaves three lines against which there is, today, decisive papyrus evidence (such evidence has in each case increased since the publication of Bolling's 1922 article):

(1) II. 3. 389, Τῇ μὲν ἔλεγμέν προσεφωνε ὅτ' Ἀφροδίτῃ, is read by all our minuscules and by Pap. 3 (Pack 634, saec. iii p.C.). Moreover, Bolling's assertion that Pap. Oxy. 542 (= Pap. 166, Pack 701, saec. iii p.C.) "omits" the line is an error: the editors describe
the papyrus as "part of a leaf from a book containing on the recto
the beginnings of [I1.] iii. 371-393 (389 being lost) and on the
verso the ends of 394-418 (413-4 being lost)", and it is evident
from the language they use that the losses are physical lacunae in
the papyrus, not omissions: when they mean an omission they say so
(thus on the same page, in describing No. 544, they say "[I1. 4.]
196-7 omitted"). This interpretation is confirmed by the
consideration that the same mutilation will account for the loss of
both the beginning of 389 on the recto and the ends of 413f. on the
verso. Thus the editors imply that the papyrus has space for 389 and
that its scribe included the line in his text. However, the line is
indeed omitted by Pap. 78 (Pack 698, saec. ii-iii p.C.) and by the
pre-Aristarchean Pap. 41 (Pack 699, saec. iii a.C.). Moreover, in
1954 another papyrus covering the passage was published, Pap. 393
(Pack 702, saec. i-ii p.C.), and it too omits the line. Finally, I
have already discussed Mette's 1956 publication of Pap. 392a
(Pack 696, saec. iii-iv p.C.), and I can only repeat of II. 3. 389
what I said there of 319, viz. that I assume that the papyrus either
reads the line or is not extant at this point. It is roughly true to
say that the two post-Aristarchean papyri which omit the line are
earlier than those which contain it. Moreover, there is no
homoiographic temptation to omission. It follows that the line is
almost certainly a post-Aristarchean interpolation - and this
conclusion receives powerful support from the absence of the line
from our only pre-Aristarchean source, Pap. 41.

(2) II. 13. 480 (= 94), τοῦς ὅ γ' ἐποτρύνων ἔπεια πτερδεντα
προσηδδα, is read by Pap. 60 (Pack 870, saec. iii-iv p.C.), Pap. 9
(saec. vi p.C.) and all our minuscules but is omitted by Pap. 10
(Pack 899, saec. i-p.C.) and by the recently-published Pap. Oxy. 2540
(saec. i-ii p.C.). Thus once again our two earliest post-
Aristarchean papyri omit the line; and once again there is no
homoiographic temptation to omission. The line is therefore almost
certainly a post-Aristarchean interpolation. Presumably, then, the
T scholium ἐν πολλοῖς οὗ φέρεται reflects either simply a post-
Aristarchean collation (so Bolling 79) or else the combination of a
report of such a collation (probably ἐν τούτων οὗ φέρεται) with a
Didymean statement of the type οὕτε Ἀριστοχρός οὕτε Ἀριστοφάνης
οὕτε Ζηνόδωτος ἐκλατυντας τούτων τὸν στίχον. 80
(3) II. 17. 326, τῷ μὲν ἐκεῖσθαι διὸς ἐνδοὺς 'Ἀκάλλων (= II. 16. 720, 20. 82), is present in all our minuscules. In 1922 it was omitted by the only known papyrus covering the passage, Pap. 43 (Pack 945, saec. iii-iv p.C.). Another papyrus covering this passage, Pap. 111 (Pack 941, saec. iii p.C.), was published in 1925, and it too omits the line. Since then the papyrus evidence on the line has not, as far as I know, expanded. Once again there is no homoiographic temptation to omission; and once again this fact and the absence of the line from our two earliest post-Aristarchean sources make it virtually certain that it is a post-Aristarchean interpolation.

These parallels strengthen the case for taking the Didymean scholium on II. 21. 73 at its face value, i.e. for regarding the line as non-Aristarchean and non-Homeric.

On the motive for the interpolation of such lines early in the post-Aristarchean era, Bolling has well demonstrated, with citations from the scholia, that "the absence of such lines seems to have caused trouble, to have required explanation". 81

Finally, although Bolling has conceded that we must "reckon ... with the probability" that the omission of the line by V32 is "merely accidental", 82 I consider it more likely to be due to descent from MSS. which were still free of the interpolation: Allen classifies V32 among his "independent" MSS. (Prolegomena pp. 178-80), and the isolation of the MS. in omitting the line is thus intelligible without the assumption of scribal inadvertence.

The hypothesis that the omission of the line ἐν ταῖς 'Ἀρσοτάρχου (i.e. both editions) is due ultimately to accidental omission from an influential (the first?) transcript of Aristarchus's first edition cannot be completely excluded, but I hope I have succeeded in showing that both the shorter text and the post-Aristarchean interpolation of such lines can be amply paralleled. Given the principle for which I argued at the outset - that "the plausibility of the hypothesis of this particular kind of [accidental] omission ... will be in inverse proportion to the plausibility of alternative explanations" - it follows that this hypothesis must here be judged extremely improbable.

In 1949 Bolling published a study of the use of the word σφρα in Homer which led him to striking conclusions about the text of two
Bolling's textual work has often been criticized for being too rigid and mechanical, and to some extent, as we have seen, this criticism is justified; and I must now make precisely the same criticism of his 1949 syntactical article. When working on the text of Homer, respect for manuscript evidence led him to defy syntax by rejecting I. 23. 804; when working on the syntax of Homer, respect for predominant syntactical patterns led him to defy the manuscript evidence in an astonishing way. A misplaced passion for uniformity in the matter currently in hand led him to diametrically contradictory results. His two extraordinary textual conclusions were as follows:-

(1) He noted that, in the Iliad, when ὅφα (in the sense of "while") introduces a clause whose verb is in the imperfect indicative, the ὅφα-clause normally precedes the clause on which it is syntactically dependent, but that at I. 11. 266 it does not. He deduced (p. 381) that ὅφα here originally began a new sentence and was followed by an apodosis now lost from our vulgate, and he seized on a mutilated Ptolemaic papyrus with numerous plus-verses after line 266 in an attempt to substantiate this position. To this position a good reply has been given by West (95): "... there are other exceptions to this rule in the Iliad ..." the rule clearly does not apply in the Odyssey ... So the sentence structure of the Vulgate, though unusual, should not be suspected." Moreover, West has rightly conceded that a restoration of the papyrus which begins a new sentence after line 266 is speculative (though she herself obviously favours such a restoration). But whatever plus-verses the papyrus contained, there is no reason to believe that any of them belonged to the genuine text: the external evidence against them should be allowed to carry its natural weight, especially as the sequence of the vulgate is entirely logical and gives excellent sense. In 1950, however, Bolling reiterated his position, demanding the insertion of an additional line after 266 beginning with τὁφα (I. Ath. ix., cf. West 94): "Haplography has caused the complete disappearance of the second line from the Alpha [i.e. post-Aristarchean] text." Bolling could not satisfactorily explain how this could have happened; but I have no cause here to introduce my own explanation for dramatically influential accidental omission.  

(2) Bolling also observed that when ὅφα means "while" and the
verb in its own clause is imperfect indicative, it is normally followed by \( \tau \delta \varphi \alpha \). However, at II. 18. 380-82 (which we have already discussed\(^\text{87}\)) this is only true if the weakly-attested 381 is pronounced genuine; and so Bolling proceeded to pronounce it genuine; "I must now correct what I said in my Athetized Lines 18-9 about [II. 18. 380-82]. To omit line 381 would be to break a most usual pattern and to create an unparalleled one. The line is 'genuine', and our effort must be to explain the very unusual disturbance of the tradition."\(^\text{88}\) A year later he included the line in the text of his II. Ath. He had thus come full circle, abandoning in 1949-50 his position of 1925 and 1944 in favour of his position of 1914. However, his allegation that if we include the line the resulting pattern is "unparalleled" must be challenged:-

(a) Bolling himself has mentioned\(^\text{89}\) two passages in the Iliad where \( \delta \varphi \alpha \) in the sense of "while" + an imperfect indicative is followed not by \( \tau \delta \varphi \alpha \) but by a clause beginning with \( \omicron \upmu \), viz. II. 5. 788-90 and 9. 352f. However, it is not self-evident that there is a significant connection between the presence of \( \omicron \upmu \) and the absence of \( \tau \delta \varphi \alpha \), especially as Od. 20. 328-30 runs \( \delta \varphi \alpha \ldots \tau \delta \varphi \alpha ' \omicron \upmu \) (an example to which Bolling himself later draws attention\(^\text{90}\)).

(b) Bolling himself\(^\text{91}\) mentions one case where \( \delta \varphi \alpha \) in the sense of "while" + a present indicative is followed by an apodosis without \( \tau \delta \varphi \alpha \) and without an opening \( \omicron \upmu \), II. 18. 61f.=442f. Does the fact that the verbs here are not imperfect but present reduce the value of the passage as a parallel to II. 18. 380, 382? On the one hand, \( \tau \delta \varphi \alpha \) in Homer is everywhere followed by a verb whose time-reference is either past or future, so that it could be argued that the present tenses here prevent the second line from beginning with \( \tau \delta \varphi \alpha \); on the other hand, this is the only case in Homer where \( \delta \varphi \alpha \) in the sense of "while" both precedes the main clause and contains a present indicative, so that we do not have enough evidence to declare with confidence that in such a situation \( \tau \delta \varphi \alpha \) is impossible; and that \( \tau \delta \varphi \alpha \) + a present indicative is possible in early Ionic poetry is shown by line 46 of the poem on women by Semonides of Amorgos (fl. c. 640 B.C.\(^\text{92}\)), \ldots \tau \delta \varphi \alpha \delta ' \iota \sigma \delta \zeta \zeta \zeta \ldots \ldots \). Thus the parallel provided by II. 18. 61f.=442f. is of some value, but cannot be pressed too hard.
(c) I now come to the point on which I would lay most stress. For the sequence ὁψα ... | τῇν δὲ (11. 18. 380, 382) we have the exact syntactical parallels ἦς (an imperfect indicative) ... | ... δ' discussed above, p. 138. ἦς in the sense of "while" has exactly the same meaning as ὁψα in the sense of "while", and, as in the case of ὁψα, when the ἦς-clause precedes the clause on which it is dependent the former clause is normally followed by a demonstrative adverb of time, either τῇς (11. 20. 42, Od. 4. 91) or τῇν (11. 10. 507, 11. 412, 15. 392, 540, 17. 107, 18. 16, 21. 606, Od. 5. 425, 12. 328), but on the three occasions discussed above it is not: it is followed, as in the sequence 11. 380, 382, by a simple δὲ.

The conclusion is clear: here, as in the case of 11. 11. 266, Bolling was wrong to elevate a normal (but far from universal) syntactical pattern into an inflexible rule taking priority over all else, including the evidence of our MSS.: the evidence he presented was inadequate to justify either his final change of mind about 11. 18. 381 or his new idea about 11. 11. 266.

So much for possible applications of my hypothesis of very occasional accidental omission from an influential transcript of Aristarchus's edition. I shall now discuss three other lines which have given rise to difficulties.

I shall start with 11. 12. 130a; I print 127-32:

... ἐν δὲ πῦλησι δὲ ἀνέρας εὑρον ἀρετοῦσιν, 127
ὑς ὑπερθύμισι λατεθάναν αἰχμητάνων, 128
τὸν μὲν Πελίθεου υῦξα κρατερὸν Πολυκοίτην, 129
τὸν δὲ Λεοντῆα βροτολογυὸ ζῶν "Αρη". 130
τῷ μὲν ἄρα προπάροιχε πυλῶν υψηλῶν 131
ἐστασαν...

130a υῦξά ὑπερθύμιος Κορώνου Καλνεκίδας.

The additional line is found in Pap. 432 (Pack 875) of the latter half of the third century B.C., but not in another "eccentric" Ptolemaic papyrus which covers the passage, Pap. 121 (Pack 894, saec. ii a.C.). It is also absent from all our MSS. and from the post-Aristarchean Pap. 352 (Pack 893, saec. ii p.C.) and Pap. 60 (Pack 870, saec. iii-iv p.C.). It is twice quoted in the T Scholia, in a (somewhat corrupt) textual note introduced by των ἔπαγγελος93 and in a literary note praising the style of 128-30a. The των ἔπαγγελος mentioned in the textual note
are probably pre-Aristarchean, but the author of the literary note cites the line as an integral part of the poet's design without giving any clear indication that he is aware of a question about its authenticity, so that West (99-101) has concluded that it was once present in some post-Aristarchean texts. In my opinion this is a possible but not a necessary conclusion, in that it is possible that the author of the literary note in T knew the plus-verse from the non-corrupt ancestor of the textual note preserved in T and regarded it, on the basis of the internal evidence, as indisputably authentic, so that he felt entitled, in a piece of literary criticism, to quote the line without explicitly raising what he would have regarded as an unnecessary question - I say without explicitly raising it because it is in fact perfectly possible to interpret the scholiast as having a strong polemical undertone in defence of 130a: the scholiast makes the point that he finds the sequence ὑπαξ... ὑξ... ὑῖον pleasing, and his commitment to the longer text would have been made clear at the outset if the original lemma had contained not only 129 but also 130 and 130a - as, in Erbse's restoration, it does. The line is a ἔφοροςμένος, occurring at il. 2. 746 (with ὑὸς for ὑῖον). Bolling (EE 138) briefly rejects it as an interpolation; Van der Valk (RTSI II 480f.) argues against its authenticity at some length; but West (13, 99-101) believes it is genuine: "The context demands that Leonteus' father should be named; the difficulty is to account for the loss of the line in the later tradition" (p. 100). Her attempt to resolve this difficulty is as follows: "... the line was accidentally omitted in some pre-aristarchean texts; the similarity of the first half of the line to the first half of 128 would account for a mechanical error ... . How Aristarchus treated the line cannot in fact be inferred with any certainty ... . If Aristarchus omitted it, presumably his main reason was that it was absent from the majority of the MSS. available to him; if he retained it, we could explain its absence from the mediaeval tradition by the hypothesis that the booksellers often failed to insert it in copies where it was omitted: if they were concentrating on additions to the text, an omission would easily escape their notice" (pp. 100f.). My objection to West's treatment of the line are (a) that her explanation of "the loss of the line in the later tradition" is unconvincing, and (b) that it is unnecessary, because the assumption which led her to concoct that explanation - that the internal evidence demonstrates
the line's authenticity - is erroneous:-

(a) The homoiarchon in 128 and 130a might quite naturally have led to the omission of 129-130a or 128-130, but if it were to lead to the omission of 130a alone that would be a very odd kind of error. However, if the line was accidentally omitted in the pre-Aristarchean era, this mistake would be extremely unlikely to occur more than once; and if it occurred only once, it would have been perpetuated only in the direct descendants of the MS. in which it occurred, and even then we would expect it to return to the text of some of those descendants (if they were at all numerous): if the operation of contaminatio in the usual sense was less extensive in the less scholarly pre-Aristarchean era than it was afterwards, the absence of the line would nevertheless tend to be noticed whenever the passage was read or copied by someone who already knew it more or less by heart. Thus one would not normally expect an accidental omission to spread to "the majority of the MSS. available to Aristarchus"; one might suggest special circumstances as a result of which such a situation could have arisen, but West has not done so. Secondly, West's account of how a line could have been lost if Aristarchus retained it is implausible: if the booksellers "often failed to insert it in copies where it was omitted" (emphasis mine) then the corollary is that the line survived (i) in those copies in which the booksellers did insert it and (ii) in those copies in which it was already present; if the pre-Aristarchean omission of the line was accidental it would be natural to suppose that copies in category (ii) alone outnumbered all the other copies which the booksellers corrected; and West's argument "If they [viz. the booksellers] were concentrating on additions to the text, an omission would easily escape their notice" needs to be challenged: we know that Aristarchus included in his edition many lines which were absent from some or even many of his MSS.; it is natural to suppose that some of these lines would also have been absent from some of the booksellers' copies destined to be corrected κατὰ τὴν Ἀρισταρχοῦ; and yet there is no clear evidence for a tendency for such lines to be absent from some of our MSS. and post-Aristarchean papyri such as we might have expected if the booksellers had often failed to insert them where they were absent. Moreover, the booksellers would presumably have been anxious to insert lines accidentally omitted from their own copies. Thus there is no reason for believing that, of
the MSS. without ll. 12. 130a which were collated with MSS. containing the numerus versuum of Aristarchus, there would naturally tend to be more in which the booksellers did not add the line than in which they did. In these circumstances, with the vast majority of the booksellers' corrected texts falling into one or other of the categories (i) and (ii) and thus containing the line, it could hardly have failed to be present in the majority of our extant MSS., especially with the cross-fertilization which would have taken place through post-Aristarchean contaminatio. That the line should fail to occupy the text of a single extant MS. would be incredible. Finally, one needs to stress that the natural implication of the scholium των ἕξεσ ἐκάγουσιν κ.τ.λ. is that Aristarchus did omit the line.

(b) West offers no argument in support of her contention that "the context demands that Leonteus' father should be named" beyond quoting a statement by Wilamowitz: "Leonteus bleibt ohne Vatersnamen; das ist auffällig, zumal sein Vater der berühmte Koronos ist, wie im Schiffskatalog steht." She is presumably influenced primarily by the view that, since the poet says υᾶς ... Ἀπειθάων in 128 and Πειρηθάου υᾶ in 129, he is obliged to say also whose son Leonteus is. However, this position is untenable. The semantic content of ὁ ἄνερας ... ἄριστος, υᾶς ὑπερθόμους Ἀπειθάων αὐχιμπάτων (127f.) is presumably not "two outstanding men, each the high-hearted son of one of the Lapith warriors" but "two outstanding and high-hearted men from the Lapith warrior-tribe": in other words, υᾶς Ἀπειθάων is little more than a periphrasis for Ἀπειθάως, just as the very frequent formula υᾶς Ἀχαϊῶν is tantamount to Ἀχαϊῶν-οὖς. If one grants that the concept of actual individual sonship is not present in 128, it follows that the poet is perfectly entitled to specify the two heroes in whatever way he wishes; he is not obliged to name Leonteus's father, even though he has named Polypoites's; and in fact there may well be no conscious link in his mind between υᾶς in 128 and υᾶ in 129. Wilamowitz's reference to Leonteus's father as "der berühmte Koronos" is special pleading: there can be no doubt that Peirithoüs was a much more prominent figure than Koronos in the consciousness of post-Homeric antiquity, and in the Homeric poems themselves Koronos is named only at ll. 2. 746 whereas Peirithoüs is named at ll. 1. 263, 2. 741, 742, 12. 129, 182, 14. 318, Od. 11. 631, 21. 296, 298. Moreover, in the Catalogue not only is Peirithoüs named as Polypoites's
father, but there are two lines on his own ancestry (2. 741f.) and two referring to his exploits against the Centaurs (743f.), whereas Koronos gets only one line (746); five lines are devoted to Polypoites and his ancestry (740-4) and only two to Leonteus and his (745f.), and although Polypoites and Leonteus are named as joint leaders of a contingent, the wording implies that the latter's status is inferior to that of the former (740 τῶν ... ἤγεμόνευς ... Πολυποίτης, 745 οὐκ ὁδός, ἦμα τῷ γε Λεοντέους). Thus in 12. 128ff. Polypoites is named before Leonteus; and in 181-94 the aristēia of Polypoites is described before that of Leonteus, while Polypoites is again named as the son of his father (182, cf. 129) whereas Leonteus is not (188 Λεοντέους ὄξος "Αρπος, cf. 130 Λεοντῆα ... ίσον "Αρπη). Polypoites is again named before Leonteus in 11. 23. 836f., and achieves prominence by winning the contest (844-9); on this occasion neither hero's father is mentioned. It is thus natural enough that in 12. 127ff. the poet should name the famous Peirithoûs and omit any mention of the relatively obscure Koronos; and it would be strange if the more important Polypoites got one line (129) and the less important Leonteus two (130, 130a).

There are several possible motives which one could posit for the interpolation. It may well have been based on the feeling that after υἷα in 128 and υἷα in 129 Leonteus's father needed to be named; or 130a may have been interpolated after 190a in order to make the allusion in that line clearer, as the genealogy of Leonteus was not well known: compatible with this hypothesis is the fact that, of the two papyri which contain 190a, Pap. 432 and Pap. 121, the former also contains 130a whereas the latter omits it. Just possibly 130a is simply part of a single deliberate attempt to expand the part of Leonteus in Book 12: the papyrus which contains it (Pap. 432) also adds (besides 190a) 193a (= 140), which increases the tally of Leonteus's victims from the five of the vulgate to eight, and 189b, which elaborates on the fate of his first victim. (The relevant fragment of Pap. 432 starts with 189b; this text may well have contained 189a as well, with the corresponding change of wording at the end of 189, as in Pap. 121.) However, this hypothesis is rendered unlikely by the fact that, although both Pap. 432 and Pap. 121 contain 189b and 190a, 130a is present in the former but absent from the latter.
To conclude:— The external evidence— the absence of the line from all our MSS. and post-Aristarchean papyri and from one of our two pre-Aristarchean papyri, and the των ἐπάγουσ] scholium— cries out against the line. Moreover, not only is it not true that "the context demands that Leonteus' father should be named" after 130, but the text of our vulgate is arguably superior to the version with 130α; and a motive for the interpolation is not far to seek. There is thus no reason to doubt the conclusion naturally suggested by external evidence— that Aristarchus omitted the line, and that the line is spurious.

