

MODERN TEXTUAL CRITICISM AND THE MAJORITY TEXT: A RESPONSE

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Permit me to begin with an announcement that should be of general interest to NT scholars. A new critically-revised edition of the Greek NT with critical apparatus is now well advanced in preparation and is projected for publication possibly in 1978, more probably in 1979. The project is under the editorship of Arthur Farstad of Dallas and myself and is being sponsored by Thomas Nelson Publishers.

The new text will be entitled *The New Testament According to the Majority Text* and will seek to present as accurately as possible—within the limits of our present knowledge—that form of Greek text that predominates in the surviving Greek MSS. As all specialists in this area will know, such a text will by no means be identical with the so-called *Textus Receptus* (TR) but will vary from it in a substantial number of places. For example, the famous *Comma Johanneum*, the introduction of which into the TR by Erasmus is a standard piece of lore in text-critical handbooks, will be eliminated from our text. So will Acts 8:37 and many other readings that lack adequate MS attestation. For that reason the new text should not be regarded as an edition of the TR but rather as a new revision of the Greek text based on principles quite diverse from those that underlie currently popular editions such as Nestle-Aland and *UBSGNT*. Two levels of apparatus will accompany the new text that will be designed to exhibit variations within the majority text itself as well as all the significant differences between our text and the third edition of *UBSGNT*, which equals the 26th edition of Nestle-Aland.

Not everyone, however, will greet this announcement with applause. In fact, in an article entitled "Modern Textual Criticism and the Revival of the *Textus Receptus*," published in *JETS* 21/1 (March, 1978) 19-33, Gordon Fee took me to task for my position in this field. I am indebted to Fee's personal courtesy for allowing me to have a prepublication copy of that paper and to study it. I trust that he will feel that my response to him here is marked by the courtesy that is appropriate when two Christians, equally committed to the authority and integrity of God's Word, have a sincere difference of opinion.

Having said that, I nevertheless feel constrained to confess my dismay that Fee has so inadequately represented the position that I actually hold. Perhaps, however, he should not be faulted for this since my own writings are not numerous and he was constrained to fill in the gaps in my thought with the views of others. Hopefully, therefore, this paper

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will serve as a corrective for this deficiency while at the same time it seeks to counter certain arguments that Fee directed against us.

Fee began his article with the assertion that "New Testament scholarship and the working pastor are generally agreed on one point: The task of NT textual criticism is virtually completed. What remains is basically a 'mopping-up' operation on some disputed readings."¹ I was astounded by this claim from Fee because, as any specialist in this area should know, it simply is not true.

As recently as 1974, Eldon Jay Epp discussed the contemporary situation. Among his statements the following is noteworthy:

One response to the fact that our popular critical texts are still so close to that of Westcott-Hort might be that the kind of text arrived at by them and supported so widely by subsequent criticism is in fact and without question the best attainable NT text: *yet every textual critic knows* [italics ours] that this similarity of text indicates, rather, that we have made little progress in textual *theory* since Westcott-Hort; that we simply do not know how to make a definitive determination as to what the best text is; that we do not have a clear picture of the transmission and alteration of the text in the first few centuries; and, accordingly, that the Westcott-Hort kind of text has maintained its dominant position largely by default. Gunther Zuntz enforces the point in a slightly different way when he says that "the agreement between our modern editions does not mean that we have recovered the original text. It is due to the simple fact that their editors . . . follow one narrow section of the evidence, namely, the non-Western Old Uncials." This lack of progress toward a theory and history of the earliest NT text is a strong indication that the 20th century has been an interlude in NT textual criticism.²

This does not sound like the "'mopping-up' operation" that Fee describes.