Let us next consider 11. 18. 604/5 of the Wolfian text, viz. μετὰ δὲ σφυν ἐμέλπετο θεός ἄνδρος | φορμύξων (= Od. 4. 17/18), which Athenaeus (181C-D) claims Aristarchus "excised" (ἐξετλευ). The line is absent from the entire post-Aristarchean tradition— i.e. from all our MSS., from all our vulgate papyri (Pap. 11 [Pack 953, saec i-ii p.C.], Pap. 108 [Pack 958, saec. ii-iii p.C.], Pap. 6 [Pack 952, saec. ii-iv p.C.104] and Pap. 234 [Pack 948, saec. vi p.C.105]), from the quotation of lines 603-6 by Dionysius of Halicarnassus at Ant. Rom. 7. 72. 9, from the scholia on the Iliad and even from a quotation elsewhere in Athenaeus (181A-B). The line is known only from this story in Athenaeus, which Wolf took at its face value and on the basis of which he inserted the line into his text.106

Van der Valk (RTSI II 223, 527-30) believes it was excised by Aristarchus on the basis of an erroneous assessment of the internal evidence; so does G. Pasquali,107 who explicitly debates the question whether Aristarchus was following an earlier excision and decides that he was probably not, i.e. that he probably had no manuscript support for the omission at all. To this view the arguments of Leaf ad loc., Bolling EE 47f., 183f. and West 134 provide an adequate reply: the story of Athenaeus is inconsistent with what we know of Aristarchus's modus operandi; we must interpret it in the same way as Plutarch's story that Aristarchus "excised" (ἐξετλευ) 11. 9. 458-61; all that we can really deduce from Athenaeus's story is the existence of a text or texts which contained 11. 18. 604/5; Aristarchus would not have omitted the line from his edition unless the majority of his MSS. had omitted it; "the text as it stands [i.e. without the line] is quite intelligible" (Leaf);108 and we should therefore regard it as an interpolation.109 Now suppose we had a papyrus of the "eccentric" pre-Aristarchean type covering the passage: if that papyrus, in spite
of containing additional lines, were to omit 604/5, the interpretation of Athenaeus's story which I have put forward would receive powerful support. As it happens, we do have such a papyrus, published in 1907, and it does omit 604/5; both Pasquali and Van der Valk report the fact, though without comprehending its significance. The papyrus is Pap. 51 (Pack 962): its date is saec. i a.C., but quite rightly Bolling (EE 183) calls it "a belated survival of the [pre-Aristarchean] Ptolemaic text" and West (132-6, cf. 12) includes it among her "eccentric" (i.e. "pre-aristarchean-type") Ptolemaic papyri: it covers I.I. 18. 585-608 and abounds in plus-verses, none of which reappear in any of our MSS. or vulgate papyri or in the quotation of 590-94 by Dionysius of Halicarnasus at Ant. Rom. 7. 72. 9: there are several (probably three) additional lines between 589 and 596 (see West 132), one after 606, four actually preserved after 608, and, as West says (p. 135), after 608d "the papyrus must have had further plus-verses ... in the next column": there can thus be no reasonable doubt that this papyrus is a late example of the expanded pre-Aristarchean type of text and not an interpolated descendant of the edition of Aristarchus.110 The external evidence against 604/5 is thus overwhelmingly strong.

However, the question cannot be left here, because the line has recently been defended by Z. Ritoök in an impressive discussion distinguished for its thoroughness, sophistication, scrupulous fairness (more specifically, a determination to expose the weakness or worthlessness of some of the arguments used by others in defence of the line), and, for the most part, sound judgment.111 He concedes (pp. 202, 206f.) that Aristarchus would not have omitted the line unless it was absent from the majority of his MSS. - it may even, he says, have been absent from all of them - but attributes its omission from these MSS. to the activity of the rhapsodes (pp. 205-7). His discussion is worthy of careful consideration; and in this context we can also consider the main arguments of Van der Valk and Pasquali in defence of the line.

Van der Valk (NTSI II 530) and Pasquali112 argue that Aristarchus excised the line because it was inconsistent with his doctrine that in Homer μολὴ and μέλος mean "play" (including "dance"), not "song" or "sing".113 The line recurs at Od. 4. 17/18 and (without
CPOP1JCl;W
13. 27, but on both these occasions Aristarchus has left it in his text. On the latter occasion it is absolutely essential to syntax and sense; this fact can, for Van der Valk, explain why Aristarchus had the decency to leave the line alone here, but the very fact that its firm attachment to its context vouches for its authenticity here would surely, one would have thought, have deterred Aristarchus from employing the scalpel at II. 18. 604/5. At Od. 4. 17/18, on the other hand, the line could have been removed without dislocation of the syntax, and the fact that Aristarchus did not remove it is at odds with Van der Valk's hypothesis, as has been stressed by Ritoðk (pp. 204, 206), who points out that the Aristarchean scholium on μολυνγίς in Od. 4. 19 equating that word with παλνιδ makes it unlikely that Aristarchus overlooked ἐμελπετο in line 17; further, the statement by Athenaeus, echoed by Schol. MT ad loc., that the school of Aristarchus "interpolated" Od. 4. 17-19 seems to imply the existence of pre-Aristarchean MSS. without these three lines (cf. Bolling, EE 226-9, Ritoðk 206); we cannot prove that Aristarchus himself knew such MSS., but it is not unlikely that he did, and if he did, then, as Ritoðk implies (205f.), his retention of the lines in spite of their containing ἐμελπετο in a context which made it difficult for him to reconcile this usage with his general doctrine on the word militates strongly against Van der Valk's hypothesis that he excised the ἐμελπετο line at II. 18. 604/5 on the basis of the internal evidence alone. Just what Aristarchus made of ἐμελπετο in Od. 4. 17 and 13. 27 we cannot be sure (cf. Ritoðk 204), but I think it quite likely that he tried to reconcile the usage with his doctrine by exegesis. While he apparently denied the meaning "song" to Homeric μολυνγί, it is clear that he regarded the word as equivalent to "play" in every sense of that concept: a scholium from Aristonicus on μολυνγίς in Od. 6. 101 says of the poet πᾶσαν παλνινα μολυνγίν λεγόν, and another scholium from Aristonicus, on Od. 4. 19, ἢ ὡπλῆ, ὧν οὐ τὴν ψόν ἀλλὰ τὴν παλνιναί λέγει σῇω, adds καὶ γὰρ "κυνών μὲληθρα" (II. 13. 233, cf. 17. 255, 18. 179), where the μελη- stem means "play" in a rather different sense and is used in a very different context (the phrase means "plaything of the dogs", said of an unburied corpse). Now the Greek verb παλνεῖν, like the English verb "play", can mean "play on a musical instrument" (L-S-J s.v. I. 4), so that without doing violence to his doctrine Aristarchus could have interpreted ἐμελπετο at Od. 4. 17 and 13. 27 as "played" in the
musical sense, in the former case regarding it as a general term more closely defined by φορμίζων in the next line. Such an exegesis, of course, would have made Η. 18. 604/5 (if he knew the line at all) no more objectionable to him than its two counterparts.

Van der Valk (RTSI II 529) has argued that the parallel passage Od. 4. 17-19 is obviously based on Η. 18. 604-6, and that therefore the recurrence of the disputed line, Η. 18. 604/5, as Od. 4. 17/18 supports the hypothesis that it is genuine in the Iliad. Ritoök concedes the weakness of this argument: there is no need, he says, to assume that the Odyssey-passage is based directly on the Iliad-passage, since the two could stem from a common formulaic stock, and in the representation of a typical scene a formulaic line more or less here or there is just what we would normally expect in oral poetry. To this I would add that, even if we suppose that the two passages were not part of the oral poet's stock-in-trade, it remains easy to account for the discrepancy between an Odyssey-passage with Od. 4. 17/18 and an Iliad-passage without Η. 18. 604/5. I shall assume that the author of Od. 4. 17-19 was either (a) an interpolator (there is both internal and external evidence against it) or (b) an Odyssey-poet who (i) knew the Iliad well and was strongly influenced by it and (ii) composed the Journey of Telemachus as one of the later stages in his gradual expansion of a poem about the wanderings, return and vengeance of Odysseus into an epic of monumental length.

Very well then: the banquet of Od. 4. 3-15 recalled the association between feasting and music common in the Odyssey, and just as Od. 13. 26, which contains the word δαίνυντ', is followed by τερπόμενον· μετὰ δὲ σφυν ἐμφάνισα, so the word (and concept) δαίνυντο in Od. 4. 15f. (emphatic because taking up the δαινύντα of line 3) suggested the same continuation, τερπόμενον κ.τ.λ. (Od. 4. 17 = 13. 27). Then the word τερπόμενον, as well as the general Homeric association between music and dancing, recalled the dance led by tumblers in Η. 18 and the couplet τερπόμενον· δολῶ δὲ κυμαστήρε κ.τ.λ., which the interpolator, or poet, was able to introduce here (as Od. 4. 18f.) by substituting the filler φορμίζων for the already-used τερπόμενον. Much later, deliberate or unconscious assimilation of the two passages produced the concordance interpolation of Η. 18. 604/5 μετὰ δὲ σφυν ...

Ritoök argues (pp. 204f.) that the bard is present in all other epic representations of communal dancing. He then considers an
objection: these other epic representations are all post-Iliadic; bards play an important role in the Odyssey, reflecting the Odyssey-poet's self-consciousness; they do not in the Iliad; so would not the absence of any mention of a bard in II. 18. 604ff., and his presence in Od. 4. 15ff., be entirely consistent with the rest of the Iliad and Odyssey respectively? This is, I think, a good argument: the Concordances list 37 occurrences of ἄοιοδος in the Odyssey and only one in the Iliad (24. 720). Ritóók concedes that the argument is satisfactory enough as far as it goes, but argues against it that the Shield of Achilles is meant to depict life, and that "die archäologischen Denkmäler ... zeigen besonders klar, dass der Sänger mit seiner Phorminx bei den Reigentänzen fast immer anwesend ist ..." (p. 205). However, I would focus on the word "fast" and argue that there is no obligation on the poet, any more than on the visual artist, to always depict every feature of a typical scene: if a bard was always present on such occasions in real life (and I am not persuaded that he necessarily was), that could sometimes go without saying, especially in a series of rapid transitions from one scene to another such as we have in the Shield - and especially if (as may well have been the case) the Iliad-poet had a positive reticence about introducing practitioners of his own profession into his poem: to do so more than absolutely necessary might have seemed to him to be in poor taste, and the "Selbstbewusstsein" (Ritóók) of the Odyssey-poet, with the extravagant praise of Demodocus and of bards in general (8. 471-91) and Telemachus's insistence that Phemius be allowed to carry on doing his own thing in spite of Penelope's distress (1. 345-53), might have struck him as tasteless exhibitionism.

It will be clear, then, that I regard the argument on which Ritóók ultimately pins his faith as worthless, given the external evidence against the line. But he has a further difficulty to face: if the line is genuine, how can one explain its absence from the majority (or even all) of Aristarchus's MSS.? With characteristic fairness, Ritóók (p. 207) considers a specific objection: the loss of the line could only have come about through the activity of the rhapsodes - but surely (he says) they would not have been likely to omit a line mentioning an ἄοιοδος, a representative, virtually, of their own profession? He replies that the rhapsodes' sense of self-importance would have been slighted, rather than reinforced, by an implicit association of their profession with "herumspringenden
Springtänzern", and once the reference to the bard had fallen out of one version of the *Iliad*, whether through deliberate excision or by sheer accident, the rhapsodes generally would have been only too glad to perpetuate this bardless version in their performances and so save themselves from this embarrassing contamination. To most, however, this will surely appear as special pleading: the occasion is clearly presented as an elegant and dignified (if lively and joyful) one, by means of the association of the dancing-floor with Ariadne, the complimentary description of the participants' dress, the complexity, skill and grace of their movements and the accompanying simile. I think it highly unlikely, therefore, that rhapsodes would have had any aversion to the line, and it is at best very doubtful whether even a universal conspiracy among fifth-century rhapsodes (for that is when Ritoňk believes the loss of the line occurred) would have led to its absence from the majority of Aristarchus's MSS., since the written transmission would not have been entirely in their hands. One could improve on Ritoňk's hypothesis by positing instead a purely accidental omission from the official sixth-century Panathenaic text due to a lapse in the dictating rhapsode's memory; but for the general reasons explained in Chapter IV I regard it as unlikely that this would have occurred and unlikely that, if it did, it would have led to omission by Aristarchus; and this particular line is particularly unlikely to have been omitted by the dictating rhapsode, not only because the post-Homeric rhapsode would naturally tend to see himself as the lineal descendant of the Homeric ἀρσόδος (note the pregnancy of the term ὀμπιλατ in this connection) and therefore to remember particularly vividly a line in which such an ἀρσόδος was portrayed, but also because of the general association between bard and dancers in the *Odyssey* (e.g. 1. 151-4, 8. 250-65, 23. 133-47) and because the actual line occurs there at least once. 120

To conclude: I see no reason to make out a special case in favour of *Il.* 18. 604/5 which would override the overwhelmingly strong external evidence against it. On the contrary, there is every reason to allow this external evidence to carry its natural weight—indeed, even more weight than usual.

Finally, let us turn to the much-discussed *Il.* 2. 558. The question here is not so much whether Aristarchus omitted a genuine
line (for hardly anyone regards II. 2. 558 as genuine) as whether he omitted a line which was present in all or most of his MSS. and, if so, what implications this would have for our general concept of his modus operandi. I print II. 2. 557f.: 

Αὐτὰς δ' ἐκ Σαλαμίνος ἔγεν δυσκαζόσκα νηάς, 557
ποτησε δ' ἄγων ἐν 'Ἀθηναίων ἔσταντο φαλαγγες. 558

558 is omitted by Pap. 2 (Pack 616, saec. ii p.C.) and Pap. 38 (Pack 650, saec. ii p.C.). Pap. 104 (Pack 643, saec. i p.C.) is mutilated at the crucial point, but the surviving fragment of the line following 557 looks much more like part of 559 than of 558. In Pap. 146 (Pack 658, saec. vi p.C.) line 558 is found only in the top margin, before line 556, and possibly in a different hand. Of our tenth-century MSS., A omits it and D is not extant at this point; and it is also omitted by a substantial minority of the later MSS. Among the numerous comments on this line in the scholia and post-Aristarchean literature are two by Aristonicus (preserved in Schol. A) contained in notes on other passages — passages which Aristarchus believed to be inconsistent with II. 2. 558:

(1) on II. 3. 230: ἡ δυσλή, δι' ἑλπίδον τοῦ Τελαμώνιου ἐν τῷ Καταλήψις ἐπάρατο καὶ ἐν τῇ ἐκπώλωσιν [II. 4. 251-73], συμφωνεῖ. Παρατητέον ἢ ἠκούσαν τόν στίχον τόν ἐν τῷ Καταλήψις ὑπὸ τῶν γραφικοὺ τοὺς ἐν τῷ Καταλήψις ἐπάρατο, "ποτήρι ..."· οὖ γὰρ ἦσαν εἰπεῖσκον Ἀλκαντος Ἀθηναίων.

(2) on II. 4. 273: <ἡ δυσλή, δι' ἑτέρως ἰδιομενέως ἐπηρέασεν Ἀλκαντος, πρὸς τοὺς ὑποτάσσομεν τοῖς Ἀθηναίοις τὸν Τελαμώνιον. 125

These comments, in conjunction with the state of the post-Aristarchean MSS. and papyri, show clearly that the line was totally absent from the edition of Aristarchus (not merely atthenized by him, as some have supposed).

Are the τίνες of comment (1) above pre- or post-Aristarchean? In comment (2) Aristonicus is clearly reproducing the views expressed in Aristarchus's ὑπομνήματα ad loc., and in the first sentence of comment (1) he is clearly doing the same, and these facts, in conjunction with the close correspondence between each of the two comments as a whole, invite us to deduce (a) that the παρατητέον sentence in comment (1) does not add an observation which Aristonicus has thought up himself.
but is once again based on a comment in Aristarchus’s ὑπομνήματα ad loc. - and this is also supported by the consideration that after συμφόνως (or its original equivalent) we would expect Aristarchus in his ὑπομνήματα ad loc. to have explicitly made his point about the disharmony with II. 2. 558 - otherwise his comment would have been allusive and elliptical to the point of virtual incomprehensibility; and (b) that the τοὺς of comment (1) are identical with the certainly pre-Aristarchean τοὺς ὑποτάσσοντας κ.τ.λ. of comment (2) - a conclusion which is supported by the fact that the τοὺς in such circumstances are normally pre-Aristarchean. Moreover, it is far from certain that the line would have been present in any post-Aristarchean MSS. known to Aristonicus in view of its very weak attestation in the extant post-Aristarchean texts down to the tenth century P.C. (though on the other hand we cannot deny that the evidence - especially the presence of the line in as many as four of the five eleventh-century MSS. - would be compatible with its presence in some texts current at the time of Aristonicus). 129 Bolling (EE 49) compares Aristonicus’s παραπτητὸν apropos of II. 2. 558 with his περιγραφτέον on Od. 11. 525, which was also absent from the edition of Aristarchus. 130 The latter word, which Bolling rightly interprets as meaning that the line should be deleted or cancelled, not merely athetized (see also EE 211f.), does suggest strongly - especially in view of the context provided by the note of Didymus and the evidence of our MSS. - that the line was present in at least some MSS. current in Aristonicus’s day and that Aristonicus is calling for its deletion from such MSS. However, παραπτητὸν, meaning "We should refuse to accept the line [into the text]", certainly need have no such implication, even if we assume (what is doubtful) that the word was chosen by Aristonicus rather than Aristarchus: περιγραφτέον implies a gate-crasher who has already gained entry and must be ejected, whereas παραπτητὸν may properly be used of a mere candidate for entry who must be refused admittance. Actually it is uncertain just how much Bolling meant to imply in his comparison: elsewhere in EE (16, 73) and in earlier discussions 131 he seemed to studiously avoid expressing an opinion on the question whether the τοὺς mentioned by Aristonicus as reading II. 2. 558 were pre- or post-Aristarchean; but in 1928 he came out clearly in favour of the view that they were pre-Aristarchean: "The scholium on Ἀ 273 shows that Aristarchus knew the τοὺς who wrote the line ... ."132
Thus on the natural interpretation of the scholia we have valuable verification that on this occasion Aristarchus's omission was consistent with his normal modus operandi and that the line was absent from the vast majority of his MSS. - for the term τῶν suggests that the (pre-Aristarchean) texts which included the line would have been few. Even if one rejects my argument that the τῶν were in all probability pre-Aristarchean, one can hardly avoid the conclusion that the reference to the certainly pre-Aristarchean τῶν ὑποτάσσοντας in comment (2) above shows that there was a weakness in the line's pre-Aristarchean attestation. It is important to stress this point, because although (as we have said) hardly anyone regards the line as genuine, scholars often tacitly assume that it would nevertheless have been present in all Aristarchus's MSS.

Moreover, Bolling has demonstrated that the story surrounding the line is entirely compatible with its absence from most of Aristarchus's MSS. The gist of this story is that during an attempt to settle the dispute between Athens and Megara over the possession of Salamis by Spartan arbitration Solon (or Peisistratus) read out at the hearing as evidence an excerpt from the Homeric Catalogue of Ships which included a forged line, our 588, and that the Megarians replied with a parody (Ἀντιπαρφρέασα) to support their own claim. Bolling (EE 73) argues that "the meaning is clearly that they [sc. both parties] recited as if genuine extra lines of their own composition, not that they forged and put into circulation copies of the lengthened text", and on one point he is even more definite at EE 16: "Ut haec interpolatio per verba sola fieri [he means: facta esse] intelligatur plane necesse est." His insistence that the interpolation was (according to the story) an ad hoc one is entirely justified, though his specific "per verba sola" point goes too far: the story surely supposes that the Athenians were able to produce a (forged) written text of Π. 2 (or of part of it) which included the interpolated line: note παρεγγαγμα ... μάρτυρι χρήσασθαι τῷ πολεμῷ in Strabo's version (394), ἐμβαλλόντα ... ἀναγγέλλαν in Plutarch's (Life of Solon 10), and τὸν Κατάλογον in that of Diogenes Laertius (1. 48). It follows from the ad hoc nature of the interpolation that even if the story were true it would not necessarily imply that the line was present in the official sixth-century Panathenaic text: we could suppose, for example, that it was Solon (who in any case has a stronger claim than Peisistratus to be the Athenian involved in the story) who was the
author (or, more likely, the patron) of the interpolation, but that
the forgery had become so widely recognized as such and so notorious
by the time of Peisistratus's rule that the latter shrank from
including it in his text; animosity on Peisistratus's part towards
Solon, who had opposed his rise to power, could have provided a
further motive. However, Bolling takes it for granted that the story
is merely a "fiction" or "legend", and his purpose in stressing
the ad hoc nature of the interpolation is rather to show that "the
line was manufactured for the story, not the story for the line".
In other words he is arguing (presumably) that the story cannot have
been designed to explain (or explain away) how it came about that all
texts of the Iliad contain line 558. In my judgment the intrinsic
nature of the story is not in fact such as to preclude an aetiological
(or quasi-aetiological) interpretation along these lines, but suits
Bolling's own interpretation at least as well. Moreover, a middle
position between these extremes would not be implausible - that the
line was present in only a small segment of the pre-Aristarchean
tradition, but that its interpolation had nothing to do with Athenian
claims to Salamis but was rather "meant to eke out the Catalogue at a
weak point" (Allen), but that it was later associated with accounts
of a Spartan arbitration over Salamis.

That the story of the citation of I. 2. 558 at the arbitration
is fictional seems highly likely. In this connection it is important
to note the context of Plutarch's comment on the story (loc. cit.),
ἀυτοὶ δ' Ἀθηναῖοι ταῦτα μὲν οὗντα ψευδάτα εἶναι, which closes the
excerpt in Allen's apparatus: the meaning is of course that the
Athenians indignantly deny that Solon employed the services of a
Dirty Tricks Department, but this denial does not, as we might perhaps
have expected, take the form of a claim that the line is genuine, but
instead asserts that Solon in fact used other, more respectable,
arguments: the comment I have just quoted immediately continues:
τὸν δὲ ἔξωμα φασὼν ἀποδείξαν τοὺς δικαστὰς ὤτε ... . Thus the
natural interpretation of Plutarch is that the Athenians concede that
line is spurious and reject the whole story of its interpolation by
Solon: in other words, Plutarch here supports Bolling's contention
that "the line was manufactured for the story, not the story for the
line", or at any rate strongly suggests that "the Athenians" cited by
Plutarch conceded that the line never had a firm place in the textual
tradition.
Whether or not there really was a Spartan arbitration, and whether or not it involved the citation of Homer, it is quite possible that in the original form of the story the Athenians, in support of their claim, did not insert an extra line (558) into the Catalogue but simply cited 546-57, in which, as Bolling has put it, "Ajax is made but a tail to the Athenian kite", and that the reference to 558 and to the Megarians' counter-interpolation is a later accretion. Thus Aristotle (Rhetoric 1375B 29f. ὅτι Ἀθηναῖοι ὁμήρῳ μάρτυρε ἐχρήσαντο περὶ Σαλαμίνος) does not actually mention 558, and, as Bolling has suggested, his reference could well be to an earlier version of the story along the lines I have just described. In this connection it is worth pointing out that the Megarian Dieuchidas, of the fourth century B.C., speaks (apud Diogenes Laertius I. 57) of the interpolation not of 558 but of "ὅλ' ἤρ' Ἀθηναῖος εἷκον" καὶ τὰ ἔξης, i.e. 546ff. (546-57?). Of course even if we were to allow that the passage 546-57 was an early Athenian version of this part of the Catalogue which differed from Homer's, that would not necessarily imply that 558 was equally early.

Moreover, if Aristotle did know 558 it is quite possible that he knew it only from the story surrounding it rather than from any text of Homer: although he defines "ancient" witnesses (οἳ καλανό) as τὸς τε πολιτας καὶ δοσιν ἄλλων γνωριμων εἰςὶ νάερες φανερα and then immediately cites the Salamis example (as quoted above), it is noteworthy that his next example but one consists of a blatant misuse of a line of Solon by Cleophon against Critias - Cleophon may even have deliberately altered the text to suit his purpose. Be that as it may, the distortion of the line's meaning has not escaped the notice of modern commentators, and is hardly likely to have escaped Aristotle's. His next example is Themistocles's use of the "wooden wall" oracle as a witness for the future - but at the time this was a κριτας φανερα only in the most superficial sense, for we know from Herodotus (7. 141-3) that its interpretation was hotly disputed. His next two examples are as follows: "Further, proverbs ... are evidence; for instance, if one man advises another not to make a friend of an old man, he can appeal to the proverb, 'Never do good to an old man'. And if he advises another to kill the children, after having killed the fathers, he can say, 'Foolish is he who, having killed the father, suffers the children to live' [Stasinus]."
These examples are neither non-controversial nor edifying. A little later on (1376A 17-23) Aristotle tells you what to say if you do not have any witnesses and your adversary does, and then again what to say if you have witnesses and your adversary does not, turning either situation to your own advantage; and a similar point is then made about contracts (1376A 33 - B 30). Bearing in mind the wider context, with its emphasis on scoring points rather than on impartial presentation of the truth, we cannot with any confidence assume that Aristotle took II. 2. 558 to be authentic or that he found it in his own text of Homer. He does of course imply that the Athenians relied on the universally-acknowledged prestige of Homer (as Cleophon did on that of Solon), but he need not have supposed that the text produced by Solon was authentic at the crucial point any more than he need have supposed that Cleophon's interpretation of Solon was accurate. We may compare the statement of Plutarch (loc. cit.) that τῷ Ἐδώνῳ συναγωγῶν σάφα ο ῖ τὴν Ὄμηδου δέξαν when he (Plutarch) is on the verge of explaining how Solon interpolated the crucial line. Indeed, if the suspicion of Cope and Sandys that the line from Solon quoted by Cleophon may have contained a deliberate alteration of the text is right, and if Aristotle was aware of this, we have here a virtually exact parallel to the supposition that Aristotle regarded II. 2. 558 as an interpolation.

Whether or not Aristotle had II. 2. 558 in mind when making his allusion, a clear terminus ante quem for the composition of the line can be established from Matro's 'Αττικὸν Δεξιῶν, a Homeric parody written towards the end of the fourth century B.C. Lines 95-7 of this poem in Brandt's text (= Athenaeus 136E) run as follows:


The third line, of course, clearly parodies II. 2. 558 (as the first does 557). However, we are not entitled to deduce from this that Matro found 558 in his text of Homer, since he could well have known the line merely from the story surrounding it. A piquant anecdote like this would certainly have lent itself to an indirect allusion in a light-hearted Homeric parody. In fact the supposition that Matro had the story rather than the text of Homer in mind becomes even more plausible when one looks at his parody more closely. Solon is said to
have interpolated 558 immediately after 557, but in Matro a line intervenes. Why? And why λμυνς ἐξ ἑρῆς? Brandt innocently remarks in his commentary (p. 88), "De lacu sacro Salaminio nihil aliunde habemus compertum." It is not a Homeric phrase; and in his apparatus (p. 69) he suggests that in this line Matro had Il. 5. 709f. in mind, where λμυνη begins the first line and μάλα πόνα comes in the middle of the second. A diligent piece of research with the then recently-published Concordances of Frendergast and Dunbar, and probably at any rate partially right; but rather a pity that the felicity commented on by Brandt on p. 88 - that Matro has successively parodied two Homeric passages about Aias (for Matro 93f. are based on Il. 16. 102f.) - should be spoilt by the intervention of a line (Matro 96) based on a Homeric couplet which has nothing whatever to do with Aias. I suggest, however, that in line 96 Matro's λμυνς ἐξ ἑρῆς may well be intended to recall the alleged version of the Megarians preserved for us by Strabo (394):

Αδας δ' ἐκ Σαλαμινος ἐγεν νέας ἐκ τε Πολύχυνς ἐκ τ' Ἀγείρωδοςς Νυσαλής τε Τριπόδων τε.

I suggest that λμυνς ἐξ may well be intended to recall ἐκ τε Πολύχυνς and ἐξ ἑρῆς to recall ἐκ τ' Ἀγείρωδοςς. If anyone claims that the verbal resemblances are too slight for this hypothesis to be plausible, a brief reply is possible: read Matro in conjunction with the list of Homeri detorti versus which Brandt appends to his text. Thus, for example, in 94 ζῳδξς τε μέλας is clearly meant to recall Ζηνός τε νῦς in Il. 16. 102, and in 95 itself τας δὲ τις is doubtless meant to recall Αδας δ'. When one also bears in mind that Matro was restricted by the necessity of choosing a provenance which would suit the ducks of line 95, one can only marvel that he has been able to get so close to the Megarian retort. The allusion would, of course, have been tantamount to a parody of a parody, but such a conceit would not be alien to the spirit of the genre. To summarize: three factors - the presence of an intervening line (96), the presence of a second ἐκ (ἐξ), and the other verbal echoes - combine to suggest that Matro may well have had the Megarian version in mind in line 96.

To these three factors I would very tentatively add two others. (1) The Megarians' second line begins ἐκ τ' Ἀγείρωδοςς, "from Poplar Place". Now the ἀγείρωδος or black poplar is mentioned eight times in Homer, and on six of these occasions it is associated with water. The
poplar of Il. 4. 482-7 grows in a marsh (Ελος) adjoining a river; the poplars of Od. 10. 510 grow on the bank of the River Okeanos; the poplars of Od. 5. 64 (cf. 70-71), 6. 292, 9. 141 and 17. 205-11 grow around a spring or springs, and in the last passage the point is underlined with the epithet "water-nourished": αἵεςρων ὑδατοτρεφεῖον (208). Now Matro was writing a Homeric parody, and may have felt that the Homeric association between poplars and water made λύμνης particularly appropriate in an attempt to suggest the Megarian Αἵερωθοσα. Alternatively, the whole thing could be explained at a more down-to-earth level: poplars had not lost their hydrophilia during the time between Homer and Matro: it was a fact of life, and it may have been this fact alone which made Matro feel that his λύμνης would be appropriately suggestive. I do not suppose he knew whether there was really a lake or marsh (λύμνη) at Αἵερωθοσα, but in the light of its name he may have thought it a fair bet that there was. In addition, then, to the echo ἐκ τε Πολυχνης - λύμνης ἐκ, it could be that the whole phrase λύμνης ἐκ ἑρής was meant to suggest ἐκ τ' Αἵερωθοσῃς: each fills the same (initial) part of the hexameter, and Matro relied heavily on line-position to reinforce his more distant Homeric echoes (e.g. the ζωμὸς ... - Ζηνὸς ... parallel cited above). (2) It is possible that the last word of Matro's line 96, μάγερος, was intended to recall the Μεγαρεῖς themselves.

Now no one supposes that the Megarian version had even a moderately firm place in the pre-Aristarchean tradition of the Homeric text - most suppose that it had no place at all. Moreover, Matro's text of Homer could not have contained both the Megarian and the Solonian version. Thus if the hypothesis that he was parodying both versions is correct, it follows that he was perfectly willing to parody material which did not stand in his text of Homer. It would be quite natural, then, to suppose that his text of Homer contained neither the Solonian nor the Megarian version but rather the unexpanded version later accepted by Aristarchus, and that he knew both expanded versions from the anecdote alone.

Thus one certainly cannot use Aristotle, as Heitsch tries to do, to prove that in the fourth century B.C. Il. 2. 558 "fest in den überlieferten und gängigen Text gehörte", or Matro, as Dihle tries to do, to prove that in the same period the line "zum Versbestand der
Ilias gehörte". Even if one were to grant that the line stood in the texts used by Aristotle and Matro, the conclusion would go beyond the evidence; after all, Ludwich himself conceded "dass Aristoteles den Vers für echt hielt", but added, "Trotz der Beglaubigung des Aristoteles konnte doch das Uebergewicht der besseren handschriftlichen Ueberlieferung schon in jener Zeit gegen die Echtheit des Verses entscheiden. Sicherlich war das zu Aristarch's Zeiten der Fall ...").

We must next consider the argument that the presence of II. 2. 558 in the sixth-century B.C. text of Homer is proved by the presence of the phrase στῆσε δ' ἄγων δή in line 384 of the Homeric Hymn to Demeter. This argument has been vigorously put forward by Heitsch on the basis of a similar treatment of the two passages by G. Jachmann. The argument, in essence, is: (1) a comparison of the two passages shows that Hymn to Demeter 384 must be modelled on II. 2. 558, not vice versa; (2) but the Hymn was composed in the sixth century B.C. or earlier; (3) therefore II. 2. 558 was in the text of Homer in the sixth century B.C. or earlier. However, I shall argue that (1) is false and the argument therefore invalid.

We may start by setting line 384 of the Hymn in its formulaic context. The grammatical subject of lines 380-83 has been the horses driven by Hermes, and they are the understood object of στῆσε δ' ἄγων in 384. At II. 24. 350 the (expressed) object of στῆσαν is ἡμέραν τε καὶ ἔπεισε; at Od. 7. 4 the understood object of στῆσαν is the mules mentioned in line 2; at Od. 4. 22 στῆσαν is used intransitively and its subject is τῶ ... αὐτῷ τε (i.e. Telemachus and Peisistratus, named in line 21) καὶ ἔπεισε. Moreover, in all three places, as in the Hymn, στῆσεν/αν comes at the beginning of the line. It would thus be entirely natural for a poet steeped in the Homeric formulaic tradition, as was the author of the Hymn, to use στῆσε at the beginning of a line to describe Hermes stopping his horses. As for the combination στῆσεν ἄγων, one may compare, as Richardson does, εἶσεν ἄγων at the beginning of II. 1. 311 and in Od. 1. 130: εἶσεν and στῆσεν are similar in both sound and meaning. We also have (though admittedly in a different metrical and semantic setting) Od. 6. 7 ... ᾧ ἀναστίσασας ἔγε ... . Thus the formulaic tradition provided the poet of the Hymn with close and ample parallels for his phrase στῆσε δ' ἄγων, and we have not yet found anything to suggest that in
using it he was guilty of an awkward imitation of II. 2. 558.

It is not unlikely, though, that the author of II. 2. 558 had the Hymn passage in mind when constructing his line: the similarity between the two passages extends beyond the phrase στησε δ' ἄγων to the fact that it is followed in each case by an adverbial clause of place, as Heitsch has pointed out. Heitsch and Jachmann, however, argue for the priority of II. 2. 558 on the grounds that ἄγων in 558 takes up ἄγεν in 557, whereas in the Hymn ἄγων "dieser stützenden Protasis entbehrt" (Jachmann), "absolut [steht] und dadurch im Zusammenhang eigentümlich unbestimmt [wird]" (Heitsch). Heitsch also claims, over-optimistically: "Dass die Junktur in B 558 besser passt, wird jeder zugeben." On the contrary: the phrase better suits line 384 of the Hymn. There ἄγων has much more point: in lines 379-83 the course taken by the horses has been described only in the most general terms, and ἄγων is needed to give an indication of the goal towards which Hermes is steering them: without this participle στησε would be unduly abrupt. The meaning is: "Hermes brought them [the horses] to the place where Demeter was staying and stopped them there, in front of her temple." In II. 2. 558, on the other hand, the participle is worse than redundant: it would have been all right to say " Ajax brought twelve ships from Salamis and stationed them where the Athenian battalions were stationed", but "Ajax brought twelve ships from Salamis and, bringing them, stationed them ..." is scarcely better in Greek - even pleonastic Homeric Greek - than in English. This is not effective anaphora or legitimate Homeric tautology: the "stützende Protasis" of 557, so dear to Jachmann's and Heitsch's hearts, is in fact no support to ἄγων at all but a positive embarrassment. The empty and awkward repetition recalls nothing so much as the notorious Davis "decipherments" of the Phaistos Disk. In his attack on the phraseology of the line in the Hymn Jachmann writes, "... man begreift, dass Mitscherlich στησε δ' ἄγων' konjizierte." To this I would reply, apropos of II. 2. 558, "Man begreift, dass Wilamowitz στησε δ' ἄγων' konjizierte." Of course ἄγεν in II. 2. 557 has helped the author of 558 to compose his line, but it should be regarded as a stimulus for his recollection of Hymn to Demeter 384 rather than as a proof that the Hymn line was quarried from II. 2. 558 (still assuming that the two lines are directly related, which is not certain.)
Of course even if the reader does not fully accept my arguments for the priority of the Hymn line (if one line was modelled on the other) on the basis of the internal evidence but grants me only a non liquet verdict, it is still open to us to posit a later date for the Iliad line on other grounds. And - to suppose the worst - if the reader remains fully convinced by the arguments of Jachmann and Heitsch in spite of my protest, I can remind him that one of the hypotheses which I mentioned above (pp. 167f.) as possible was that Solon was in fact the patron of the interpolation but that it was nevertheless absent from the Peisistratean Panathenaic text. On this hypothesis it would be perfectly possible to hold that the author of the Hymn was influenced by the Iliad line without denying that it was absent from the majority of Aristarchus's MSS. Thus Allen assumed that it entered the text in the sixth century but was transmitted in only a segment of the tradition.166

Dihle's allegation167 that we can deduce from the pseudo-Hesiodic Fragment 204 Merkelbach-West168 that I1. 2. 558 "wohl auch im 6. Jh. v. C. ... zum Versbestand der Ilias gehörte" can be quickly disposed of. The fragment, which includes the phrase Ἀδρίας ὁ ἐκ Σαλαμῖνος (line 45), attests, at the most, a knowledge of I1. 2. 557 in the sixth century, and hints that Salamis and Athens were on friendly terms with one another. That is clear enough from Heitsch's treatment of the fragment:169 not even he claims that it can be used to demonstrate a knowledge of I1. 2. 558. We may recall Bolling's statement that in 546-57 "Ajax is made but a tail to the Athenian kite":170 that passage alone (i.e. without 558) would have sufficed to restrain the author of the pseudo-Hesiodic fragment from the infelicity of making Ajax plunder Attica - though a recognition of Athens' claim to Salamis would have helped. Equally valueless, for similar reasons, is the argument of Allen that "Pindar, who uses Ajax as an Athenian (Hem. ii), probably read it [sc. 558]",171 or the argument of Allen172 and Von der Mühll173 that Herodotus 5. 66 attests the line.

To summarize our discussion so far: there is no adequate reason for supposing that Aristarchus found I1. 2. 558 in more than a few of his MSS., and there are various reasons for supposing that he did not.

It is, I suppose, just possible that the line was present in the Peisistratean text and its earliest descendants but that, long before
the time of Aristarchus, the interpolation had become so notorious as such that it had become customary not to include it in texts of the Iliad; but, especially given the prominence of Athens in the early transmissions of the Homeric poems, it hardly seems likely that a conscientious excision like this would have become so widespread as to result in the omission of the line from the majority of Aristarchus's MSS. Aristarchus's conservative reputation could be saved by such a hypothesis, but there is no need to resort to it when the evidence much more naturally suggests that the state of Aristarchus's MSS. was due simply to the fact that the interpolation had not spread very far. 174

Let us suppose, however, that the reader still believes, in spite of my arguments, that all Aristarchus's MSS. must have contained Il. 2. 558. I must stress that it is not because of any lack of confidence in the correctness of my demonstration of the extreme improbability of this supposition that I now proceed to put forward a defence (admittedly a less effective one) of Aristarchus's conservatism which would apply if my demonstration were rejected. If Aristarchus omitted the line in spite of its presence in all his MSS. this could not have been on the basis of his judgment of the internal evidence alone: we would then have to suppose that he had sources which led him to take the story of the line's interpolation by Solon (or Peisistratus) as a trustworthy and accurate historical report - a reliable and valuable piece of external evidence attesting an earlier state of the tradition in which the line was absent from all versions of the Iliad. 175 I would stress the phrase "external evidence": in such circumstances we can see how Aristarchus may have regarded the unanimous omission of the line in the very early tradition, even though only indirectly attested, as far outweighing the unanimous direct testimony in favour of the line by the more recent tradition. Of course the existence of the apparently, or actually, historical story of the line's interpolation would make this a special case.

However, I conclude by reiterating my conviction that the evidence points very strongly to the conclusion that the line was absent from all but a few of Aristarchus's MSS., so that the external basis for his omission would have been no different from usual.
What can we conclude from this Chapter as a whole? The hypothesis of Aristarchus's conservatism remains unshaken. The hypothesis that some of the weakly-attested lines of our MSS. are genuine lines which a conservative Aristarchus omitted because of strong but misleading manuscript evidence against them has received scant support from our investigations: in the case of II. 23. 804 this hypothesis has turned out to be virtually impossible, and the alternative hypothesis that Aristarchus included the line in his text and that the behaviour of the post-Aristarchean tradition is due to an accidental omission at the start of that tradition has turned out to be virtually certain. If some of the other lines considered in this Chapter are authentic, this same hypothesis of accidental omission at the start of the post-Aristarchean tradition must be judged a far more plausible way of accounting for the behaviour of that tradition at these points than the rival hypothesis of omission by Aristarchus stemming from influential pre-Aristarchean accidental omission, not only because the case of II. 23. 804 implies that the former hypothesis may well be applicable to a few other lines but also because of the general difficulties for the latter hypothesis as explained in Chapter IV. Of course the necessity of allowing even for only very occasional accidental omission from an influential transcript of Aristarchus's edition has complicated the picture somewhat. It would have been neater if such omissions could have been completely ruled out, but in the sphere of textual criticism the truth is seldom quite as neat as one might wish. However, a very heavy burden of proof remains with him who would argue for the authenticity of a line against which the post-Aristarchean tradition testifies strongly, and with most of the lines considered in this Chapter the case in favour of the line has turned out to be either weak or completely illusory.
NOTES ON CHAPTER V

1. For the text of the scholium, the testimony of our MSS. and the manner of the line's entry into the post-Aristarchean tradition see above, p. 35 and p. 43 n. 5.

2. See above, p. 73.

3. See below, p. 197.

4. Bolling lists it as an interpolation at EE 29 and says at AJP 37 (1916) 457 that the papyrus omits the line "probably correctly".

5. Bolling lists Od. 21. 66 as an interpolation at EE 29 and as a "certain interpolation" at AJP 37 (1916) 453. Van der Valk (TGO 228, 272) regards both 65 and 66 as genuine and their omission as the result of critical excision; but at least he does not contend that a line has been lost after Od. 16. 415, so that an important part of my argument remains entirely untouched by his position.

6. Although it contains over a thousand lines there is not a single plus-verse among them; it even omits all the interpolations in the post-Aristarchean vulgate. See F.G. Kenyon, Classical Texts from Papyri in the British Museum (London 1891) pp. 100-108; the Prolegomena to Allen's edition, pp. 4, 89; Bolling, EE 22f.

7. See L. Friedlaender, Nicanoris Ἱλακης στημυς (Berlin 1857, repr. Amsterdam 1967) p. 274. μετ’ ὀλύγον presents something of a problem, but Friedlaender's comment on the phrase is astonishingly inane: "Illud μετ’ ὀλύγον epitomatoris esse videtur omittentis scilicet quod de versu 804 dixerat Nicanor, infinitivum pro imperativo esse accipendum": first, it is incredible that Nicanor, if his text had included 804 (as Friedlaender assumes it did), should have offered such an extraordinary exegesis of 802-4; and secondly, Nicanor's εἶ μὲν γὰρ συνάπτομεν τὸ "ἀνόρε δῶ, ὑπότερος κε φάδου" proves conclusively that he did not read 804. What, then, are we to make of μετ’ ὀλύγον? Although Nicanor's initial συναπτέον πάντα μέχρι τοῦ "χαλκὸν ἔλαιον" embraced the whole of lines 802f., he then paused to focus at some length on the single word κελέομεν in the middle of 802, and after this it is quite possible that he wrote μετ’ ὀλύγον to refer to the remainder of 802f. Otherwise the phrase will have been interpolated at some stage during the transmission of the scholium by someone whose text of the Iliad included 804.

8. AJP 35 (1914) 143; AJP 37 (1916) 12, 19, 21, 26, 30; EE 4 n. 1, 10, 12, 14, 22. In 1916 (loc. cit. 30) Bolling expressed the view that, as the line was not a δωροθέμενος, its ultimate source was probably a "Ptolemaic text": cf. EE 15 and below, p. 128.

9. See Leaf ad loc.

10. Anhang zu Homers' Ilias, Schulausgabe von K.F. Ameis Heft 8 (Leipzig 1886) ad loc.


13. The reference is to Wecklein 71f., where Wecklein (like Ludwich ad loc.) attributes the emendation to Düntzer.


15. Even if we suppose that this line existed in more than one form, and that Aristarchus's suspicion of it was thereby increased, and that he also found ἐλέοθα in 803 in a few of his MSS., it remains highly unlikely that his conservatism would have allowed him to do more than atheitize 804, putting one version of it (probably the best-attested) in his text and recording the relevant variants in his ὑπομνήματα.

16. D, which omits 804, has ἐλέοθα in 803, but that must be pronounced no more than a feeble attempt at emendation, for the resultant text remains syntactically impossible: as Wecklein says (p. 71), the reading is "nicht brauchbar".


18. See Pfeiffer 217f., 275.

19. See above, p. 16.


22. See e.g. Bolling, EE 171.

23. Περὶ στιγμὴς τῆς παρ' Ὀμήρῳ καὶ τῆς ἐς αὐτῶν διαφορᾶς ἐν τῇ διανοαί. Greek text.