To support his contention that the work of textual criticism is largely done, Fee produces a quotation from Joachim Jeremias.³ This strikes me as significant since, though Jeremias is a prodigious scholar indeed in the field of Judaism and NT backgrounds, he is not a specialist in the area for which Fee claims his support. Just a few years after the work of Jeremias from which Fee cites, Kenneth W. Clark—a recognized specialist in the area of textual criticism—wrote quite differently:

All the critical editions since 1881 are basically the same as Westcott-Hort. All are founded on the same Egyptian recension and generally reflect the same assumptions of transmission.⁴

¹G. D. Fee, "Modern Textual Criticism and the Revival of the *Textus Receptus*," *JETS* 21/1 (1978) 19.

²E. J. Epp, "The Twentieth Century Interlude in New Testament Textual Criticism," *JBL* 93 (1974) 390-391. To speak, as Fee does, of a "methodological consensus" ("Criticism," p. 19) seriously misrepresents the state of affairs, and to dismiss the methodological alternative of pure eclecticism as "unrest over this consensus" (ibid.) is to gloss over the fact that, after all, no consensus really exists.

³Fee, "Criticism," p. 19, citing J. Jeremias, *The Lord's Prayer* (Philadelphia: Fortress, 1964) 7.

⁴K. W. Clark, "Today's Problems with the Critical Text of the New Testament," *Transitions in Biblical Scholarship* (ed. J. C. Rylaarsdam; Chicago: University Press, 1968) 159.

Shortly after, in the same article, Clark adds:

Psychologically it is now difficult to approach the textual problem with free and independent mind. Where textual emendations are at issue we have literally moved in frequent circle, with alternating favor between two choices. Eclectic experimentation and sporadic emendation constitute the order of the day. Critical alteration in the frequent editions has been slight and amounts only to intermittent patches. The main fabric is still that of Westcott-Hort. A recent expression of Colwell's, though related to a different factor, is truly applicable here also: "Hort has put blinders on our eyes." However great the attainment in the Westcott-Hort text, the further progress we desiderate can be accomplished only when our psychological bonds are broken. Herein lies today's foremost problem with the critical text of the New Testament.⁵

I agree with Clark that there is urgent need to break the psychological bonds that the Westcott-Hort theory imposes upon us, though I admit that the direction I wish to take is one with which Clark might not be pleased. Still, it is clear that reputable contemporary scholars are very far from affirming that "the task of . . . textual criticism is virtually completed," and Fee does not do service to sound scholarship by insisting that it is.

I do not wish to spend much time on the charge that Fee raises early in his article to the effect that we object to Westcott and Hort principally because they were not orthodox. Let me quote Fee's words:

What begins to surface, therefore, is a hidden agenda, which is the common denominator of all modern advocates of the TR—namely, that Westcott and Hort's Greek text is suspect because their orthodoxy with regard to Scripture is suspect, especially in contrast to the "learned men of 1611" and Westcott-Hort's doughty antagonist, J. W. Burgon.⁶

Here, it seems to me, Fee is not maintaining the proper level for scholarly dialogue. To speak of "all modern advocates of the TR" as having a "hidden agenda" is an impermissible *argumentum ad hominem*. It also is not true. I, for one, would be quite happy to accept the Westcott-Hort text as it stands if I thought that the grounds on which it rested were adequate. But the adequacy of their text has been questioned by many scholars over a long period of years, and not only by those who prefer a text like that found in the TR. My agenda at least—and I speak here only for myself—is precisely what I have expressed it to be—namely, a call to re-examine the claims of the majority text in the light of the increasingly perceived deficiencies of the theory that underlies today's editions. I happen to think that a man's theology *can* affect his textual theories, but I am perfectly willing to entertain sensible arguments from any quarter no matter what theology they may be as-

⁵Ibid., p. 160.

⁶Fee, "Criticism," p. 21.

sociated with.⁷ I prefer, therefore, to pass by the remainder of Fee's observations about my presumed theological position and address myself directly to the technical arguments he raises.⁸

Fee quotes a statement of mine which reads as follows:

The manuscript tradition of an ancient book will, under any but the most exceptional conditions, multiply in a reasonably regular fashion with the result that the copies nearest the autograph will normally have the largest number of descendants.⁹

It should be noted—though Fee does not call attention to it—that in making this statement I can, for once, claim a real measure of support from Westcott and Hort. In a footnote to the article from which Fee quotes I cited the following observation from Hort:

A theoretical presumption indeed remains that a majority of extant documents is more likely to represent a majority of ancestral documents at each stage of transmission than *vice versa*.¹⁰

Of course, Westcott and Hort did not believe this theoretical presumption was actually the case. But at least they acknowledge it for what it is—namely, a proposition antecedently more probable than its opposite. I have the feeling from Fee's critique that he definitely does not recognize the force of this consideration.

As a matter of fact it seems clear that Fee has completely misconstrued my point. He asks, for example:

Will those copies geographically closer automatically also be textually closer? The latter is Hodges' assumption, but nothing inherently demands that we believe it.¹¹

But that most certainly is not my assumption. I am not talking about geographical proximity at all, as a close reading of my text should have shown. Indeed, in the very next sentence after my statement of principle I added:

The further removed in the history of transmission a text becomes from its source the less time it has to leave behind a large family of offspring.¹²

⁷Fee is very unfair to accuse me of both inveighing against rationalism and employing it (*ibid.*). In the article to which he refers I carefully defined the kind of rationalism I objected to as the sort which, in regard to revelation, "demands that its alleged truths and injunctions shall submit themselves to reason for testing and approbation" ("Rationalism and Contemporary New Testament Textual Criticism," *BSac* 128 [1971] 28 n. 7). This certainly does not mean that I abjure the use of reason or rational arguments within the framework of revealed truth, any more than Fee does. I do, however, object when unbelieving assumptions are brought to bear on textual questions. The dangers of this ought to be evident.

⁸For the most part, Fee's discussion of "The Problem of Theological Perspective" ("Criticism," pp. 21-24) is irrelevant to the position I actually hold, however much it might reflect the views of others.

⁹*Ibid.*, pp. 21, 25; citing Hodges, "The Greek Text of the King James Version," originally published in *BSac* 125 (1968) 344.

¹⁰B. F. Westcott and F. J. A. Hort, *The New Testament in the Original Greek* (Cambridge and London: Macmillan, 1881), 2. 45; cited by Hodges, "Greek Text," p. 344 n. 26.

¹¹Fee, "Criticism," p. 26.

¹²Hodges, "Greek Text," p. 344.

The crucial phrase here is "history of transmission." I am not speaking geographically but genealogically. What I affirm is that a manuscript high up on the genealogical tree has a better opportunity to leave a large number of descendants than one much lower on the tree. All surviving manuscripts are, of course, descendants of the autograph as well as of the direct copies of the autograph. This is the same as saying that all human beings are descendants of Noah and of Noah's three sons, Shem, Ham and Japheth. A great-great-great-grandson of Noah is not likely to have more lineal descendants alive in the world today than one of Noah's sons. In the same way it is not antecedently probable that a fourth-century copy of the NT will have more descendants among extant manuscripts than a first-century copy has. This, of course, is obvious and that is precisely why it was conceded by Hort.

The question I am really raising, however, is this. Why should the "theoretical presumption" acknowledged by Westcott and Hort be overturned? To overturn it in regard to the majority text leads, in my view, to insuperable problems. Even if we were to postulate that the majority text-form began its career as early as, say, the archetype of P⁷⁵ and B—that is, certainly not after the close of the second century—how did it come to be copied so much more frequently than the parent of P⁷⁵ and B?

But the problem does not end there. If, as Fee apparently thinks,¹³ P⁷⁵ and B represent the purest form of text that has survived, must it not have existed in many copies at one time before the majority text made its appearance? But if so, why was it almost driven off the field by a later—and far less pure—form of text?

Westcott and Hort's answer to this is well known. Their so-called "Syrian text" was the product of an official, ecclesiastical revision that spread by virtue of the ecclesiastical sanction with which it was promulgated. This view is now recognized as an historical improbability and is generally abandoned. But it must be admitted that, had it been possible to verify it, it would have constituted an adequate transmissional explanation for the majority text. The collapse of the recensional view of the majority text, however, has left a vacuum that contemporary theorists have not been able to adequately fill. Fee shows this, I think, in his *JETS* article.