24. See Friedländer (above, n. 7) pp. 104f., 112f.

25. It may perhaps be objected: if the transcript was made so carefully, why should it have contained any accidental omissions? The answer is that in such matters care does not guarantee infallibility, as every scholar should know: even some of our best typists are prone to skip the occasional line, and our most carefully proof-read books and articles retain some misprints. Moreover, it is pretty clear that the text of Zenodotus suffered similarly in the course of transcription in at least one case, the omission of Il. 1. 446/7: See Bolling, EE 43, 63, and for other possible examples his Index p. 257 s.vv. "Aristonicus ... hawks at small game".

27. The very slight homoiomeson 431 δ’ ἡρυγένεα, 432 δή would be most unlikely to constitute such a temptation. T.W. Allen (Papers of the British School at Rome 5 [1910] 70) lists the omission among his homoiographic examples of what he calls the "heads and tails" variety, which he defines as "when the end of one line affects the beginning of the next, or vice versa". Now within this cercecephalous genus Allen evidently regards the omission of 432 as belonging to the species represented by e.g. Od. 19. 18, omitted by his family δ because the word πατρός both ends the preceding line and begins the following line, and Od. 24. 354, omitted by his family γ because the preceding line ends with κάννες and the following line begins with κάννης; but the resemblance of Ἄνδρος at the end of Od. 4. 431 to κατα at the beginning of 433 is so slight that it does not provide a plausible explanation for the omission of 432.

28. A naming of Eidothea in 432 would make the reference of ἢ γ’ in 435 clearer, since she has not been referred to since line 425 and the previous day, but on the other hand the absence of her name would not create a serious difficulty.

29. Cf. Bolling, EE 4f., "The scribe of T seems rather prone to skip with or without temptations to haplography", and the examples given there.

30. The omission of the τημός-line (441) by three MSS. and a papyrus is accidental: see my discussion in Acta Classica 17 (1974) 30, esp. n. 70. Strabo (44) quotes 441 with καὶ τότε for τημός, but this is in all probability due to a slip of memory, as all our MSS. agree in reading τημός, and as Strabo again diverges from the direct tradition in this line in reading υου where all our MSS. have either τά γε or τάς.

31. "The MS. has all the appearance of a comparatively late date, and may perhaps be ascribed to the third or fourth century" - Kenyon (above, n. 6) p. 98, and this dating is accepted by Allen in his edition; but the papyrus is ascribed to the second century by H.J.M. Milne, Catalogue of the Literary Papyri in the British Museum (London 1927) pp. 25f. No. 24.

32. The other lines are 200f., 427 and 441, against all of which there is other papyrus and manuscript evidence; like 381, they fall within lacunae in Pap. 6. 427 and 441 are next in line for discussion in this Chapter. See Bolling, AJP 35 (1914) 141; op. cit. (above, n. 12) 1017f.; Kenyon (above, n. 6) pp. 98f.

33. AJP 35 (1914) 145f.


35. It is very dubious whether there is any (pre-Aristarchean) external evidence against 15f., as even Bolling (EE 229) admits.

36. K.F. Ameis and C. Hentze, Homers Ilias Band 2 Heft 2 (3rd ed. Leipzig 1894) ad loc.; C. Hentze, Anhang to the foregoing, Heft 6 (2nd ed. Leipzig 1900) p. 137. The difficulty led them to take ol’ in the forced sense of "seinem Hause".

37. Loc. cit. (above, n. 34).

39. Cf. also n. 28 above and my discussion of ll. 13. 316 below, pp. 144-6.

40. See above, p. 136.


42. Loc. cit. (above, n. 11) 1186f.


44. Bolling strangely denied the presence of any homoiographic temptation to omission - "für Haplographie [liegt] gar keine Versuchung [vor]" (op. cit. [above, n. 12] 1017 n. 11).

45. See above, p. 136.


47. Cf. Bolling, *AJP* 37 (1916) 27: "a scholium ... which need not be of Alexandrian origin, but merely the result of an early collation of manuscripts"; cf. *AJP* 35 (1914) 142, 145 n. 1.


49. Cf. above, p. 13 n. 33.

50. See e.g. Ludwich (above, n. 46) Vol. I 93-5.

51. See above, p. 4.

52. See above, p. 13 n. 33.

53. *AJP* 35 (1914) 141f., *EE* 11, 21, 256.


55. I include C (saec. xi-xii); the exception is E4 (saec. xi).


57. *AJP* 48 (1927) 378.
58. For similarly slight homoiomesa leading to the omission of lines see e.g. A.E. Housman's edition of Lucan (Oxford 1926) xixf.; Allen, *Papers* (above, n. 27) 70 n. 2.

59. M5 (saec. xiii) and W3 (saec. xiii-xiv).

60. We find ει καλ μάλα καρπερός έστι at II. 5. 410 and (with ...) καρπερός εύη at Od. 8. 139: hiatus at this point in the hexameter is illegitimate. We do find καλ ει μάλα καρπερός εύη at Od. 22. 13, but here, and in all the other occurrences of the combination καλ ει listed by Ebeling, *Lexicon Homericum* (Leipzig 1880-85, repr. Hildesheim 1963) Vol. I 623 IV. 5 s.vv. καλ ει, the αι is shortened by corruption.


62. This point has already been made by H. Erbse (*Hermes* 87 [1959] 294), who regards II. 21. 73 as a post-Aristarchean interpolation.

63. 786 may well be a (pre-Aristarchean) interpolation (see e.g. Bolling, *EE* 89), but if it is, this merely reduces the intervening lines from two to one.

64. This line is omitted by our earliest minuscule, L4, by the early Pal. (1201 A.D.) and U5 (saec. xiii), and by three other MSS. (Allen mentions P5 and U8, Ludwich T); in view of this evidence, combined with the absence of any homoiographic temptation to omission and the fact that there was (as we shall see) a strong tendency to interpolate lines of this type, it is virtually certain that the line is a post-Aristarchean interpolation.

65. See above, p. 4.

66. See above, n. 62.

67. "On the Interpolation of Certain Homeric Formulas", *CP* 17 (1922) 213-21. There are earlier treatments of the same question by Bolling at *AJP* 35 (1914) 134f. and *AJP* 37 (1916) 23f., later treatments at *EE* 11f. and *AL* 17f., 21. See also Wecklein 68-70 for a similar view.

68. E.g. for the evidence against Od. 18. 111a see below, p. 194 with p. 218 n. 5. I have deliberately excluded the extraordinary (and grossly unmetrical) Od. 15. 44a from my list; 45 already ends in an explicit verb of speaking, so that the motive for interpolation mentioned above would be lacking, and I suspect that the line may originally have been intended as a conjectural alternative, in the form of a marginal note, to the athetized 45, i.e. as a version which would remove the objection mentioned in the scholia.

69. This line is omitted by all our MSS. earlier than saec. xiii (if one includes M11, saec. xii-xiii) and by Pap. 9 (saec. vi p.C.) and Pap. 449 (Pack 980, saec. i a.C., a vulgate papyrus), and it is clear from the note in Eustathius ad loc. that his text did
not include the line. Moreover, the absence of the line from the edition of Aristarchus is clearly attested by the note of Aristonicus in the scholia: see e.g. Leaf ad loc. and K. Lehrs, De Aristarchi studiis Homericis (3rd ed. Leipzig 1882, repr. Hildesheim 1964) 338. The passages II. 7. 476f. and II. 320-22, cited by Lehrs in support of the ἀπὸ κανονοῦ construction, are worthy of note.

70. See locc. cit. above, n. 68 ad init., to which one may add that it is clear from Ludwig's apparatus that on this occasion the family k, mentioned by Allen as containing the line, does not include the early L4 (saec. x-xi).

71. For the last line see above, n. 64.

72. CP 17 (1922) 216f.


74. For the dating see above, p. 30 n. 9.

75. AJP 37 (1916) 23, CP 17 (1922) 217, EE 17, II. Ath. ad loc.


77. Above, p. 17.


79. CP 17 (1922) 217 n. 3.

80. Cf. above, p. 13 n. 33.

81. CP 17 (1922) 218.

82. CP 17 (1922) 217.


85. West mentions II. 2. 768f., 17. 270f. Bolling strangely overlooked the irregularity (within his own framework) of the former passage, calling it a "normal construction" (op. cit. [above, n. 83] 384) - but he regarded 769f. as an interpolation, positing a different ending originally to 767 (loc. cit., EE 76, cf. Leaf ad loc.), and he may have been right. Of the second passage Bolling said (op. cit. [above, n. 83] 380) that "the last line may well be an interpolation" - but as West observes, it "is not ... omitted in any MS.", nor is there any other external evidence against it. Another place where ὀφρα means "while", introduces an indicative clause and follows the clause on which it depends is II. 14. 357-9, mentioned by Bolling, op. cit. (above, n. 83) 381. Here the verb
in the ὅψα-clause is present, not imperfect; I do not think
this diminishes the value of the passage as a parallel to
II. 11. 266, but perhaps the fact that the preceding verbs are
imperatives does to some extent.

86. Bolling himself was well aware of this: see op. cit. (above, n. 83) 384f. The passages supporting West's statement, listed by both West and Bolling, are Od. 1. 232f., 8. 180f., 20. 136.


93. See Bolling, EE 138; West 99f.; Erbse's edition of the scholia ad loc.

94. Cf. pp. 70-74 above.

95. Cf. the omission of II. 4. 89 from Pap. 41 and of II. 23. 92 from Pap. 12: see above, pp. 1f., 81, 50f.

96. Die Ilias und Homer (Berlin 1916) 212 n. 2; West 100.

97. I exclude (besides of course II. 12. 130a itself) II. 12. 190a (= 2. 746), found in both Pap. 432 and Pap. 121 (cf. p. 154 above) but absent from all extant post-Aristarchean MSS. and vulgate papyri; it is thus virtually certain that it was absent from the edition of Aristarchus, presumably primarily (if not solely) because it could not muster sufficient diplomatic support to qualify for admission, in which case it would almost certainly be spurious. The line-long periphrasis for Leonteus with no mention of his actual name is odd, as Van der Valk has pointed out (RTSI II 549f.). Presumably the line was inserted to reintroduce Leonteus in the nominative after the change of subject provided by the additional line 189b ὅψαςεν ἓ πεσὼν, ἀπάθεστε ἓ τεῦξε ἑπ' αὐτῷ in the expanded text of both papyri (see West 93, 101f., 120).

98. This line may be an Athenian interpolation: see e.g. Bolling, EE 242f. and Stanford's comment ad loc.

99. Polypoites throws last of the four contestants, but this is so that the poet can achieve a particular dramatic effect by making him overshoot the marks of all the others (844-7): cf. Odysseus's throw at Od. 8. 192.

100. I have now dealt with all the passages in Homer where Polypoites and/or Leonteus are mentioned except II. 6. 29: here Polypoites kills the Trojan Astyalus in a context in which Leonteus is not mentioned.
101. I refrain from using the awkwardness seen by H. Fränkel (see West 101 n. 77) in the repetition ὑδας ὑπερθύμβους (128), ὑδον ὑπερθύμβου (130a) as an argument against the authenticity of 130a: if Homer had found the epithet metrically convenient it is doubtful whether he would have baulked at repeating it in such circumstances.

102. For 190a see above, n. 97.

103. This seems to be the suggestion of Van der Valk at RTSI II 481, 555.

104. For the dating see above, n. 31.

105. Since the sparse collation of its original publication, this last papyrus (= Pap. Oxy. 15. 1817) has been re-edited on a lavish scale by W. Lameere, Aperçus de paléographie homérique (Paris, Brussels, Anvers and Amsterdam 1960) 175-204, 267 and Plate 10. It is mutilated at the crucial point, and the surviving remains fall tantalizingly short of demonstrating absolutely conclusively that it omitted 604/5, but considerations of space strongly suggest - what the evidence of the rest of the tradition in any case makes all but certain - that it did: Lameere (p. 184) deals competently with the vertical spacing but fails to point out that the horizontal spacing also favours the restoration of the longer [τέρπαινον] over that of the shorter [φομπέοι] in line 605: this is clear from my calculations (which have taken into account the different widths of different letters) based on a comparison with the lines below in the photograph (Plate 10).


108. Cf. Monro ad loc.: "The picture of the dance and the two tumblers is simpler and more intelligible without the player on the lyre."

109. For similar views see Wecklein 70f. and P. Chantraine on p. 33 of P. Mazon's Introduction à l'Iliade (Paris 1942).

110. For this conclusion cf. also Z. Ritošk, Acta Antiqua 19 (1971) 202 n. 8: the omission of the line "kann man nicht einfach mit Aristarch's Einfluss abtun, weil dann auch die übrigen Plus-Verse verschwunden sein müssten". The critical σημών in this papyrus, including the διάλεκτοι attached to 608abcd, remain something of a problem (cf. West 132f.) but do not shake the validity of this conclusion. I would suggest that someone outside the circle of professional Alexandrian scholars, perhaps a schoolmaster, made some notes of his own on ll. 19 and employed the fashionable and useful Alexandrian symbols to link his copy of the text with his notes.


113. See Lehrs (above, n. 69) 138-40.

114. Van der Valk is not unaware of this difficulty for his hypothesis, but his attempt to deal with it (RTSI II 530) is patent special pleading.


116. Of the two examples of this sense given by L-S-J, Aristophanes Frogs 230 is certain, the Homeric Hymn to Apollo 206 doubtful, as the verb here could be taken in the sense "dancing" (cf. lines 201f.).

117. See Bolling, EE 226-9, and above, p. 161.

118. For this assumption cf. A. Heubeck, Der Odyssee-Dichter und die Ilias (Erlangen 1954).

119. My discussion in this and the following paragraph is based on the assumption that the passage describing the decoration on the Shield of Achilles (II. 18. 483-608) is genuine; this is also the belief of Van der Valk (RTSI II 35f.) and appears to be tacitly assumed by Ritoök. The most recent discussion of the Shield, R.S. Shannon III, The Arms of Achilles and Homeric Compositional Technique (Mnemosyne Suppl. 36 [Leiden 1975]) 21-30, defends its authenticity, but makes a completely unwarranted assumption about the external evidence: "Despite Zenodotos' well known ruthlessness in athetizing lines, his athetesis of this passage indicates that some of the manuscripts available to him did not contain it. Zenodotos was not only 'satisfied' with the introductory lines about the shield; he was also dissatisfied with the attestation of the remaining lines" (p. 22). All this is based merely on Schol. A Ζηνόδωτος ἤθετηκεν ..., ἐρκεφεῖς τῇ κεφαλαίῳ προεκάσθεν - but this does not entitle Shannon to infer "dissatisfied with the attestation of the remaining lines": the words underlined should be deleted. Thus Leaf (Vol. II p. 607 [14]) understandably felt able to say, "Zenodotos' judgment appears ... to have been based on aesthetic grounds alone"; only Bolling's unproven theory that Zenodotus never athetized a line "unless its attestation seemed to him seriously defective" (ALI 30) could form a possible basis for the inference Shannon draws - and yet he appears to have rejected this theory in speaking of "Zenodotos' well known ruthlessness in athetizing lines". Moreover, Bolling himself concedes (ALI 161) that "the passage is the one that has caused me the greatest difficulty" in defending his theory; one possible solution, he believes (ALI 30, 162), is that the passage was deliberately omitted, like the Catalogue of II. 2 in part of the post-Aristarchean tradition, by some scribes who felt they could save space by omitting this long digression. Thus even if one were to accept Bolling's general theory about Zenodotus's atheteses, this would not necessarily imply any genuine external evidence against the passage. Shannon's own explanation of what he believes to have been the weakness in its attestation - that this "was probably at least partially due to its language and subject matter" (p. 22) - seems to me to have nothing in its
favour: pre-Zenodotean readers and scribes were not nineteenth-century German Analysts.

Moreover, even if we suppose that the decoration on the Shield was not part of Homer's Iliad, the argument in the paragraph of my text just completed will remain unaffected if Od. 4. 17-19 is an interpolation post-dating the interpolation of the Shield or if the Shield was interpolated from some early epic known to the Odyssey-poet, and the argument of my next paragraph will be affected only in part.

120. Viz. at Od. 13. 27, minus ворота; at Od. 4. 17/18, as we have seen, it may well be part of an interpolation.


122. For the last two papyri see Bolling, EE 255.

123. For further details of the minuscule attestation see p. 648 of E. Heitsch, "Ilias B 557/8", Hermes 96 (1968) 641-60.

124. I do not propose to quote and discuss all these passages. A fairly comprehensive collection may be found in Allen's edition ad loc., and for collections with discussions see Allen, Homer: the Origins and the Transmission (Oxford 1924, repr. 1969) 234-7; R. Merkelbach, RhM 95 (1952) 23 (3) and 27-31 = Untersuchungen zur Odyssee (Zetemata Heft 2 [2nd ed. Munich 1969]) 241 (3) and 243-6; J.A. Davison, TAPA 86 (1955) 15-18; Heitsch (above, n. 123), esp. 649-57.

125. For the argument of these two comments cf. the note of Aristonicus (Schol. A) on II. 11. 6, which shows that Aristarchus drew attention to the poet's description, in 11. 5-9, of the position of the ships of Aias at one of the ends of the line of ships. Thus, Aristarchus argued, if Idomeneus was stationed next to Aias, the Athenians could not have been. For Aristarchus's special interest in the question of the νοθετεως see Lehrs (above, n. 69) 221-4, Pfeiffer 213, 258.

126. Cf. U. von Wilamowitz, Homeriche Untersuchungen (Berlin 1884) 238f.; Ludwig (above, n. 46) Vol. II 397f.; Bolling, AJP 35 (1914) 137f.; AJP 37 (1916) 8, 19, 26, 29f.; EE 10, 12, 14, 16f., 49; "καρατατεον  = ὧθετελ", CQ 22 (1928) 101-6; ALI 20f.; Wecklein 67f.; P. Von der Mühll, Kritisches Hypomnema zur Ilias (Basle 1952) 57; Van der Valk, RTSI II 487, 519-21; Heitsch (above, n. 123) 645-7.

127. T.W. Allen, CR 15 (1901) 8f.; The Homeric Catalogue of Ships (Oxford 1921) 56; Origins (above, n. 124) 235 n. 2; CQ 22 (1928) 203f.; Prolegomena 197; J.T. Sheppard, CR 41 (1927) 128; P. Collart, RPh 7 (1933) 46; H. Erbse, Hermes 87 (1959) 295 n. 1. Bolling, CQ, answers Allen and Sheppard; Bolling, ALI, answers Collart; Van der Valk and Heitsch answer Erbse (cf. n. 126 above). J.A. Davison (TAPA 86 [1955] 17 n. 27) is doubly wrong when he
says, "Aristarchus had doubts about 557-58 (Schol. A to II. 3. 230, 4. 273)" (my emphasis): first, this understates the harshness of Aristarchus's treatment of 558, and secondly, we have no evidence that Aristarchus suspected 557.

128. In this phrase ὑποτάσσετω can hardly be taken in the technical sense "add by way of interpolation" desired by Ludwig (op. cit. [above, n. 46] Vol. II 398 n. 343), Bolling (CQ 22 [1928] 104) and Heitsch (op. cit. [above, n. 123] 646f.), because "those who 'add' Aias to the Athenians" would have to mean "those who interpolate II. 2. 557f.", not "those who interpolate II. 2. 558" - yet we know from the behaviour of the post-Aristarchean tradition and from Aristonicus's comment (1) above that Aristarchus omitted only 558, not 557 as well. It is to Heitsch's credit that he has not overlooked this difficulty (op. cit. [above, n. 123] 647 n. 1), but his attempt to evade it is unconvincing. He argues that the scholium is an abbreviation for "Der auf die Athener folgende Aias-Abschnitt ist so, wie er überliefert ist, nicht in Ordnung, sondern enthält (mindestens) in 558 eine Interpolation". However, this is not the natural meaning of the words; there is no evidence that Aristarchus athetized or even suspected 557, and even if we were to suppose that he did, there would remain the difficulty that elsewhere in the scholia ὑποτάσσετω in the sense of "interpolate" is always used only of lines totally absent from the edition of Aristarchus, as Heitsch himself has pointed out (pp. 646f.; Schol. A on II. 5. 807, listed but not discussed by Heitsch, was regarded by Bolling as an exception [EE 89f.], but I have argued against this position above, pp. 4-6). Rather, τοὺς ὑποτάσσοντας must surely mean "those who position Aias under the wing of the Athenians", taking up the word τὰξις in the previous phrase (and cf. ἑιάσετο in comment (1)). This interpretation is also favoured by the fact that the scholium speaks of people (τοὺς Ἀθηναίος, τὸν Ἀλίμον [taking up Ἀλαιος]) and not (as we should have expected if the reference were to interpolation) verses. Now in this context "those who position Aias under the wing of the Athenians" are precisely those who interpolate II. 2. 558, so that while ὑποτάσσετω does not mean "interpolate", the phrase in which it occurs does (as it happens) contain an implicit reference to interpolation.

129. Quintilian's reference to the line (5. 11. 40) as one "qui tamen ipse non in omni editione reperitur" need not imply that it was present in most of the texts current in his own day: it is hardly likely that he would have conducted anything resembling an extensive collation, and he may well have commented as he did either (a) because he knew the story behind the line but found it absent from his own text of the Iliad, or (b) because he knew some statement about its omission which actually referred to pre-Aristarchean MSS. - e.g. ἐν τοῖς μετὰ τούτον [557] φέρεται ὁτις ...", or perhaps even Aristonicus's comment (1) above.

130. See above, p. 4.

131. AJP 35 (1914) 137, AJP 37 (1916) 29f.

132. CQ 22 (1928) 105.
133. For this conclusion cf. Ludwich (above, n. 46) Vol. II 396-8, Cauer (above, n. 34) 114.

134. AJP 37 (1916) 29f., EE 16f., 72f.

135. For our sources for the story see above, n. 124.

136. The interpretation of A. Dihle (Homer-Probleme [Opladen 1970] 99f.), which supposes that Plutarch is speaking of the interpolation of both lines (557f.), strikes me as extremely implausible. Plutarch says of Solon: ἐμβασάντα γὰρ αὐτὸν ἔτος εἰς Νεὼν Κατάλογον ἐπὶ τῆς ὄικης ἀναγνώσαι "Ἄκα ... νῆας, τί σῆμε ... φάλαγγες ". Now it is true that, as Dihle says, ἔτος can be used of a passage longer than a single line; but here one must surely suppose that Plutarch cites the line Ἀκα ... νῆας merely to show where the interpolation came in and what the lines actually read out by Solon (according to the story) were - for the story could not have made Solon quote 558 without its preceding context. The quoted couplet is the object not of ἐμβασάντα but of ἀναγνώσαι, and (however one punctuates) the phraseology and syntax militate strongly against the assumption that ἔτος and the quotation following ἀναγνώσαι are co-extensive. Cf. Strabo 394 ... Ἔλλωνα παραγγέλθαντα ἐν τῷ τῶν νεῶν Κατάλογῳ μετὰ τὸ ἔτος τούτο, "Ἄκα ... νῆας", ἐξης τούτο, "σῆμε ... φάλαγγες" ... ; Diogenes Laertius 1. 48 ... ἔγγραφαν αὐτὸν [sc. τὸν Ἐλλώνα] εἰς τὸν Κατάλογον ... μετὰ τοῦ "Ἄκα ... νῆας" <; τὸν> "σῆμε ... φάλαγγες". This interpretation is strongly supported by the fact that all our numerous other sources which explicitly mention the interpolation of 558 refer to the interpolation of this single line, not the couplet 557f. There is one apparent exception (ignored, surprisingly, by Dihle, who is content to call Plutarch the "einzige Zeuge für einen Homer-Text ohne [Il. 2. 557f.]" [p. 100]), the scholium on Demosthenes 19. 251 cited in Allen's edition ad loc.: ... ἀμφισβητοῦσιν Μεγαρῶν καὶ Ὄμηρονας περὶ ἐλαμένον ἐν ἕκεν δήμην ἐπὶ τῶν ὀμηρωμάτων ἐκὼν τὴν ἀπόδειξιν ἐπούσατο στίχον παρενθέσεις πεπλασμένον, "Ἁκα ... νῆας", καὶ προσέθηκε τόδε τὸν στίχον, "σῆμε ... φάλαγγες". τούτου πλάσας τὸν στίχον ἐνίκησεν ἐν τῇ δήκη τούς Μεγαρές ... . However, I suggest that in all probability there is a lacuna in the text here. How did Solon prove his point? στίχον παρενθέσεις πεπλασμένον - "by interpolating a line" - according to the beginning of the note. How did Solon beat the Megarians at the arbitration? τούτου πλάσας τὸν στίχον - "by forging this line [singular]", viz. the line στίχος κ.τ.λ. [558] just cited - according to the end of the note. The wording of the beginning is taken up in the wording of the end, and both refer to a single line. If Solon actually won his point and beat the Megarians by interpolating two lines, why does the scholiast not write στίχους ... πεπλασμένους at the beginning and τοῦτος ... τοὺς στίχους at the end, omitting the clause καὶ ... στίχου between the lines? I suggest that after πεπλασμένου some words have fallen out, and that we should restore along the following lines: ... πεπλασμένου: "ἀνέγυρω γὰρ ἐπὶ τῆς ὄικης τῶν στίχου" Ἄκα ... κ.τ.λ. (as in the paradosis). This will remove both the contradiction in the paradosis and the apparent reference to the interpolation of 557. The loss can plausibly be put down to the homoioteleuton -ον. However, if this argument is not
accepted, the Demosthenes scholium will presumably be a
collation of the Megarian allegation (Diogenes Laertius I. 57)
that Solon interpolated ι. 2. 546 "..." (i.e. 546-57?) and the (later?) story that he interpolated only 558 (see below
in my text); and the same judgment will apply to Plutarch if
Dihle's interpretation happens to be correct. Otherwise we
could suppose that the Demosthenes scholium stems from this very
passage in Plutarch (so Erbse in his edition of the Iliad-
scholia in his commentary on the scholium on ι. 2. 558) and
that the Demesthones scholiast (like Dihle!) has misunderstood
Plutarch. Finally, one cannot conclude without discussing an
extraordinary story in the late pseudo-Herodotean Life of Homer
372-98; Wilamowitz ι. u. H. (above, n. 96) 414-6] dates the
Life between c. 130 and 80 B.C.). This story, which has
recently been discussed by F. Krafft (Vergleichende Untersuchungen
zu Homer und Hesiod [Hypomnemata Heft 6, Göttingen 1963] 16f.),
alleges that Homer himself added various specified lines eulogiz-
ing Athens to his poems before setting out on a trip to the Greek
mainland (including Athens). This should presumably be seen as
a bizarre attempt to answer critics who held the lines in question
to be non-Homeric Athenian interpolations, whether these critics
were the grammarians (so Wilamowitz) or Megarian irredentists
(so Krafft) or both. What were the lines in question? Od.
7. 80f.; ι. 2. 547f., 552-4, 558 — but where I have written
"558" Kraft has written "557f.", so let us look at the wording
of the Life (lines 390-93): Αὐάντα δὲ τὸν Ἐλευθέρων καὶ
Ἐλευμύλλον ἑν Νέων Καταλόγῳ ἔσταξε [sc. ὁ "Ομήρος] πρὸς
Ἄθηναλόους, λέγων τὰς (v.1. ἄδει): "Ἄδας ... νής | στῆς ... φάλαγγες". The preceding context, to be sure, is the additions
made by Homer, but Αὐάντα ... ἔσταξε πρὸς Ἀθηναλόους most
naturally suggests the addition merely of 558 (cf. τοὺς
ὑποτάσσοντας τοῖς Ἀθηναλόους τὸν Τελαμάνδον in Aristarchus's
comment (2) quoted above in my text with my commentary in n. 125),
and, as in the case of Plutarch, Ἄδας ... νής is presumably
added to show where the addition came in — the couplet is the
object not of a verb "adding" but of λέγων (cf. Plutarch's
ἀναγνώρισα). However, Krafft's interpretation is not impossible,
and in the wider context of the Athens-lines in general a
conflation between the story of the interpolation of 558 alone
and one which alleged the interpolation of 546-57 (or some of
these lines, including 557) is less implausible than in the case
of Plutarch. My concern to make a clear distinction between (1)
the story that Solon (or Peisistratus) interpolated 558 for the
particular occasion of the Spartan arbitration over Salamis and
(2) Megarian allegations that the passage 546-57 was an
interpolation stems from a desire to stress that (1) certainly
need not imply that 558 was present in the standard Athenian
text (whereas (2), on the natural interpretation, presupposes
that 546-57 were present in this text).

137. AJP 37 (1916) 29, EE 73.


139. Catalogue (above, n. 127) 57; a later suggestion by Allen
(Origins [above, n. 124] 236f.) — that the insertion of the line
may have originated with the Athenian family of the Philaidæ,
which claimed descent from Aias - cannot be ruled out either.

140. Merkelbach's treatment of this comment (RhM p. 29 (b), = Untersuchungen pp. 244f. (b) - see above, n. 124) is misleading, in that he ignores Plutarch's actual continuation in favour of a conjectural link-up with the Dieuchidas-fragment (see below in my text, p. 169, with n. 143).

141. Leaf (ad loc.) thinks it unlikely that there was a Spartan arbitration at all; so also, apparently, Bolling (AJP 37 [1916] 29f., EE 73). However, the story of the arbitration is apparently accepted lock, stock and barrel by D.J. Mosley on p. 185 of his and F.E. Adcock's Diplomacy in Ancient Greece (London 1975): "The parties resorted to arbitration by Sparta and each tried to reinforce its claim by appeal to the authority of Homer in quoting the Catalogue of Allies and each accused the other of misquotation." Adcock himself (Cambridge Ancient History Vol. IV [Cambridge 1926] 61) accepted that there was an arbitration but somewhat cautiously merely ascribed the story of the interpolation and quotation of II. 2. 558 to "an ancient tradition".


143. For the relevant passage (which presents some textual problems) see Leaf, Vol. I pp. xviiif.; Monro, Appendix 407-9; Merkelbach (above, n. 124) RhM pp. 24f. (1) = Untersuchungen pp. 240f. (1); Krafft (above, p. 190 n. 136) 12f. Merkelbach's restoration of the lacuna (almost identical with Leaf's) strikes me as the most plausible. Krafft's attempt to deny that there is any reference by Dieuchidas here to any interpolation strikes me as far-fetched, though he is entirely right in his argument (p. 13) that there is no reason to suppose that Dieuchidas was the source of Diogenes's reference at I. 48 to the story of Solon's interpolation of II. 2. 558.

144. It is actually not necessary to see the single line assigned to Aias, 557, as an Athenian attempt to downgrade Salamis, a replacement for a longer authentic description (whether that occurred after 556 or elsewhere). Instead, the brevity of the reference could be seen as a rather half-hearted attempt to reconcile an originally Aiasless Catalogue with the main body of the Iliad: cf. D.L. Page, History and the Homeric Iliad (Berkeley 1959) 147: "... somebody at some time inserted him [sc. Aias] apologetically with a single lonely line to his credit, and much later a second line was added by somebody else." Alternatively, the reference could conceivably be pre-Iliadic, the brevity being due to the smallness of the contingent contributed by Aias, plausibly reflecting the small size of his native island. On early links between Salamis and Athens (though without 558 these are not necessarily implied in the Catalogue) see Whitman (above, n. 121).

146. From ξανθότρυχος to πυρρότρυχος. To the discussion of Cope and Sandys (above, n. 145) one may add that at Aristophanes Frogs 730 πυρρότρυχος, whatever its precise meaning, is certainly used in a pejorative sense: see e.g. the commentary on the play by W.B. Stanford (London 1958) ad loc.


148. I have not overlooked the possibility that the reference to the line in Plutarch Life of Solon 10 comes from Hereas of Megara, but Dihle (op. cit. [above, n. 136 ad init.] 98) has shown that this is very doubtful and that a fourth-century date for Hereas is less than certain. Attempts to establish an earlier terminus ante quem will be discussed presently.

149. See P. Brandt, Corpusculum poesis epicae Graecae ludibundae Vol. I (Leipzig 1888) pp. 56-8 for the date of Matro and pp. 60-95 for text and commentary. A long citation at Athenaeus 134D-137C comprises almost the whole of the surviving portion of Matro's 'Αττικόν Δείπνον.


154. Die homerische Schiffskatalog und die Ilias (Cologne and Opladen 1958) 246.

155. Heitsch (p. 644) is cautious about the dating - "eine relativ frühe Zeit, sagen wir das 6. Jhdt." - and Jachmann (p. 246) refers to Wilamowitz's "nicht älter als das sechste Jahrhundert" (my emphasis) with approval; but the latest discussion - in N.J. Richardson's edition of the Hymn (Oxford 1974), pp. 5-11, esp. 10f. - favours "a terminus ante of the mid sixth century" (p. 10) and a seventh-century date rather than a sixth-century one (p. 11).

156. Cf. Richardson (above, n. 155) in his commentary ad loc. (p. 281); he also compares II. 5. 368 ξενός εἰπέως ἔτησε, to which one could add II. 5. 755 ξενός εἰπέως στησασα.


158. One may add Od. 14. 49 εἶσεν δ' εἰσαγαγόν, Od. 10. 233 εἶσεν δ' εἰσαγαγόν, Od. 10. 314 and 366 εἶσεν δ' εἰσαγαγόν, in each case at the beginning of a line.

159. Op. cit. (above, n. 123) 643f. - though the fact that one adverbial clause begins with δ' and the other with έν' when metrically and semantically the same word would have been possible in both places militates against this hypothesis to some extent.


164. See Wilamowitz, *HU* (above, n. 126) 237, 239, 242, 244 - though against this conjecture see Jachmann himself (above, n. 154) 246ff. n. 13.

165. I think it possible that the two lines were composed quite independently of one another: the Homeric parallels outside these two places are adequate to give this hypothesis a certain degree of plausibility, and cf. n. 159 above. If this hypothesis happens to be correct, then of course *Hymn to Demeter* 384 will not be able to tell us anything significant about the date when ll. 2. 558 was concocted.

166. *Catalogue* (above, n. 127) 56ff.


170. See above, n. 142.

171. *Catalogue* (above, n. 166) 57.


174. Allen once put forward a similar excision-theory (though from a different point of view) - CR 15 (1901) 9 - but later understandably retracted it "to avoid the imputation of special pleading" - *Catalogue* (above, n. 127) 56. Cauer (op. cit. [above, n. 34] 114) suggested, less implausibly, that Peisistratus did try to have the line interpolated, but with only limited success.

175. Cf. Wilamowitz, *HU* (above, n. 126) 238: "Aristarch istweit entfernt, die pisistratischen interpolationen nicht zu kennen: er wagt auf grund derselben, was er sehr selten wagt, er wirft den vers ganz und gar aus." Wilamowitz, however, assumed that Zenodotus and Aristophanes had also omitted the line (op. cit. 239). The hypothesis that Aristarchus was motivated here by acceptance of a story of Solonian or Peisistratean interpolation is not disproved by the absence of any mention of such motivation in the scholia (pace Ludwich [above, n. 46] Vol II 398ff.), since the only surviving reports of his motivation in the scholia are the two comments from Aristonicus quoted in my text on passages which Aristarchus took to be inconsistent with ll. 2. 558, and in these contexts this inconsistency was the only objection to the line which it was relevant to state. Moreover, Strabo 394 makes it likely that Aristarchus was aware that the line was assigned to Solon or Peisistratus, however much weight he may have attached to such reports: see Heitsch (above, n. 123) 649-51, esp. 650 n. 2.
CHAPTER VI
APPLICATIONS

It is now time for me to fulfil the promise made in my Introduction to give some indication of the relevance of my conclusions on the numerus versuum to the areas of formula-analysis, literary criticism and the Homeric Question. I shall use these three areas as headings in what follows, although the division is not intended as anything more than a rough and ready one: naturally enough, the three areas overlap at various points. My treatment aims to be illustrative rather than comprehensive, but my illustrations do have a comprehensive moral: in reading and writing books and articles on Homer, scholars should constantly beware of the danger of error through the acceptance as genuine of lines against which there is strong external evidence.

(1) Formula-analysis

At the outset of a recent article analysing the various ways in which Homer introduces speeches, M.W. Edwards states (p. 2), "For the purposes of this paper ... I have taken the vulgate text as in the Oxford Classical Text and am ignoring the fact that some of the lines with which I am dealing, including some that I much admire, have sometimes been considered un-Homeric." He cites (or rather mis-cites) an article by Bolling. Now in fact Bolling did go too far here: he confidently condemned a few speech-introductions against which there was actually only very slender evidence. However, it is clear that Edwards fails to appreciate the strength of the evidence against the other speech-introductions condemned by Bolling: I count twelve lines read by Allen (II. 3. 389, 10. 191, 13. 480, 17. 219, 326, 585, 21. 73, 480, Od. 10. 265, 430, 482, 18. lIla) which the external evidence indicates were absent from the edition of Aristarchus. The arbitrary and unscientific nature of the procedure of following Allen blindfold can be graphically illustrated by the fact that Edwards accepts Od. 10. 265 and 18. lIla because they are read by Allen, although the evidence against them is overwhelmingly strong (Allen's apparatus records that each is absent from all his MSS. except two families and two other MSS., and there is also papyrus evidence against each), but implicitly rejects II. 12. 162a, 13. 218 and Od. 10. 225a because they are not read by
Allen, although they are no more poorly attested than the accepted Od. 10. 265 and 18. 111a. One effect of Edwards's procedure is his treatment of the verb λέειν on p. 19: the exclusion from the text of the interpolated 11. 21. 73 and Od. 10. 265 would have made a substantial difference here.

One particular type of speech-introduction is the line ending έπει Περλεντα προσηθα. In raising again the question of Homer's winged words, P. Vivante includes in his discussion six lines - 11. 10. 191, 17. 219, 21. 73, Od. 10. 265, 430, 482 - which the external evidence indicates were absent from the edition of Aristarchus. At one point (p. 9 n. 1) he prefaces a list of these lines (plus 11. 4. 369, of dubious status) with the comment that such a line "is omitted by some manuscripts in several instances in which it might have seemed superfluous". He seems to be thinking here of deliberate curtailment of the genuine text, and has actually incorporated four of the lines listed into his spontaneity-theory earlier in his text (11. 4. 369 and 10. 191, p. 6, 11. 17. 219 and 21. 73, p. 7) without expressing any doubts there about their authenticity. While the interpolated Od. 10. 265 and 430 would also fit his spontaneity-theory well enough, the interpolated Od. 10. 482 would hardly, for there Odysseus's speech must be premeditated rather than spontaneous, and if the line were genuine Vivante's attrition-explanation ("through its extensive use, the phrase tends to lose its original value" - p. 9 n. 1) would indeed, within the framework of his own general theory, be a possible way of accounting for it. But of course if the line is an interpolation it ceases to be an anomaly within that theory requiring special explanation. G.M. Calhoun found his own theory about winged words - that they indicate heightened emotion, animation, urgency - compatible with the authenticity of these same seven lines, and evidently assumed that this told very strongly in favour of their authenticity; but the rejection of Calhoun's theory, in different ways and for different reasons, by both Vivante and Milman Parry shows up the danger of elevating the internal evidence at the expense of the external evidence in this way - though the value of Calhoun's compatibility as evidence in favour of the disputed lines would in any case be very slight. The fundamental point to be made is that any theory on the controversial έπει Περλεντα, if it is to be soundly based, must start by eliminating those lines which the external evidence
exposes as interpolations.

C.M. Bowra, in discussing the development of Homeric formulae, states: "First, we may perhaps assume that two different phrases have a common origin. The Hours are entrusted with watching the entrances and exits of Olympus: ἡμέν ἀνακλίνας τυκινὸν νέφος ἥ' ἐπιθέταιναι [Il. 8. 395]. So Odysseus protects the entrances and exits of the Wooden Horse: ἡμέν ἀνακλίνας τυκινὸν λόχον ἥ' ἐπιθέταιναι [Od. 11. 525]. We may surmise that νέφος and λόχον are both variations on some word for 'door' and have been substituted for it to provide a more precise picture." However, this ingenious explanation is thrown into considerable doubt in the light of the scholium, ignored by Bowra (but now confirmed, as we have seen, by a papyrus), which tells us that Od. 11. 525, although discussed by Aristarchus, was absent from his edition. We know, of course, that it is pre-Aristarchean, but the likelihood of its going back to a period when creative oral poets were still composing within the formulaic epic tradition is very dubious; and if Od. 11. 525 is post-Homeric and post-oral, there is much less reason for believing that Il. 8. 395 is a formula at all.

(2) Literary Criticism

In a recent article W.C. Scott gives a detailed structural analysis of Od. 1. 125-48. Line 148, a whole-line formula, plays an important part in his discussion. Nowhere does he show any awareness of a question about the line's authenticity. But the external evidence cries out against it. Not only is it completely omitted by Pap. 106 (Pack 1024, saec. i p. C.) and by four of our minuscules, including the oldest, L4, but it is placed immediately after 146 in some MSS., after 147 in others, after 148a in others and after 149 in others, and this mobility is a further indication that it has spread to our later minuscules by contaminatio via the margins of exemplars and that it was absent from the edition of Aristarchus. Again, one may note the arbitrariness involved in following the printed Wolfian vulgate blindfold: Scott includes 148 in his analysis, because it is part of that vulgate, but excludes 148a, because it is not - yet the manuscript evidence against 148 is as strong as that against 148a (if we take misplacement as tantamount to omission). One may add that there is also internal evidence against both lines, as has recently been well argued by A. Heubeck - although it is noteworthy that he nowhere introduces the external
Scott's discussion is also marred by his unquestioning acceptance of other weakly-attested lines as genuine: the meal-preparation formula Od. 10. 368-72, omitted by Pap. 8 (Pack 1092, saec. i-ii p. C.), five of Allen's families (including our oldest minuscule, L4), Pal., and also (according to Ludwich ad loc.) T and Z, and bracketed in three other MSS. - Scott's pp. 544, 546 and 549;\(^\text{20}\) Od. 10. 315 - Scott's p. 546, cf. my pp. 125-7.

(3) The Homeric Question

The Analyst R. Merkelbach,\(^\text{21}\) in apportioning the authorship of Od. 10 between "A", the "Dichter der älteren Odyssee", and "B", the Bearbeiter, assigns Od. 10. 201f. without hesitation to "A". However, this couplet is in all probability a post-Aristarchean interpolation: it is omitted by our two earliest minuscules, L4 and L8, and a number of other MSS.; there is no homoiographic temptation to omission; and the lines' lack of originality (10. 202 = 10. 568; 10. 201 first half \(= 16. 216\) first half, 10. 201 second half is a frequently-recurring formula, and the whole line is very like 11. 391) is compatible with the hypothesis of interpolation, as is the fact that the couplet can be seen as concordance interpolation based on 10. 566-8 (10. 198 = 566, 201 corresponds in sense to 567, 202 = 568). Merkelbach, however, ignores the external evidence against the lines.

In 1970 D.M. Gunn\(^\text{22}\) suggested that two oddities in the Odyssey could be satisfactorily accounted for only by A.B. Lord's theory of the oral dictated text:\(^\text{23}\) in each case the poet, in the course of dictation, makes a slip, then realizes that he has done so and tries to retrieve the situation, but with only partial success, because the damage has already been done and nescit vox missa reverti. Gunn's first oddity was the late stage of the libation-scene at Od. 15. 147ff., i.e. when Telemachus's departure is already under way (146). However, in reply G.P. Rose\(^\text{24}\) has argued very persuasively that it is a mistake to talk about "narrative inconsistency" here and that the last-minute impulse to pour a last libation is typical of Menelaus's "urge to be a grand host", while M.W. Edwards, whose own views on the transmission of Homer differ from those of Lord and Gunn,\(^\text{25}\) argues (less plausibly, in my view) that "what is involved here is not a moment's error in singing, but an attempt to combine (in shortened form) more type-scenes than the poet can comfortably handle within
one episode." But that still leaves us with Gunn's second oddity, which surrounds Od. 5. 91 ἄλλη ἔπεο προτέρω, ένα τοι πάρ ἔξωνα θεών.