Before I address the arguments on this point that Fee has offered, let me restate the problem so that its magnitude may be more clearly perceived. At whatever point we postulate the first appearance of the majority text as a recognizable textual entity—even if we date it to the close of the second century—at that point the exemplar that contained it was a minority of one. If a hundred years of copying had elapsed in the case of many of the NT autographs, there must have been literally hundreds of venerable copies of the NT books spread all over the Roman

¹³Cf., for example, G. D. Fee, "Rigorous or Reasoned Eclecticism—Which?", *Studies in New Testament Language and Text* (ed. J. K. Elliott; Leiden: Brill, 1976) 180-181. Cf. also his "P⁷⁵, P⁶⁶, and Origen: The Myth of Early Textual Recension in Alexandria," *New Dimensions in New Testament Study* (eds. R. N. Longenecker and M. C. Tenney; Grand Rapids: Zondervan, 1974) 44-45.

world and by now well established in the use of the various churches. How then, we ask, did this single lonely exemplar representing the majority text form manage to become the parent of a vast eighty per cent or so of the surviving manuscripts, while the hundreds of other copies coeval with it are represented today in only about twenty per cent of the surviving witnesses? That would be a little like maintaining that Eber, the great-great-grandson of Noah, has more descendants alive today than all the remaining children of Shem, Ham and Japheth put together. It would be like saying that those descendants outnumber the rest by a ratio of 8:2!

My brother, David M. Hodges, who possesses the mathematical background that I lack, has helped me to articulate an argument based on statistical probabilities to show how unlikely it is that the majority text could have achieved its commanding position in the extant materials unless it had always been a majority text. His material appears in the appendix of Wilbur N. Pickering's recent book.¹⁴ I will not attempt to repeat his presentation here, except to say that the impression I have just given that the hypothesis with which we are dealing is highly improbable on its face can also be affirmed in more technical ways.

In attempting to explain how the majority text became dominant, Fee—who calls this text Byzantine as do many others—points to several factors. He claims that “by the fourth century all of the factors that led to diversity had been superseded by their opposites.”¹⁵ In this connection he mentions the emergence of trained Christian scribes and the concept of canon that brought ecclesiastical concern with the wording *per se*.¹⁶ Even if we could grant these rather loose and undocumented generalizations, they would hardly explain how scribes and ecclesiastics managed to bring order out of the comparative chaos bequeathed to them from the earlier centuries of copying.¹⁷

Fee makes a special point out of the fact that “instead of copies being made to be carried off to some other center copies were now being made to remain where they were, for study purposes.”¹⁸ He then adds: “Herein lies one of the most significant factors both for ‘dominance’ and uniformity.”¹⁹ But Fee’s conclusion does not follow from his premises. If texts remained where they were in this period, the result must have been the perpetuation of the local text forms inherited from the previous centuries. For a single text form to emerge

¹⁴W. N. Pickering, *The Identity of the New Testament Text* (Nashville: Nelson, 1977). The material by D. M. Hodges is found in Appendix C, pp. 151-156.

¹⁵Fee, “Criticism,” p. 29.

¹⁶*Ibid.*

¹⁷Fee seems to admit this and lays only limited weight on the factors mentioned (*ibid.*). This caution is well advised.

¹⁸*Ibid.*, p. 30.

¹⁹*Ibid.*

and spread until it dominated the transmission in many places requires more communication between centers of copying, not less.