In this line Calypso invites the visiting Hermes in for a meal—but she has already given him a seat (86) and, as Gunn says, in 92f. "she proceeds to ignore the implications of ἔπεο προτέρω and sets before the god a table and food". Gunn notes that in the parallel passage, II. 18. 385-90, line 387 (= Od. 5. 91) is followed by Charis's giving the visiting Thetis a seat. He concludes that in the Odyssey-passage the poet inadvertently made Hermes sit down too soon; while delivering line 91 he suddenly realized his mistake, and therefore omitted the seating-verses which follow the line in the Iliad-scene: "by moving straight to the laying of the table, he manages to avoid making an already seated Hermes sit down again, which would have been a prominent error, but at the small cost of ignoring ἄλλη ἔπεο προτέρω". However, if one draws the right conclusion from the very strong manuscript evidence against Od. 5. 91, viz. that the line is an interpolation, the "narrative inconsistency" disappears completely and the second part of Gunn's attempt to substantiate Lord's "oral dictated text" theory turns out to have been built on a foundation of illusion. The line is omitted by four of Allen's families and by four other MSS. (I have gleaned J and T from Ludwich's apparatus): this group includes our two earliest minuscules, L4 and L8. It is to Gunn's credit that he has not completely ignored this evidence, but unfortunately he is content to refer briefly to Van der Valk, TCO 272: "Van der Valk ... cites this omission as one example of ... the Alexandrian principle of abridgement of the text." Of course the external evidence against the line is supported by the internal evidence: not only does its presence lead to the inconsistency discussed by Gunn, but it can easily be seen as an example of concordance interpolation: II. 18. 385, cf. Od. 5. 87; II. 18. 386 = Od. 5. 88 (change of gender); II. 18. 387 = Od. 5. 91; and the general situation is very similar (one immortal visiting another).

Now while Rose has nothing to say about this example of Gunn's, Edwards deals with it at length. The obvious, and correct, way for him to refute Gunn's argument here in favour of the oral dictated text would have been to argue from the weak attestation that the line was an interpolation: instead, like Gunn, he assumes that the omission of 91 is due to Alexandrian excision and embarks on an elaborate and implausible explanation whose gist is that the poor
unfortunate poet is so bogged down in his complex permutations, combinations and variations of type-scenes that he does not know whether he is coming or going. Edwards then goes on to compare this with another scene whose incongruities he tries to explain in a similar way, but ironically this only leads him deeper into the mire. The reason is that he has failed to realize that the passage Od. 10. 368-72 is an interpolation: it is omitted, as we have seen, by a papyrus and many of our MSS. He does not refer explicitly to this evidence, but seems to have it in mind when he says that the "verses should not be omitted" (p. 68) - again he would presumably attribute the manuscript omissions to Alexandrian tampering with the genuine text. "But who bids Odysseus eat in 375?" Edwards asks (p. 69). "Circe, one supposes, but the tamiē has been the subject of the preceding verb and participles. ... the poet has inserted a new idea without taking care to carry his hearers along with him. ... a change of subject has not been well indicated." But without the interpolated 368-72 there is clearly no change of subject at 373, whether one thinks the subject of 366f. and 373 is Circe (as Edwards does) or still the fourth nymph-servant of 358 (as I do).

To conclude: to a large extent a theory has been both built up (by Gunn) and demolished (by Edwards) on the basis of a shared illusion. Discussion of an issue of the utmost importance to Homerists - how the poems were transmitted immediately after their composition - has been seriously vitiated by ignorance of the true significance of omissions by our MSS.

As my final example in this section I shall discuss Od. 15. 113-19 at some length. I shall start by reprinting Od. 15. 109-30 exactly as the passage appears in Monro's edition:
The question of the authenticity of Od. 15. 113-19 (= 4. 613-19) has been much debated. The lines are omitted by two of Allen's families, b (fathered by Pal., 1201 A.D.) and i (including M2, saec. xiii), and by two other minuscules (one of which is H3, saec. xiii 33). The MSS. whose date I have supplied are relatively early by the standards of the Odyssey, of which there are only two minuscules earlier than saec. xiii. The lines are also omitted by Pap. 28 (Pack 1106, saec. iii-iv p.C.). 34

The only other papyrus to furnish evidence on the passage is Pap. Amherst II. 18 (saec. i-ii p.C.), containing scholia minora (mainly simple glosses) on Od. 15, and it is highly probable that these scholia were based on a text which lacked lines 113-19: 35 at the bottom of Column V is a gloss on προτέρω (Od. 15. 109); at the top of Col. VI the editors print [.]ε.[..............]τα[; the
next line reads φαελυν (15. 121). Now it should be noted that
the scholiast is very thorough, in that he has glossed most of the
less common epic words which occur and some of the more strange-
looking epic forms. Thus within 113-19 we should expect to find
notes on most of the following: τιμηστατον, τετυμησων, 37
κερδαναυ, ἕς (ἕς is glossed later, at 132), τεῖν (even σετο is
glossed at 158!), Ὑξασσα (ὑξασσον is glossed later, at 310); and
also some of the following: κελμίλα, πόρεν, ἐ, δομος, ἀμφεκάλυψε,
κεῖστο, νοοτήσαντα. 39 Thus if 0d. 15. 113-19 were in the text used
by the Amherst scholiast we should expect the notes on them to cover
a large portion of Col. VI. 40 But if we suppose (what is by no
means certain) that these scholia constitute a fragment of a
commentary on the whole of the Odyssey, may we not argue that the
reason for the absence of comment on these repeated lines here may be
that they had already been commented on in the scholia on Book 4? Such
an explanation becomes most implausible when we consider that on a
number of occasions this scholiast has taken the trouble to gloss the
same word twice within his notes on Book 15 – not only different
forms of the same verb (κελχάνι, κελχάνω, 157, 260; ἀνακατω, ἀποθεομένη, 268, 357; ναετάμων, ναετάσσε, 255, 385), but also the
same form of a verb (τετυμησε, 77, 94), and the same noun (κελμίλαν
91, κελμίλα 159); moreover, he comments on a number of formulaic
lines which have occurred on several previous occasions in the poem:
there are glosses on προχάφ (135), ζαλλον (142), σκυλωντο (185),
δοδσατο (204). In all probability, then, 113-19 were absent from
his text. The word glossed at the top of Col. VI (second letter ε)
is probably μενουνις (111).

Finally, it is perhaps worth mentioning that Dindorf's scholia
contain no reference to the lines.

The witness against the passage is strong enough to rule out
the explanation of post-Aristarchean excision; and the explanation
of accidental omission is likewise ruled out by the strength of this
witness, by the combination of the length of the passage and the
absence of any homoiographic temptation, and by the fact that it
would be strange that this long omission should coincide precisely
with a passage which has occurred earlier in the poem (whereas, of
course, the interpolation of such passages is very common). Moreover,
the fact that the passage is, to say the least, dispensable (as I
shall argue below) is compatible with the hypothesis of interpolation. Clearly, then, it was absent from the edition of Aristarchus. We have no particular reason to suppose that it was present in any pre-Aristarchean texts, but if it was, those texts would have been few, since Aristarchus would not otherwise have omitted it. It is therefore almost certainly an interpolation. However, we shall need to look closely at the internal evidence, round which most of the debate has revolved, because although some critics interpret this evidence as showing the lines to be intruders in their context, others believe that it demonstrates that they are indispensable to that context.

Bolling regards the passage as an interpolation, Van der Valk regards it as genuine; but the debate has extended beyond the arena of textual criticism (in the narrower sense) into the battleground of the Analysts and Unitarians: the lines have occupied an important place in the argument about the relationship between the Journey of Telemachus and the Return of Odysseus. On this larger question I am a thoroughgoing Unitarian, but a good cause is not strengthened by a bad argument, and in what follows I shall be as much concerned to attack false arguments in support of this position as false arguments against it.

In Book 4 Menelaus says he will give Telemachus a cup (590-92) and a fine silver \( \kappa\rho\tau\iota\rho \) (613-19). In Book 15, after Telemachus has made abundantly clear his desire to return home immediately and without tarrying on the way (64-91), Menelaus goes down to his storeroom with Helen and his son Megapenthes, fetches a cup and tells Megapenthes to bring a silver \( \kappa\rho\tau\iota\rho \), while Helen chooses out a beautiful \( \pi\varepsilon\pi\lambda\varsigma \) (99-108). Then follow the lines printed above.

For P.D.C. Hennings it is absurd (a) that Menelaus, when actually presenting the \( \kappa\rho\tau\iota\rho \), should be made to repeat the words he used when promising it, and (b) that he himself hands over not the \( \kappa\rho\tau\iota\rho \) described in 15. 113-19 but a \( \delta\varepsilon\pi\alpha\varsigma \) (whereas it is Megapenthes who hands over the \( \kappa\rho\tau\iota\rho \)). Hennings's second objection is repeated by U. von Wilamowitz, who derisively translates, "Ich will dir einen schönen mischkessel geben, sagte Menelaos und gab ihm einen becher", and by W. Theiler. Further, Hennings alleges that in 114f. the future \( \delta\omicron\omega\omicron\upsilon \) is inappropriate (whereas Helen in 125 correctly says \( \delta\omicron\omega\omicron\upsilon\nu\)), and this objection is repeated by F. Blass, E. Bethe and Theiler. All five scholars regard the passage as in
some sense an intrusion. For Wilamowitz in 1884 it was an insertion by the Bearbeiter who joined the "Telemachy" to the Odyssey: the repetition, he claimed, is inane, but is a natural consequence of the Bearbeiter's separation of the description of the xρητηρ from its presentation by ten Books; but by 1927 he no longer regarded it as indispensable to the Bearbeiter's text: it is in fact so awkward and pointless, he says, that we can only attribute it to a later rhapsode. U. Hölscher claims that Wilamowitz's translation is misleading and reminds us that the poet immediately goes on to tell us that Megapenthes put the xρητηρ down in front of Telemachus. Hölscher concedes that the passage was originally composed for its place in Book 4 and that it is more appropriate there than in 15: the future δῶσω, he says, better suits the context in 4, where Menelaus merely promises the xρητηρ, and his description of it is more appropriate "wo man ihn noch nicht vor Augen hat": in 15 the description is, strictly speaking, a task for the poet in propria persona, not Menelaus, just as it is the poet himself who describes Helen's gift (105-8); and in 4 Menelaus must say what he proposes to substitute (612 μεταστησω) for the horses which Telemachus has rejected. However, Hölscher argues that it does not follow from this that the lines must be intruders when they recur in 15: on the contrary, they are indispensable to their context there. First, he says, Helen's words at 15. 125, δῶσω τοι καὶ ἔγω ... δῶσωμι, take up the similar words of Menelaus in 113-19, as A. Kirchhoff pointed out. Secondly, "auch verlangt das Gleichgewicht der Reden, dass Menelaos ebenso wie Helena etwas zu seinem Geschenke sage". (Cf. the view of Ameis-Hentze "dass es nicht homerische Sitte sei, ein Geschenk stillschweigend zu überreichen"). Thirdly, ἤ τοι in 111 requires a following δὲ or αὐτῷ, "denn es hebt immer ein erstes Glied gegen ein zweites ab". Consequently the passage cannot be expelled from either 4 or 15. Now (Hölscher's argument continues) the hypothesis of an originally independent and continuous Telemachy which was subsequently split open by a Bearbeiter posits an original continuity between 4. 619 or 620 and a point in Book 15 not long before line 112, but certainly not after it; but this would mean a repetition of the passage 4. 613-19 impossibly soon after its first occurrence; therefore the hypothesis is untenable. G. Bona accepts this argument as a valid refutation of the classical Analytical position. Theiler accepts Hölscher's contention that the occurrence of ἤ τοι in 15. 111 means that
Menelaus's speech cannot end at 112, but unlike Hölscher believes in an originally independent Telemachy in which 4. 619 was immediately followed by 15. 75ff. and yet regards 15. 113-19 as impossibly inept. His solution to the dilemma is that 113-19 stem from the Bearbeiter—the "Vater-Sohn-Dichter"—and have supplanted a single line in the original Telemachy such as δώρα δέ σοι δεδομέν κερδήσα τιμηστα which would eliminate the difficulties of our present text.

A. Heubeck, from a Unitarian standpoint similar to that of Hölscher, defends the authenticity of 113-19: he acknowledges that the repetition creates a certain awkwardness, but tries to account for it by exegesis: "Auf den Mischkrug kam es in erster Linie an—das δέξας ist nur eine Beigabe!—; Nestor [sic!] rühmt ihn noch einmal mit seinen früheren Worten, aber soll er das schwere Stück während seiner Rede in der Hand halten?" For "Nestor" read, of course, "Menelaos"—though some may feel that the argument would gain in weight if the donor were really the aged Nestor! With Heubeck's arguments compare the reply of Ameis-Hentze to Hennings: Menelaus hands over only the cup "weil Telemachos nur diesen in die Hand nehmen kann", whereas the mixing-bowl, on account of its weight, must be put down in front of him; and "der Pocal [bildet] zum Mischkrug eine selbstverständliche Zugabe". The Unitarian E. Seitz makes the obvious point that the gifts of Helen and Menelaus are symbols of the esteem in which they hold Telemachus, but he uses 15. 113-19 in support of this point without showing any awareness of a question about the lines' authenticity. M. Dyson, in defending a Unitarian thesis, argues that it is characteristic of the Odyssey-poet to relate an earlier scene to a later one in the same place after a long interval of time in such a way that "certain problems of chronology are disguised by making each of the scenes almost identical when picked up again as it was when left". Dyson applies this to the two Sparta-scenes in 4 and 15: "Menelaus [in 15 again] offers gifts ..., even using seven lines unchanged that occur in the earlier passage, xv 113-19 = iv 613-19". Dyson, like Seitz, shows no awareness of any problem concerning the authenticity of the repeated lines in 15. Two other contemporary Unitarians, H. Eisenberger and F. Eichhorn, defend the lines: Eisenberger comments that just as the κρήτηρ reminds Menelaus of Phaidimos (15. 117-19), so the gifts of Menelaus and Helen must remind Telemachus of them, and that the repetition is quite acceptable after the great length of the intervening material;
and Eichhorn, while acknowledging the priority of the lines in Book 4, finds the absence of any mention of the cup when they are repeated in Book 15 scarcely more troublesome than the future δῶσω in 15. 114, regarding both as typical of the "Unebenheiten" which tend to result when Homer transfers lines from one context to another. Edwards says of 15. 113-19, "The repetition of the lines here ... does not in my view warrant their rejection"; he goes on to acknowledge that "there is certainly awkwardness in having Menelaus describe the bowl to Telemachus in glowing terms and then hand him the cup," but lists this awkwardness among others which he evidently regards as explicable without recourse to theories of spuriousness — though he does not in fact offer any explanation of the awkward ἀντλήρ-δίκας sequence.

All this criticism (except that of Blass) suffers from a common lack: a failure to give a proper assessment of the external evidence against the lines. Bolling's EE appeared in 1925, and an earlier comprehensive treatment by him of the post-Aristarchean interpolations in both the Iliad and Odyssey in 1916; yet in discussing Od. 15. 113-19 — which Bolling included in his treatment in both 1916 and 1925 — neither Wilamowitz (1927), Hölscher (1939), Theiler (1950), Seitz (1950), Heubeck (1954), Bona (1966), Dyson (1970), Eisenberger (1973), Eichhorn (1973) nor Edwards (1975) shows any awareness of the importance of Bolling's work. Hölscher, Seitz, Bona, Dyson and Edwards, while accepting the lines as genuine, do not even mention the manuscript omissions, let alone consider their implications; Eichhorn lacks any clear reference to the diplomatic evidence, Heubeck's only reference to it is "Vgl. z. B. Van der Valk 206", where Van der Valk's treatment happens to be particularly unsatisfactory, and Eisenberger's only reference to it is even more oblique — "Cf. Heubeck, 63 A. 95." For Wilamowitz and Theiler the lines are intruders, but neither even mentions the possibility that they made their début as late as the post-Aristarchean era, nor does Wilamowitz, either in 1884 or in 1927, even mention the external evidence against them. I have already discussed this external evidence, and shall eventually return to it; but let us first consider what can legitimately be deduced from the internal evidence.

I shall take it for granted here that the fact that a repeated passage (even one which is not itself a formula) in Homer is somewhat less appropriate in Place B than in Place A is in itself insufficient
to condemn the passage as spurious in Place B. Thus in 15. 113-19 -
to take a minor inconsistency which, as far as I know, has not been
pointed out before - Menelaus is made to say δῶραν ὅ', ἵτσο' ἐν τῇ
οἰκῳ κελεύθαι κεκταί, δῶρων ὁ κ. τ. λ. At 4. 613ff. the κρήτηρ is in
fact one of the valuables which "lie stored away": at 15. 113ff. it
is not: it has been brought up from the storeroom and is at present
in Megapenthes's hands. However, the inconsistency is obviously a
very trivial one which cannot be pressed: the rest of Menelaus's
treasure still lies stored away, and the κρήτηρ itself did too until
a few moments ago. As for the objection raised against the future
δῶρα (15. 614ff.), the assumption of Hennings, Blass, Bethe and Theiler
that it is inappropriate here is unjustified, and even Hölscher,
Heubeck and Eichhorn, who defend the lines in 15, concede too much.
Heubeck speaks of "das futurische δῶρα o 114, das strengh logisch nur
für δ 614 passt" - but in strict logic the presentation, in the
sense of the actual handing over of the gifts, is still in the future
during Menelaus's speech in 15, and this use of the future can be
closely paralleled: cf. Od. 3. 50f. "τοῦνεκα σοὶ προτέρῳ δῶρῳ
χρήσελων ἀλεύρων." | δὲ εἰκὼν ἐν χειρὶ τῇ ἐπὶ δέπας ἡδὸς οὖν,
II. 23. 609-13 "ἴππον δῶρῳ ... ἤ ἡ ὅρα, καὶ ... ἡ κίνην ... ᾰπον ...,
ibid. 591-7, 796f. Moreover, I think Hölscher makes another
unwarranted concession: we can agree with him that the description
of the κρήτηρ "steht besser da, wo man ihn noch nicht vor Augen hat",
but it can be argued that he goes too far in continuing, "Im 15.
Buch würde die Beschreibung eigentlich dem Erzählenden selber
zufallen": we may compare Od. 8. 401-5, where Euryalus, addressing
Alcinous in the presence of Odysseus, describes the sword he is about
to present to Odysseus - τὸδ' ἄρο, "this sword", very much "vor
Augen": yet it is Euryalus, not the poet in propria persona, who
points out that it has a silver hilt and an ivory sheath immediately
before presenting it to Odysseus (406 ὅς εἰκὼν ἐν χειρὶ τῇ ἐπὶ).
Finally, there is some awkwardness about the fact that Menelaus is
made to repeat in 15 information already imparted to Telemachus in 4,
but this point cannot be pressed, given that the two Books are
separated by so much intervening material (so Eisenberger) and (as in
my opinion they are) by the passage of an extended period of time in
Sparta.

The real incongruity produced by the repetition consists in the
fact that in 15 these lines, devoted entirely to the κρήτηρ, and
ending τείν δ' ἐθέλω τῦδ' ὑπάσσαν, are immediately followed by the presentation not of the κρητηρ but of the δέκας, which Menelaus has not even mentioned. The juxtaposition is undoubtedly very awkward. The defence of Ameis-Hentze and Heubeck based on the weight of the κρητηρ is legitimate as far as it goes: certainly an objection like that of Theiler,74 "Menelaos bringt nicht καλλυστον κελμήλον, den Mischkrug von δ 615; den übergibt vielmehr der Sohn Megapenthes", lays itself open to the obvious reply that Megapenthes is simply acting as a porter for Menelaus: note 15. 103 ὑπὸν δὲ κρητηρα φέρειν Μεγαπένθες ἁνωγεν. The purpose of Ameis-Hentze and Heubeck in advancing the view that the δέκας is a "Zugabe" or "Beigabe"75 would seem to be to counter the view expressed most clearly and forcefully by Blass:76 "Hier in o bringt Menelaos ein δέκας selbst, und lässt seinen Sohn Megapenthes gleichzeitig den κρητηρ bringen; bei der Ueberreichung nun hat er entweder über beides etwas zu sagen, was nicht geschieht, oder über keines von beiden; so nach P und H [ = Allen's Pal. and H3], und richtig": Heubeck in effect replies that the δέκας is of such minor importance that we need not expect Menelaus to mention it in his speech. Who is right, Blass or Heubeck?

It is true that Menelaus himself describes the κρητηρ as his most beautiful and most valuable item of treasure (4. 614 = 15. 114), but if the κρητηρ is καλλυστον, the cup is still καλὸν (4. 591), and Menelaus wants it to have a special and enduring sentimental value for Telemachus (4. 591f. ἦν σπένδησα θεός | ἀδανάτος ἐμέθεν μεμνημένος ἡματα πάντα).77 Thus if Menelaus is to refer specifically to either of the two items in his speech, we should naturally expect him to refer to both; but I do not think that we can (with Blass) demand that he do so unless we can justify this demand on grounds which go beyond those offered by Blass. However, neither the "Schwere" argument nor the "Beigabe" argument nor Hölscher's indignant protest at Wilamowitz's "irreführende Übersetzunge" succeeds in meeting what I would regard as the crucial objection to the κρητηρ-δέκας sequence, an objection which I think is implicit in Wilamowitz's translation: if 15. 113-19 are integral to their context, why was the handing over of the δέκας made to intervene between Menelaus's account of the κρητηρ (with his insistence on his intention of presenting it: δῶσω ..., δῶσω ..., τείν δ' ἐθέλω τῦδ' ὑπάσσαν) and the actual presentation of the κρητηρ? Even if we regard the actions of
Menelaus and Megapenthes in 15. 120-22 as simultaneous, the awkward sequence in the narration remains. After 119 the poet could easily have continued e.g. ὡς ἔφατ' Ἀτρείδης· ὁ δ' ἄρα κρητηρα φαελών | θηκ' αὐτῶ τροπάρωσε φέρων κρατερὸς Μεγαπένθης, | αὐτὸς δ' αὖτ' ἐν χελρὶ τῇθεν δέπας ἀμφικάπελλον | Ἀτρείδης Μενέλαος (or ἢμείς Ἀτρείδης). Given the seven-line focus on the κρητηρ (113-19) and the role of Megapenthes as Menelaus's son and porter, the alternation of subject in this version (Menelaus - Megapenthes - Menelaus) would be far less awkward than the alternation of object in the other (119 τὸς [= κρητηρα], 120 δέπας, 121 κρητηρα). To this someone may reply that the poet may have followed the 119-22 sequence because he was influenced by the formulaic system ὡς ἔπτοσ' ἐν ἕπτον | ἐπὶ τῷ ἕπτον | ἐπὶ τῷ ἕπτον (Od. 3. 51, 8. 406, 15. 130, 11. 1. 446, 23. 623, 797) and because he liked the symbolism of mentioning first a gift actually handed over to Telemachus by Menelaus, the host, the speech-maker and the actual donor of both gifts. Very well then: we return to Blass: in this case — that is, if the poet is to insist on mentioning the presentation of the δέπας first — he could easily have avoided the awkwardness by adding to Menelaus's speech a line or two on the δέπας (perhaps modelled on 4. 591f.); these lines would come best at the end of the speech, so that the presentation of the δέπας would immediately follow Menelaus's mention of it, and a characteristically Homeric chiasmic or "ring-composition" sequence would result (κρητηρ - δέπας - δέπας - κρητηρ). The fact that the longer text of our MSS. offers neither of these obvious alternatives cannot be satisfactorily accounted for except on the supposition that the weakly-attested passage 113-19 is an interpolation. Eichhorn's appeal to the "Unebenheiten" typically resulting from Homeric repetitions could explain minor awkwardnesses within 15. 113-19 but not the absence of something which we would expect to find after the lines.