Fee also appeals to the influence of Chrysostom. He asserts:

It is almost inevitable that the text form Chrysostom used first at Antioch and then later carried to Constantinople should become the predominant text of the Greek Church.²⁰

In making this claim, Fee is very incautious indeed. I am reminded of the statement made many years ago by Kirsopp Lake:

Writers on the text of the New Testament usually copy from one another the statement that Chrysostom used the Byzantine, or Antiochan, text. But directly any investigation is made it appears evident, even from the printed texts of his works, that there are many important variations in the text he quotes, which was evidently not identical with that found in the MSS of the Byzantine text.²¹

More recently Metzger has called attention to the work of Geerlings and New on the text of Chrysostom. Metzger writes as follows:

It has often been stated by textual scholars that Chrysostom was one of the first Fathers to use the Antiochan text. This opinion was examined by Jacob Geerlings and Silva New in a study based on evidence which, in default of a critical edition, was taken from Migne's edition of Chrysostom's *opera*. Their conclusions are that "Chrysostom's text of Mark is not that of any group of manuscripts so far discovered and classified. . . . His text of Mark, or rather the text which can faintly be perceived through his quotations, is a 'mixed text,' combining some of the elements of each of the types which had flourished before the end of the fourth century." ²²

I quote now from Geerlings and New themselves where, speaking of Chrysostom's text of Mark, they say:

No known manuscript of Mark has the text found in Chrysostom's homilies, or anything approaching it. And probably no text which existed in the fourth century came much nearer to it.²³

Later Geerlings and New add this about Chrysostom's text:

The number of variants from the *Textus Receptus* is not appreciably smaller than the number of variants from Westcott and Hort's text. This proves that it is no more a typical representative of the late text (von Soden's K) than it is of the Neutral text.²⁴

Unfortunately, Fee has ignored this research. Otherwise he could never have stated that it was "almost inevitable" that the text used by Chrysostom "should become the predominant text of the Greek Church." Indeed,

²⁰Ibid.

²¹K. Lake, *The Text of the New Testament* (6th ed., rev. by S. New; London: Rivingtons, 1949) 53.

²²B. M. Metzger, "The Lucianic Recension of the Greek Bible," *Chapters in the History of New Testament Textual Criticism* (Leiden: Brill, 1963) 21-22.

²³J. Geerlings and S. New, "Chrysostom's Text of the Gospel of Mark," *HTR* 24 (1931) 135.

²⁴Ibid., p. 141.

by making this unjustified claim Fee has only proved my point. It is by no means easy to explain the rise and dominance of the majority form of text.

The collapse of Fee's argument on the text of Chrysostom permits us also to dispose of his final point. Let me quote Fee's own words: "The most important factor for the dominance and general uniformity of the Byzantine text is directly related to (b) above."²⁵ In Fee's paper (b) referred to his point about Chrysostom. Fee continues:

By the end of the seventh century the Greek NT was being transmitted in a very narrow sector of the Church—viz., the Greek Orthodox Church with its dominant patriarchate in Constantinople. By the time of Chalcedon Greek is almost unknown in the west, and after Chalcedon the decline of Alexandria and the subsequent rise of Islam narrow Greek-speaking Christendom still further.

All of these factors together ensure both the dominance and general uniformity of a text form properly called Byzantine.²⁶

Let me begin by expressing my opinion that in describing the decline of Greek copying outside the Byzantine empire Fee has somewhat overdrawn the picture. The Muslims, for example, did not attempt to exterminate Christianity. Christians were not compelled to convert and, though sometimes forbidden to engage in religious activities outside the Church, could continue their worship.²⁷ There are evidently no grounds for claiming that the Muslim conquests halted MS transmission in the areas controlled by them, though obviously Christianity withered in north Africa and elsewhere.

Equally, the decay of Greek in the west did not terminate manuscript transmission there either. As is well known, the monasteries became the conservators of books and culture. One might readily think of Cassiodorus (c. 485-c. 580) who founded two Benedictine monasteries at Vivarium and introduced the work of copying manuscripts, both classical and Christian.²⁸ Nor was the period following Chalcedon (451) as static or inert as Fee's comments might imply. One should recall the long and enlightened reign of Theodoric (493-526) at Ravenna in eastern Italy. As a hostage in Constantinople he had acquired some Greek and Latin learning and was an admirer of all things Roman. His reign might well be described as the first Renaissance.²⁹ After his death (526) Italy was in disarray, but Belesarius, the brilliant general of Justinian I, eventually recovered Italy, north Africa and southern Spain for the eastern empire. This supremacy was brief but helps to show the con-

²⁵Fee, "Criticism," p. 30.