Hölscher's argument that Helen's speech (15. 125-9) presupposes 113-19 is invalid: there is no reason why the καὶ in Helen's δῶρον τοι καὶ ἐγὼ ... δῶρῳ should not refer back simply to Menelaus's act of giving, and if we are to talk about das Gleichgewicht der Reden we can legitimately argue that a different and more significant kind of balance or symmetry is present without 113-19: first, the simple sequence δέπας - κρητηρ - πέπλος of the storeroom-scene (102-8).
is retained in the presentation-scene (110-30); and secondly, in
the storeroom-scene the ὁσας and κρητήρ each rate only a brief
mention, whereas Helen's πέπλος is given four lines of elaborate
description; so, in a text without 113-19, the ὁσας and κρητήρ
are again handed over with the minimum of ceremony, whereas the πέπλος
is again given special treatment (123-30), and its presentation
comes as a clear climax to the presentation-scene just as its
description did to the storeroom-scene; but with 113-19 Menelaus
steals Helen's thunder. Similarly, Eisenberger's attempt to show
that Phaidimos has a special relevance to the context in Book 15
does nothing to redeem the vices of the longer text, since the
alleged parallel is one which we can quite happily do without.

As for the generalization of Ameis-Hentze "dass es nicht
homerische Sitte sei, ein Geschenk stillschweigend zu übereichen",
even without Od. 15. 113-19 Menelaus of course does not hand over the
gifts "in silence"; Ameis-Hentze presumably mean, then, that Homeric
custom requires the donor to say something about the gift itself when
handing it over. However, even with this interpretation the
assertion is open to attack, since the Homeric corpus does not
contain enough detailed instances of gift-giving to entitle us to
pontificate in this manner, and anyway those which it does contain
do not all correspond with the Ameis-Hentze principle. Thus
Agamemnon says nothing about his numerous and valuable gifts of
recompense when actually handing them over to Achilles (Il. 19.
241-81). It might be objected that he did speak about them to
Achilles at an earlier point in the narrative, when announcing his
intention of giving them (19. 140-44), but the reply to this is that
Menelaus has likewise spoken to Telemachus earlier about the gifts
which he intends to give him, not only in Book 4, but also, more
recently, at 15. 75f. It is perhaps also worth pointing out that
the actual speech of presentation which Euryalus makes to Odysseus
when presenting him with a sword (Od. 8. 408-11) includes no mention
of the gift but does contain a wish for a happy home-coming very like
that expressed by Menelaus to Telemachus at 15. 111f., though
admittedly the value of this parallel is reduced by the fact that
Euryalus has immediately before described the sword in a speech
addressed to Alcinous, but meant to be overheard by Odysseus.

Finally, we must consider Hölscher's claim that ἦ τοῦ must
designate the first member of an antithesis. This claim must be
given very careful consideration, for if it is allowed to stand, the internal evidence will for once actually prove that matter was wrongly omitted from the text by Aristarchus. Now it is not difficult to demonstrate that the claim cannot stand in the form in which Hölscher has put it; but we shall need to go on to consider whether the use of ή τολ at 15. 111 nevertheless remains anomalous without 113-19.

It is true that ή τολ (also written ήτολ) usually does mark the first member of an antithesis, but there are certainly occasions when the clause in which it occurs is not followed by any antithetical clause. In one set of examples the ή τολ clause comes in the middle or at the end of a speech and stands in relation not to what follows but to what the speaker has just said: there is a strong contrast with what has gone before at 11. 7. 458 (where ή τολ means something like "certainly", 11. 20. 435 ("in fact"), Od. 3. 195 ("certainly"), Od. 21. 98 ("[but] actually"); at 11. 20. 313 ή τολ introduces a γάρ clause which also contains a contrast with the previous clause. At Od. 3. 236 ("of course") and 11. 17. 514 (perhaps "actually", perhaps "of course") the speaker adds a reflection or afterthought, beginning with ἀλλά', on what he has just said - "contrast" or "antithesis" would be rather too strong a term for the relationship. In another set of examples in which ή τολ does not mark the first member of an antithesis, the phrase comes at the beginning of a speech immediately after the opening vocative in a clause contradicting or qualifying something the previous speaker has said. A clear example is Od. 7. 299-301, where Alcinous says ξετύν', ή τολ μὲν τοῦτο γ' ἐναύωμον οὖν ἐνδήσε | ταύς ἐμή .... Hölscher speaks of "Ausznahmen, deren besondere Pragnanz den Sinn der Partikel nur um so deutlicher zeigt", and cites one example, this same passage Od. 7. 299-301. But this is an artificial interpretation: the opposition is not to a second element of an antithesis which is left to the understanding, but to what Odysseus has actually said towards the end of his speech, Ἦ δ' οὖ τι νοθματος ἡμβροτεν ἐσόλωο κ.τ.λ. (292-7: note how Alcinous's πρώτην ἱκέτευσας, 301, takes up Odysseus's τὴν ἱκέτευο', 292). A similar opposition occurs at Od. 18. 251: Eurymachus praises Penelope's beauty (245-9), and Penelope replies, "Actually (ἡ τολ) the gods ruined my beauty when Odysseus sailed for Troy."

The function of ή τολ in Od. 19. 124 is presumably similar -
19. 123–9 = 18. 250–56 (except for a necessary variation in the first line) – though the opposition is clearer in Book 18 (Eurymachus in 18 praises Penelope's beauty, Odysseus in 19 only her fame). Similarly, in Od. 19. 555–8 Odysseus tells Penelope, "The meaning of your dream is clear: the suitors are all on the brink of death", and Penelope replies, "Actually (ἡ τοῦ) dreams are ἄμμαχανος, ἄκρυτάμυθος, and I do not think this one will come true" (560–69).

Now in all the examples which we have so far considered, the relationship of the ἡ τοῦ clause to what precedes is one of contradiction, qualification, contrast or quasi-contrast. These examples are sufficient to refute Hölscuer's assertion that ἡ τοῦ always introduces the first member of an antithesis, but it could be objected that they do not constitute exact parallels to the use of ἡ τοῦ in Od. 15. 111f. (without 113–19), where (at any rate on the face of it) there is no such relationship to what has gone before. Of course it need not follow that this difference is vital, and indeed there is no obvious reason why it should be. Some may claim, on the basis of the evidence so far presented, that a connotation of "nevertheless" or something similar, being common to the examples we have cited, is essential to all cases where ἡ τοῦ does not begin an antithesis; but if ἡ τοῦ can be used in both members of an antithesis, and can also start a new thought by a new speaker which contradicts or qualifies what the previous speaker has said without itself beginning an antithesis, we have already established considerable freedom for it, and it already becomes difficult to claim that the essence of the expression, the nucleus of meaning without which it cannot be applied, is anything more than the infusion of emphasis into an utterance. However, our case will become even stronger if we can find places where ἡ τοῦ neither introduces the first member of an antithesis nor occurs in a clause which stands in an antithetical or quasi-antithetical relationship to what precedes.

One such example is Od. 3. 419; another is Od. 6. 86. At 11. 3. 305–9 (ἡ τοῦ ἔγὼν ἔμυς κ.τ.λ.) it seems most probable that lines 308f. (Ζέυς μὲν τοῦ κ.τ.λ.) give the reason (in paratactic form) why Priam refuses to watch his son fighting Menelaus: "One of the two is predestined to die [and it may well be Paris]". The variant Ζέυς γὰρ found in Eustathius at least gives a plausible exegesis. On this interpretation an antithesis between ἡ τοῦ ἔγὼν and Ζέυς μὲν seems unlikely: there is no obvious contrast
between the two clauses, and in any case an attempt to see an antithesis here is not aided by the use of μεν in 308 rather than δέ or one of the other particles which normally introduce the second half of an antithesis. What, then, is the force of Ἡ τοῦ here? Perhaps "Please note that ...", emphasizing the decision itself, and reinforcing the opening κελευθέ μεν (304); perhaps it merely adds emphasis to the pronoun ἐγὼ. In the latter event it would be artificial to posit a whole contrasting clause left to the understanding - "I am going back [though you will stay here]": we may legitimately say that by drawing attention to himself the speaker implicitly marks himself off from the rest of those present, but emphasis often involves the isolation of a single element from its surroundings, and I doubt whether we can legitimately speak even of a latent antithesis unless there is rather more emphasis on the surroundings than there is here. At any rate, it is far from self-evident that it is only this possible element of implicit antithesis which makes the use of Ἡ τοῦ legitimate here. Places where Ἡ τοῦ may well be used without antithetical overtones include Π. 7. 191, Ο. 15. 111.

My conclusions on the meaning of Ἡ τοῦ are these. Its essential function is to lend emphasis to an utterance (ὁ) in a way which implies a strong desire to "get across" to the listener (τοῦ). Its sense is usually weakened somewhat when it is used concessively, or with pronounced concessive overtones, in the first of two contrasting elements (e.g. "it is true that ..., but ..."), and always weakened still further when it merely marks the first member of an antithesis; but elsewhere its force is something like "You must realize that ...", "Let me emphatically tell you that ...". Some kind of opposition to what follows is a frequent accident of the tone of insistence in the phrase rather than part of its essential meaning. Of course in a high proportion of its occurrences Ἡ τοῦ merely marks the first member of an antithesis, but this ratio, I suspect, represents a relatively late stage of its development: we may compare the fact that the emphasizing use of μεν without any following antithetical clause is common in Homer but practically extinct in Attic. As for the use of Ἡ τοῦ at Ο. 15. 111 (without 113-19), it is not difficult to see how the tone of emphasis and urgent communication inherent in the expression strengthens Menelaus's
wish: if the whole sentence is emphasized, the speaker points out to the listener that his desire for the wish to become reality is strong: "May Zeus really bring this about for you", or, as E.V. Rieu (Penguin, 1946) well translates, "It is my earnest hope, Telemachus, that Zeus ... will ..."); if it is only the first line which is emphasized, then the meaning will be "A return, I say, such as your heart desires - may this by what Zeus brings about for you".

Thus the internal evidence does not demonstrate that the lines are indispensable to their context: on the contrary, if we include them an extremely awkward sequence arises in the immediate context (111-23) and the balance of the wider context (99-130) is arguably disturbed. The internal evidence, then, strongly supports the external evidence against the lines.

Why was the interpolation made? It is just possible that the initial entry of the lines into the text was accidental - that they were written out in full in the ample margin of a very early minuscule MS. by way of commentary on the κρητῆρα ... ἄγγερεων of Od. 15. 103f. and 121-3 and shortly afterwards incorporated into the text by a copyist who mistook this annotation for a correction. However, this thesis does not seem very attractive, since although it would probably allow just enough time to account for the spread of the lines by contaminatio to the vast majority of the extant minuscules, it does not seem particularly likely that the seven lines would have been written out in the margin in the first place. I think the interpolation is more likely to have been deliberate. Perhaps the lines were inserted to make it clear that the silver κρητῆρ of Book 15 was the same as the one described in Book 4; but there may well have been another motive either instead of or in addition to this - namely, the opinion that an ἱ τοῦ clause needed an antithetical sequel. Hölscher and Theiler have had many venerable predecessors: the main grammatical, lexicographical and exegetical tradition, from Alexandrian antiquity to Eustathius, on the meaning of ἱ τοῦ in Homer was that it had exactly the same force as μὲν. So Apollonius Sophistes in his Homeric Lexicon (end of first century p.C.), Apollonius Dyscolus in his Περὶ Εὐντίξεως (first half of second century p.C.)91 and the Homeric scholia;92 and the commentary of Heliodorus on Dionysius Thrax,93 the Etymologicum
Magnum and Eustathius\textsuperscript{94} all tell us that the only difference between \(\tilde{\eta}\ \tau\omega\) and \(\mu\varepsilon\nu\) is that \(\tilde{\eta}\ \tau\omega\) comes at the beginning of a clause and is poetic whereas \(\mu\varepsilon\nu\) comes later in the clause and belongs to the sphere of ordinary language. The \(\tilde{\eta}\ \tau\omega\ = \mu\varepsilon\nu\) equation is also found in other commentaries on Dionysius Thrax.\textsuperscript{95} Hesychius alone, as far as I know, gives a different explanation.\textsuperscript{96} Now it was widely recognized that \(\mu\varepsilon\nu\) in Homer could be used in an emphatic sense without an answering antithetical clause,\textsuperscript{97} and it is not unlikely that whoever first formulated the \(\tilde{\eta}\ \tau\omega\ = \mu\varepsilon\nu\) equation made allowance for exceptions where \(\tilde{\eta}\ \tau\omega\), like Homeric \(\mu\varepsilon\nu\), did not introduce an antithesis; but in all probability the grammarians (or at any rate most of them) in post-Aristarchean times who glossed \(\tilde{\eta}\ \tau\omega\) with \(\mu\varepsilon\nu\) meant to do no more than equate it with the ordinary "preparatory" \(\mu\varepsilon\nu\) of later Greek, and this is surely how virtually all their readers would have understood such glosses.\textsuperscript{98} This is quite clear in the case of Apollonius Dyscolus, who tells us that in the same clause \(\tilde{\eta}\ \tau\omega\ \delta\ \gamma'\ \omega\varsigma\ \epsilon\delta\dot{\iota}\dot{\iota}\nu\nu\ \kappa\alpha\tau'\ \delta\alpha'\ \varepsilon\zeta\varepsilon\tau\omicron\) (e.g. \textit{Od.} 1. 68) \(\tilde{\eta}\ \tau\omega\) has the same force as \(\mu\varepsilon\nu\), on account of which (\(\delta\omicron'\ \delta\)) it is necessarily (\(\delta\alpha\gamma\kappa\lambda\alpha\dot{\iota}\varsigma\)) followed by \(\delta\epsilon\) in the next clause; and it is worth pointing out that the only example cited in the surviving text of Apollonius Sophistes is the same Homeric formula, which is followed by \(\delta\epsilon\) on all its occurrences. Moreover, the dictum stating that the word \(\mu\varepsilon\nu\), while being the semantic equivalent of the "poetic" \(\tilde{\eta}\ \tau\omega\), differs from it in belonging to the sphere of ordinary language (\(\sigma\nu\nu\\theta\nu\varsigma\) Heliodorus on Dionysius Thrax, \textit{Etymologicum Magnum}, \(\tau\nu\varsigma\ \kappa\omega\nu\nu\\varsigma\ \phi\rho\delta\sigma\epsilon\omicron\varsigma\) Eustathius) presumably excludes from its \(\mu\varepsilon\nu\)-concept the use of emphatic, unanswered \(\mu\varepsilon\nu\) in Homer, which was thought to require special explanation.\textsuperscript{99} Thus during the period within which \textit{Od.} 15. 113-19 is most likely to have been first interpolated it would have been widely held that the function of \(\tilde{\eta}\ \tau\omega\) in Homer was to introduce the first member of an antithesis; and the interpolation may well have been an attempt to fill a supposed lacuna.

To summarize and conclude:- The passage \textit{Od.} 15. 113-19 is omitted by a substantial minority of our minuscule MSS. Further, we have the unusual good fortune of possessing \textit{two} papyri covering this part of Book 15, and both testify against the passage in question. Moreover, this very strong external evidence is strongly supported by the internal evidence. It is thus virtually certain that the passage is an interpolation. What follows? The typical Analytical
view of the lines as an illustration of the awkwardness of the junction of the Journey of Telemachus with the Return of Odysseus can no longer stand; nor can Hölscher’s argument that the repetition demonstrates that there was never an independent Telemachy; nor can Dyson’s assumption that the repetition is part of a deliberate and sophisticated attempt by the Odyssey-poet to assimilate the Sparta-scene of Book 15 to the Sparta-scene of Book 4. The literary criticism of Seitz and Eisenberger also suffers. Heubeck argues that the Odyssey-poet has a freer attitude towards repetition than the poet of the Iliad and cites Od. 4. 613-19 = 15. 113.19 as an instance of this— but this example can no longer legitimately be used in support of such a thesis. Finally, the Unitarian reflex action of gallantly springing to the defence of the great poet to ward off the callous attack of some insensitive Analytical detractor has turned out to be sadly misplaced here.

Pre-Aristarchean omissions

My main focus, here as elsewhere in this book, must remain on the lines weakly attested by the post-Aristarchean tradition. But it will not come amiss to briefly make the point that Homerists in general also need to keep their eyes open for pre-Aristarchean external evidence against lines read by Aristarchus, since a substantial weakness in the pre-Aristarchean attestation of such a line will normally constitute grounds for suspecting its authenticity and, where a stronger formulation than ἐν τούς οὐκ ἦν is used, for suspecting it strongly. To illustrate this general point I shall give just two examples, both from Book 1 of the Odyssey.

A scholium (MT) on Od. 1. 97-101 tells us: προσθετοῦντο κατ' ἔνα τῶν ἀντιγράφων οἱ στίχοι, κατὰ δὲ τὴν Μασσαλιωτικὴν οὐδ' ἦσαν. What are we to make of κατ' ἔνα τῶν ἀντιγράφων? The term προσθετεῖν denotes athetesis by one or more of Aristarchus’s Alexandrian predecessors: are we really to understand that an athetesis was marked in only some of the copies of the editions of Zenodotus and/or Aristophanes? This hardly seems likely, especially in view of the unqualified use of ἀντιγράφων, and in all probability one or other of the two explanations suggested by Dindorf in an excellent note in his edition of the scholia ad loc. is right: either the phrase was interpolated by some ignorant scholiast "nescientis προσθετεῖν non
esse τῶν ἀντιγράφων, sed certarum quarundam ἐκδόσεων" who sought to
gloss the enigmatically brief προσθετοῦντο; or else we are faced
with a false transmutation of a scholium which originally read
something like προσθετοῦντο οἱ στίχοι· καὶ ἔννα δὲ τῶν ἀντιγράφων
οὐδ' ἔσαν combined with another scholium which referred specifically
to the absence of the lines from the Massaliōtīkē: Dindorf well
compares Schol. HΜQ on Od. 1. 185: προσθετοῦντο δὲ ὑπὸ
'Ἀρσιστοφάνους· καὶ ἔννα δὲ τῶν ἀντιγράφων οὐδ' ἐφέροντο. If this
second explanation is the right one, then of course the external
evidence against the lines is increased. I do not hold any special
brief for the hypothesis that they are an interpolation, but it is
worth noticing that the following circumstances are compatible with
this hypothesis: the lines lack originality (each occurs at least
twice elsewhere in Homer), the initial spur to their intrusion could
have been the temptation to concordance interpolation (cf. Od. 5.
44-7, II. 24. 340-43), and transcriptional error is virtually ruled
out as the source of the omission by the absence of any homoiographic
temptation. This is not the place for a full discussion of the
question: I merely point out that three recent defences of the lines
by Unitarian critics make no attempt to come to terms with the
ancient manuscript evidence: though all three critics refer or
allude to the Alexandrians' objections against the passage, they say
nothing about the omission by the Massaliōtīkē, and perhaps other
pre-Aristarchean MSS., attested by the scholia: Heubeck:
"Zweifellos sind die vv. α 97-101 gegen die Alexandriner zu halten"; 102
Marion Müller: "Die Zusammensetzung aus formelhaften Iliasversen
liessen [she means: liess] α 96-102 [sic 103] schon von alters her
verdächtig erscheinen"; 104 K. Rüter: "Hier ... hat die Kritik
einzugreifen versucht", followed by a reference to the Alexandrians'
athetesis and to the internal evidence adduced by the scholia in its
support. 105 (The emphasis in each quotation is mine.) However, to
say the least, it simply cannot be taken for granted that excision
motivated by typically Alexandrian arguments accounts for the omission
by the Massaliōtīkē, a MS. hailing from the far west of the Greek
world which may well have been written at the start of the third
century B.C. or earlier, and perhaps other pre-Aristarchean MSS.

The pre-Aristarchean external evidence against Od. 1. 356-9 is
much stronger. Schol. H tells us: ἐν δὲ ταῖς χαριστέραις γραφαῖς
οὐδ' ἔσαν. Even if we allow for oversimplification (and thus
exaggeration) in this statement, it still amounts to a very strong case against the passage. The internal evidence is in itself inconclusive: the lines are dispensable, but it is certainly not true to claim, as Bolling does (EE 220), that "the shorter text ... is clearly superior". The presence of the lines certainly makes Telemachus's speech more sensational, but then sensationalism was just the sort of thing the rhapsodes went in for. Moreover, it is possible to see the lines as concordance interpolation under the influence of what follows, since Od. 1. 356-64 = 21. 350-58, and the context is very similar (Telemachus addressing Penelope). The internal evidence, then, does nothing to counter the weight of the external evidence. However, the Unitarians Rüter and Eichhorn not only assume that the lines are genuine, without considering the external evidence against them, but argue that they were composed before the parallel passage Od. 21. 350-53.

To summarize and conclude this Chapter:— There is widespread ignorance among Homerists about the true significance of weak attestation by our MSS. Sometimes, indeed, a passage is discussed without even any apparent awareness of the weakness in its attestation. Formula-analysis, literary criticism and discussions about the genesis and early transmission of the poems have often been seriously marred by this ignorance. Pre-Aristarchean external evidence against Aristarchean lines also tends to be ignored or its significance underestimated.
NOTES ON CHAPTER VI


3. See above, pp. 16-18; Bolling also argued here (pp. 219-21) that ll. 8. 493-6 was a pre-Aristarchean interpolation on the sole basis of its omission by Zenodotus.