²⁶Ibid.

²⁷Cf. D. Stewart, *Early Islam* (New York: Time-Life Books, 1967) 58.

²⁸F. H. Littell, *Atlas History of Christianity* (New York: Macmillan, 1976) 25.

²⁹G. Simons, *Barbarian Europe* (New York: Time-Life Books, 1968) 41.

tinuing cross-contacts between east and west. We may feel sure that manuscripts moved back and forth during a period such as this.

Also to be taken into account is the so-called "Carolingian Renaissance" during the reign of Charlemagne in the ninth century. During this period the energies of the scholars were especially directed at the Scriptures and books of the Church fathers. While knowledge of Greek was probably quite limited it was of interest to the Carolingians, and even Charlemagne is said to have learned some. The copying of ancient books was a major concern of this Renaissance, and by the end of the ninth century at least a dozen monasteries had libraries of a few hundred books. It has been said that "no Roman work that had survived long enough to be copied by Charlemagne's scholars was subsequently lost."³⁰ It need hardly be said that the copying of the Greek Scriptures in the Carolingian centers of learning could be carried out even by scribes who did not themselves know Greek. In fact, the copying of NT MSS went right on through the Middle Ages as the survival of bilingual Graeco-Latin codices bears witness. To imagine, therefore, the extinction of text-types in the west is to severely overdraw the picture.

Accordingly I think that Fee is guilty of a vast oversimplification of the transmissional history of the NT text outside of the Byzantine east. Nevertheless, I could grant him his points about north Africa and the west and still his explanation of the rise and dominance of the majority text would be completely without foundation. Already I have shown that Fee overlooks research on the text of Chrysostom, to which he has linked his final arguments, and that Chrysostom cannot be used as a vehicle for explaining the spread of a standardized text in the eastern empire. But there is another piece of research that Fee also ignores and that has disastrous consequences for his argument. I am referring to a series of studies undertaken by J. Neville Birdsall on the text of Photius in the gospels, the Acts and the epistles.³¹ Photius,

³⁰Ibid., p. 110. A statement made by D. Bullough in *The Age of Charlemagne* (London: Elek Books, 1965) 23 reminds us of another factor not to be overlooked in this discussion. Of the use of Greek in the west in the early eighth century he writes that it was "fully understood by few laymen or clergy, except for those who had fled from east Mediterranean lands to escape a zealously orthodox emperor or Arab domination." Such men, it ought to be observed, no doubt often brought Biblical manuscripts with them. In the same way the intercourse between the courts of Charlemagne and the eastern emperor must have led to Greek manuscripts going from west to east. It seems artificial to imagine that there was not continued intermixture between the textual traditions of all parts of the Christian world, and there is certainly nothing to show that the majority text was an eastern tradition only. The Greek text of Codex Bezae—a monument of textual peculiarity—is not at all likely to have ever existed in more than a very small number of Greek codices. As is well known, the so-called "Western text" is so amorphous and so spasmodically and irregularly diffused through the witnesses claimed for it that its right to be called a "text-form" at all is far from established. I agree with Aland that, if we are going to talk about text-types, we can speak only of the majority form and the Egyptian. Cf. K. Aland, "The Significance of the Papyri for Progress in New Testament Research," *The Bible in Modern Scholarship* (ed. J. Philip Hyatt; Nashville: Abingdon, 1965) 336. I therefore demur from Fee's statement that "the other type of text that existed in the second century is commonly called 'Western'" ("Criticism," p. 17). What we are probably looking at in the alleged Western readings is a complex phenomenon of mixture in which the versions played a major role and which had little impact at any time on the Greek manuscript tradition as a whole.