4. For details see above, pp. 146-51.


6. In one part of his later analysis Edwards seems to ignore Od. 18. 111a (the only non-Wolfian line read by Allen) - he apparently omits it from his count on p. 17 (top), presumably because it is absent from Dunbar's Concordance - but his failure to list Od. 18. 111 as a unique form of introduction on p. 33 would seem to imply an acceptance of 111a.


8. See above, pp. 16f.


12. "About Winged Words", CP 32 (1937) 59-63. Parry argued (1) that Homer has no whole line without ἐπει τερέντα meaning "and he said" (without naming the subject) and that it is solely this metrical and formulaic consideration which determines when Homer's words are winged, and (2) that "another critic, if he knew how to write as well as Professor Calhoun, could paraphrase in the same lively way the speeches introduced by any other group of formulas" (p. 61).

13. In this connection the quotation from Parry in my previous note ad fin. is again relevant.

14. Companion 31ff.; I have corrected the slip ἀναθεῖνα (for ἔκκαθεῖνα) in the line from the Iliad.

15. Above, p. 4.

16. The line has also often been attacked on the basis of the internal evidence: the strongest argument is that stated by Merry ad loc.: "the line seems to give an unsatisfactory exegesis of πόντ "ἐταλτο, which appears to imply more than
this one detail of duty"; so also K.F. Ameis and C. Hentze, Anhang zu Homers Odyssee Heft 2 (3rd ed. Leipzig 1889) ad loc.: "Die Leitung auf das Amt des Thürschliessers zu beschränken, wäre hier unpassend."

17. The line also occurs at II. 5. 751, but the whole passage II. 8. 381–96 has been taken over verbatim from Book 5: see e.g. Leaf ad loc.


19. "Zu Odyssee a 148, 148a", Ziva Antika 20 (1970) 73–7. Briefly, Heubeck's argument is as follows: (1) Nowhere else in Homer is the formula of Od. 1. 148 found without the formula of 148a (except at Od. 7. 182, where a line of similar sense follows); therefore 148 and 148a should be considered and judged as a pair, and accepted or rejected together. Now this formulaic couplet was originally designed for a libation-scene; but nowhere else in Homer does the couplet interrupt a description of a meal, as in Od. 1; and indeed the meal-context of Od. 1 suggests that 148–148a are not meant to be taken in the religious sense they bear elsewhere – but this would involve a misuse of the couplet. (2) The filling of the mixing-bowls described in 148 has already been described, in 110, and such a description cannot legitimately be repeated. Heubeck's arguments were largely anticipated (though he fails to mention this) by K.F. Ameis and C. Hentze, Anhang zu Homers Odyssee Heft 1 (4th ed. Leipzig 1890) ad loc.

20. This passage is also treated as genuine by Heubeck, loc. cit. (above, n. 19) 75.


23. See e.g. Lord's "Homer's Originality: Oral Dictated Texts", TAPA 84 (1953) 124–34.


26. TAPA 105 (1975) 51–61, esp. 60f.


30. Loc. cit. (above, n. 26) 65 n. 25, 66.


32. Above, p. 197.

33. In H3 the lines have been added in the margin, but by a later hand: see Ludwich's edition ad loc.

34. See A.S. Hunt, Catalogue of the Greek Papyri in the John Rylands Library Vol. I (Manchester 1911) p. 170 (on line 127): "Since the preceding page ended with 1. 91 and the pages hereabout do not contain more than thirty verses, it may be inferred that the codex agreed with [Ludwich's] PH [= Allen's Pal. and H3], etc., ... in omitting ll. 113-19. This reduces the present page to the normal number of twenty-nine lines."


36. The editors state (p. 9) that "it is occasionally difficult to decide whether there is anything lost at the top [of a column] or not, since there is no upper margin preserved"; but in their transcription of Col. V they indicate no lacuna above the line beginning [...]ε (contrast their transcription ofCols. II, V, XIII, XIV, XVII, XIX), and in their notes (p. 15) they state without qualification that there is "only one gloss between lines 109 and 121": it seems highly probable, then, that nothing is missing above the entry [...]ε μ.τ.λ. at the top ofCol. VI.

37. It is true that the form τετυκετήν has already been glossed twice recently, at lines 77 and 94, but in both places as παρασκευάζετεν, whereas here both form and meaning are different.

38. It is true that this noun has been glossed shortly before, at 91, but the two glossings of τετυκετήν (see above, n. 37) are even closer to each other.

39. Cf. the simple glosses on τέλν and ἄνεχθαλψε by Schol. P. and H respectively in Dindorf's scholia on Od. 4. 613-19.

40. Even if we do posit the loss of a whole line at the top of the column (cf. n. 36 above), this will make little difference to the argument.

If Aristarchus knew the lines, his objection to them, as far as the internal evidence was concerned, would surely have been that they were ὀφθαλμοῦχενος and not merely superfluous but positively intrusive in their context, whereas they suited their context in Book 4, whence they had been wrongly imported. However, Van der Valk curiously assumes that the objection of the ancient critics - "probably... the Alexandrians" - who excised the lines was based on a misunderstanding of the "archaic mentality" which represented the Homeric heroes as having "no hesitation in expressing themselves on subjects, which in after times are usually not touched upon. Thus... Menelaus... stresses before his guests the costliness of the gifts which he offers Telemachus (ο 113-19, δ 613-9)", and 15. 113-119 were excised because this ostentation was regarded as unbecoming. I do not believe that this extraordinary objection would ever have occurred to Aristarchus, but if it had occurred to him, it would also (as Van der Valk himself implies) have been applicable to Od. 4. 613-19 - which, however, are found in all our MSS., and which Aristarchus certainly read: Didymus reports the wording of his text at 4. 618 (Schol. HP).

Jahrbücher für classische Philologie Suppl. 3 (1858) 199.

Hennings adds, "zumal da das Versprechen zwanzig Verse vorher gegeben ist." K.F. Ameis and C. Hentze, Anhang zu Homers Odyssee Heft 3 (2nd ed. Leipzig 1877) ad loc., have completely misunderstood Hennings's point here: they are puzzled by the figure 20 and assume he must be referring to 15. 75, but if they had read Hennings in context they would have realized that he was actually referring to 4. 613-19, which on his theory was immediately followed by 15. 93ff. in the original Telemachy (see esp. his pp. 194, 198).

Homerische Untersuchungen (Berlin 1884) 91 n. 4.

Museum Helveticum 7 (1950) 117.

Die Interpolationen in der Odyssee (Halle 1904) 162.

Homer: Dichtung und Sage Band II (Leipzig and Berlin 1922) 19.

Loc. cit. (above, n. 46).

Loc. cit. (above, n. 45).

Die Heimkehr des Odysseus (Berlin 1927) 3.

Untersuchungen zur Form der Odyssee (Hermes Einzelschriften Heft 6 [Berlin 1939]) 24f.

Kirchhoff (Die homerische Odyssee [Berlin 1879, repr. Hildesheim 1973] p. 192) strangely regarded the passage as original in 15, a later addition in 4, perhaps by the Bearbeiter, perhaps still later - an opinion which Wilamowitz (above, n. 45) and Blass (above, n. 47) rightly condemn strongly. Kirchhoff's opinion was supported by M. Herdt (Die Irrfahrten des Menelaos, mit Bemerkungen über die Komposition der Telemachie [Programm des Königlichen Maximilians-Gymnasiums für das Schuljahr 1891/92,}
Munich 1892] 37-9), who added the further argument that at 4. 613 "die Partikel ἐ' nach δἀρων kaum zu erklären ist" (p. 38): in fact, however, this is an entirely normal use of continuative ἐ'.

54. Loc. cit. (above, n. 44).

55. Line 612 for Kirchhoff (above, n. 53): but Hölscher has already rejected this theory with his argument that the passage was originally composed for its place in Book 4.

56. Studi sull'Odissea (Turin 1966) 216f.

57. Loc. cit. (above, n. 46).

58. For this equation see Theiler, loc. cit. (above, n. 46) 122.

59. Cf. P. Von der Mühll, RE Suppl. VII (1940) 738, commenting on Od. 15: "So ist 99ff. Überschobenes T [sc. Telemachie], sogar 113-119 (nach ἐ 613ff.) mag ein τη ἐπαγ ... überdecken" (I have corrected the obvious misprint "113-139").

60. Der Odyssee-Dichter und die Ilias (Erlangen 1954) 62f., esp. 63 n. 95.

61. The same slip recurs on Heubeck's p. 68.

62. Loc. cit. (above, n. 44).

63. Die Stellung der "Telemachie" im Aufbau der Odyssee (Diss. Marburg 1950) 125.


65. Studien zur Odyssee (Palingenesia 7 [Wiesbaden 1973]) 93.

66. Die Telemachie (Garmisch-Partenkirchen 1973) 32f.

67. Loc. cit. (above, n. 26) 57.

68. Loc. cit. (above, n. 26) 61.

69. Blass (above, n. 49) does as well as could be expected of a scholar of his time: in rejecting the passage he makes much of the manuscript omissions (p. 162), and he has already explained the weight he attaches to such omissions (p. 13). His discussion of this passage falls into a section headed "Interpolationen der Rhapsoden" (p. 26), but he has made it clear that in this category he includes copyists' interpolations (pp. 5, 24). Unlike Bolling he fails to distinguish between pre- and post-Aristarchean interpolations, but in 1904 he did not have the benefit of Bolling's demonstration of the relationship between our vulgate and the edition of Aristarchus.

71. See above, n. 41.

72. See above, n. 42.

73. Wilamowitz's 'späther Rhapsode' (above, n. 51) is presumably pre-Aristarchean. Theiler (above, n. 46) assumes that the omission of 15. 113-19 in some of our MSS. is due to excision in an 'antiken Rezension' and, as we have seen, places the supplanting of his posited original single line by our 113-19 as early as the Bearbeiter.

74. Loc. cit. (above, n. 46).

75. I should add that the detail of the argument in Ameis-Hentze (above, n. 44) at this point is very weak: they cite a string of alleged parallels, none of them of any real relevance, and the result is a glaring example of preposterous special pleading.

76. Loc. cit. (above, n. 47).

77. The cup, like the κρητήρ, is given in fulfilment of Menelaus's specific promise in Book 4. There Menelaus first offers Telemachus three horses (with a chariot) and a cup (589-92). Telemachus declines the horses as being unsuited to the terrain of Ithaca (600-618), whereupon Menelaus offers a κρητήρ in their place: ταύτα μεταστήσω (612) must refer only to the horses and chariot, not to the cup as well. The cup is called ἀλέσσον at 4. 591, δέπις at 15. 102 and 120, but this does not imply that a different kind of object is given from that promised: there are other places in the Odyssey where δέπις and ἀλέσσον are used indiscriminately of a single article: the δέπις of Od. 3. 41, 46, 51 is called ἀλέσσον at 50, 53; similarly the ἀλέσσον of Od. 22. 9 is called δέπις at 22. 17. In all these places the choice of word is obviously determined by mere metrical convenience. Likewise ξόφος, δόρ and φάσγανον are interchangeable in Homer (see e.g. Od. 8. 403, 406); also ἔγχος and δόρον (see e.g. Od. 10. 145, 162); see Stubbings on pp. 517f. of the Companion.

78. For two occurrences of a person's name in the nominative within a few lines of each other cf. e.g. Τηλέμαχος Od. 2. 416, 418; 22. 92, 95; Ὀδυσσεός Od. 1. 57, 60; 7. 139, 142, 145; 19. 237, 239.

79. Compare the way the poet again makes the performance of Helen eclipse that of Menelaus a little later, when her speech forms the dramatic climax of the departure-scene. Overhead, on the right, an eagle appears carrying a large goose in its talons (15. 160-65); Peisistratus asks Menelaus to interpret the sign (166-8); Menelaus ponders in silence (169f.), but before he has a chance to reply Helen gets in with her own elaborate interpretation (τὸν δ’ Ἑλένην ... ὑποθαμεύειν φατο μοῦνον, 171): Odysseus will return and take vengeance on the suitors, or is already at home plotting against them (171-8).

80. See above, n. 65, and the corresponding section of my text.
81. The parallel has to be laboriously excavated from the text, since although Helen does call her own gift a μυημ' Ἐλένης χειρῶν for Telemachus (15. 126), Menelaus makes no such point about his gifts in Book 15, nor does he explicitly say that the κρήνη serves to remind him of Phaidimos: the mention of King Phaidimos of Sidon as its previous owner in 4. 617-19 (= 15. 117-19) was presumably designed to give it added lustre in Telemachus's eyes: cf. the comment on the passage by M.I. Finley, The World of Odysseus (revised ed. London 1956) 134: "A trophy with such a history obviously shed greater glory on both donor and recipient than just any silver bowl .... Status was the chief determinant of values, and status was transmitted from the person to his possessions, adding still more worth to their intrinsic value as gold or silver .... ."

82. I say "enough detailed examples": at Od. 13. 16f., for example, the narrative is too brief to enable us to say what, if anything, the Phaeacian nobles say to Odysseus when presenting their tripods and cauldrons.

83. Cf. n. 77 above.

84. It seems unlikely that ᾿Η τοῦ also looks forward to a contrast with εἰ κεύοις γ' η.τ.λ. (254f.): rather, εἰ κεύοις γ' seems to mark a new start, introducing an antithesis whose second member is υῦν δ' η.τ.λ. (256).

85. Menelaus makes no direct reply to Telemachus's speech of 86-91 until 111f.; it would thus be possible to argue that ᾿Η τοῦ in 111, introducing a wish for a happy home-coming, marks a contrast with the fears expressed by Telemachus in 88-91 that there will be no home-coming for him or a disappointing one. However, since a fairly long passage (91-110) has intervened between the two speeches I do not regard this as the most natural interpretation.

86. It is hardly possible to see an antithesis between ᾿Η τοῦ τπλυνοὶ Ἀδαν (Od. 6. 86) and τολὼ δ' ὅωρ ... ὑπεκπρόεσσ ... (86f.), since the τπλυνοὶ and the ὅωρ are intimately connected in an associative way, and the δ' must surely be merely continuative.

87. Cf. J.D. Denniston, The Greek Particles (2nd ed. Oxford 1954) 360 (2), where he lists examples of emphatic (unanswered) μεν after pronouns: most of the Homeric examples in his first paragraph include the element of isolating an individual, or group of individuals, from others, yet Denniston is right to place these examples here rather than in his separate category of "elliptically antithetical" μεν (359, 380-84).

88. Cf. Denniston (above, n. 87) 553: "Strictly, τοῦ serves to bring home a truth of which the certainty is expressed by ᾿Η: 'Verily, I tell you'."

89. Cf. H. Ebeling, Lexicon Homericum (Leipzig 1880-85, repr. Hildesheim 1963) Vol. I 529 s.v. η τοῦ: "debilitato ... pondere non raro fit μεν." Denniston (above, n. 88) should not have ignored this usage.
90. I suspect that the doctrine on η τοῦ espoused by Hölscher and Theiler stems from some erroneous statements in the introduction to Ebeling's article (above, n. 89): "Non est nisi in priore membro ... . . . habet quod opponatur ... ."


92. E.g. Schol. HQ on Od. 6. 86 and Schol. B on Od. 8. 87. Note the sweeping generalization in the H scholium on the former passage: σημειωτέρον πανταχοῦ τὸ ητοῦ ἀντὶ τοῦ μεν.

93. Gram. Graeci (above, n. 91) I. iii. 103. 32 Hilgard.

94. 50. 10, on II. 1. 68; cf. 1404. 31, on Od. 1. 155.

95. Gram. Graeci (above, n. 91) I. iii Commentary of Melampus or Diomedes 63. 4, Scholia Vaticana 286. 2 and 36, Scholia Marciana 437. 31.

96. To be more precise, he is aware of the η τοῦ = μὲν equation but puts it on a low footing among other explanations. He has two entries on simple ητοῦ: (a) συνεσθος λοσοναμίων τῷ δέ. τινες δὲ καὶ ἀντὶ τοῦ μὲν χρὴνται. (b) καὶ δὴ οὕτως. Further, ητοῦ δὲ γε is glossed as οὕτος δὲ καὶ ητοῦ ὁ μὲν ας καὶ δὴ οὕτος (v.l. ὁ μὲν δὴ).

97. For references to Apollonius Dyscolus, the Homeric scholia and Eustathius see Ebeling (above, n. 89) Vol. I 1046 col. 2, I ad init.; add Gram. Graeci (above, n. 91) I. iii. 284. 15 and 441. 38 (Scholia Vaticana and Marciana respectively on Dionysius Thrax). We would also expect Aristarchus to have been aware of this usage, but we cannot prove that he was (as Ebeling tries to do) by citing his reading ἡ μὲν instead of ἡ μὴν at II. 9. 57, since he may have intended μὲν to mark the contrast between ἡ μὲν καὶ νῦς ἐσσοί κ.τ.λ. (57f.) and ἀτάρ πεπνυμένα βάζεως κ.τ.λ. (58f.).

98. It is true that "the original, affirmative, sense of μὲν ... maintains some kind of precarious existence throughout classical Greek" (Denniston [above, n. 87] 359), but I doubt whether, at any rate in the period after Aristarchus, the μὲν of ordinary (correct) prose would have been thought of as other than necessarily "preparatory". See Aristotle Rhet. 1407a 22 ὁ μὲν καί ὁ ἐγὼ μὲν ἀπαίτετ τὸν δὲ καὶ τὸν ὁ δὲ and the commentaries on Dionysius Thrax: Gram. Graeci (above, n. 91) I. iii. 61. 27 (Melampus or Diomedes) [ὁ μὲν] ἀπαίτετ ... τὸν δὲ ἐπενεχθήναι ἐν τῷ δευτέρῳ λόγῳ ἡ τοὺς λοσοναμιοῦντας τῷ δὲ; 104. 7 (Heliodorus) χρη δὲ εἰδοναι εἰπτάνως τῷ μὲν ἑπιφέρεται ὁ δὲ (or a similar particle: 104. 10, 13); and 284. 15 (Schol. Vat.). Thus scholiasts and others felt it necessary to comment on occurrences of unanswered μὲν in Homer: e.g. Schol. B on II. 13. 622 ὁ μὲν ἀντὶ τοῦ μὴν... οὐ γὰρ ἔχει τὸν δὲ; the "μὲν = μὴν" explanation recurs elsewhere in the Iliadic scholia, and is also found in Apollonius Dyscolus and in the scholia on Dionysius Thrax; and we are told at various places in the Odyssean scholia that μὲν is used in the sense of δὴ. For
references see above, n. 97.

99. See above, n. 98 ad fin.

100. Op. cit. (above, n. 60) 68f., cf. 63 n. 95.

101. In addition to the examples of this already mentioned there is the particularly touching case of the reply by E. Kammer (Die Einheit der Odyssee [Leipzig 1873] 235f.) to the attack by Hennings (above, n. 43).


103. Here she has presumably been misled by Allen's apparatus.


106. See above,


SUMMARY

Preliminary Note: This Summary, like the Introduction, contains some deliberate oversimplification.

Bolling's demonstration that the numerus versuum of our Homeric vulgate, when shorn of the lines weakly attested by the post-Aristarchean tradition, is identical with that of Aristarchus has been strongly confirmed by recent papyrus publications. Some of these weakly-attested lines are demonstrably survivals from pre-Aristarchean times which have returned to the text, but the vast majority of the remainder probably entered the text for the first time in the post-Aristarchean period. In any case, the pre-Aristarchean survivals among the non-Aristarchean lines in our MSS. are themselves interpolations, since an examination of Aristarchus's modus operandi shows that he omitted only lines which were absent from the vast majority of his MSS., and such lines would have been spurious. In pre-Aristarchean times reciters' forgetfulness, copyists' slips and excision would have led to the omission of some lines from some of Aristarchus's MSS., but not enough MSS. to induce him to omit the lines. The possibility of very occasional exceptions cannot be completely excluded, but the chances that any given non-Aristarchean line in our MSS. is genuine must be virtually nil, a fortiori where the line cannot be shown to have been pre-Aristarchean.

A detailed consideration of various manuscript omissions which create difficulties for the hypothesis that Aristarchus did not omit genuine lines has left that hypothesis essentially unshaken.

There is widespread ignorance among Homerists about the significance of weak attestation by our MSS. This has had unfortunate consequences in certain discussions in the areas of formula-analysis, literary criticism and the Homeric Question. For example, the question of the authenticity of Od. 15. 113-19 is important for the question of the relationship between the Journey of Telemachus and the rest of the Odyssey, but the passage has usually been discussed with no appreciation of the significance, and sometimes with no apparent awareness of the existence, of the strong external evidence against it. If further discussions in these areas are to be soundly based, they must be based on a text which excludes the non-Aristarchean lines.
Introductory Note: The Ph.D. regulations of the Australian National University, in their most recent form, stipulate that the thesis should contain "a bibliography of works to which reference has been made". I have thought it best to interpret this requirement literally. Thus I have not included various works which I consulted but which, for one reason or another, I did not have occasion to refer to in the thesis. One result is that the mere mention of a papyrus in my thesis, by means of the accepted codes (e.g. Pack No. etc.), is not enough to entitle the edition in which it was published to a place in this Bibliography; thus the only papyrus editions which appear here are those which were published too recently to get into Pack's second edition and those whose contents I needed to discuss in some detail (e.g. by citing the editor's comments). On the other hand this Bibliography contains all the works actually cited in the thesis, even those which I have mentioned only in passing and those which do not deal directly with Homer.

Within the various sections of this Bibliography, works are normally arranged in alphabetical order of authors and editors. Where an author has more than one work listed, the order of his works is chronological. Although in this Bibliography my references to the Ameis-Hentze-Cauer Iliad and Odyssey are merely synoptic, precise details have been given wherever these Protean editions have been cited in the body of the thesis. Many books written in the last century or early in this have recently been reprinted; where I happen to have used such a reprint I have recorded the fact, because it constitutes useful information about the book's present availability, but I have not wasted any time in attempting to discover which of the old books which I used in their old printing have been reprinted.

The Bibliography is divided into the following sections:-

Section A: Iliad: (1) Editions; (2) Scholia; (3) Translations;
Section B: Odyssey: (1) Editions; (2) Scholia; (3) Translations;
Section C: Papyri;
Section D: General.
In A(2) and B(2) only actual editions of scholia are listed; discussions etc. are relegated to Section D. Editions of papyrus scholia are listed in Section C, which includes all works devoted exclusively to papyri - editions, discussions and inventories; works dealing with papyri only inter alia are included in Section D. Works of joint authorship (except L-S-J) are cross-referenced in Section D, but in the other, shorter, sections I judged this unnecessary.

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