³¹J. N. Birdsall, "The Text of the Gospels in Photius: I," *JTS* 7 (1956) 42-55; "The Text of the Gospels in Photius: II," *JTS* 7 (1956) 190-198; "The Text of the Acts and the Epistles in Photius," *JTS* 9 (1958) 278-291.

of course, was a scholar, statesman, theologian and bibliophile who was also a patriarch of Constantinople in the ninth century. The ninth century, one must note, is a very substantial amount of time after Chalcedon and the beginning of the Muslim conquests. It has normally been regarded as the heyday of the so-called Byzantine text, and if anyone should have been using the official, or standard, text of the empire Photius should have. But Photius was not, as Birdsall's studies showed, and this has rightly raised suspicions that the concept of a standardized Eastern text is an historical fiction.

I think that Jacob Geerlings has come very close to the conclusion that a careful examination of the facts forces upon us. Of the so-called Byzantine text he has written:

Its origins did not wholly center in Constantinople, nor was its evolution the concern of either ecumenical councils or patriarchs. . . . Its origins as well as those of the other so-called texttypes probably go back to the autographs.³²

I entirely agree that the origins of the majority text go back to the autographs, though I think we may well question whether this is true for the other so-called text-types.

What I am saying, therefore, is precisely what Westcott and Hort recognized long ago as a "theoretical presumption"—that is, a majority of extant documents presupposes a majority of ancestral documents at every stage of transmission reaching back. Another way of putting this is that the majority text is a majority text in the surviving documents precisely because it has always been the majority text since the autographs themselves began to be copied. Such a phenomenon would constitute a perfectly normal kind of transmissional history for the NT writings, and nothing that Fee has asserted is in any way adequate to persuade us to the contrary.³³

No one denies that the text of P⁷⁵ and B is very old, and that their

³²J. Geerlings, *Family E and Its Allies* (Salt Lake City: University of Utah Press) 1.

³³Fee exhibits several misconceptions in reference to this kind of argument. (1) He argues that apart from a systematic attempt to check copies with earlier ones, the process of transmission leads to a text further and further removed from the original ("Criticism," p. 26). But this is not true of the manuscripts *en masse*, since the errors of any particular line of descent will normally occur in places peculiar to that line while the remainder of the lines of descent will reproduce their own inherited reading. No one denies, of course, that coincidence in errors can occur in more than one transmissional line, but the point is that the autograph reading is not likely by coincidence to be expunged over and over again in all or most of the lines of descent. I should think that this point would be obvious. (2) On a "normal" view of transmission there would be no comparable "mystery" about the origin of the Egyptian text as there is, on Fee's view, about the majority text (cf. *ibid.*, pp. 28-29). The Egyptian text can be easily explained transmissionally as an "offshoot" of the mainstream of the tradition, but to postulate the posteriority of the majority is to postulate the emergence of a new mainstream and the reduction of the old mainstream to a tiny rivulet! (3) Fee seeks to disallow the argument from the history of the Vulgate text, which I had used to show that a new form of text cannot achieve uniformity when pitted against an older and widespread rival (*ibid.*, p. 26). But he seems to miss my point that the prior existence of the Old Latin text-forms was what made this impossible for the Vulgate tradition. The majority text, therefore, must have had no "Old Greek" to compete against, since it achieved the uniformity that the Vulgate tradition lacks. As to his assertion that the copies of the Vulgate are "far more like one another than they are like Jerome's original" (*ibid.*), I can only say that the statement strikes me as wildly untrue.

common archetype can easily be supposed to belong somewhere in the second century. But the relative paucity of surviving Greek witnesses to this form of text argues that it is not likely to have originated as high up on the NT family tree as the representatives of the majority form. Of course, Fee repeats the standard argument to the effect that the majority text form "is completely unknown by any of the evidence up to A. D. 350, the earliest evidence being found in some Church fathers, then later in the fifth century in portions of Codices W and A."³⁴ This statement, however, is debatable on a number of grounds, among which it may be pointed out that a good case can be made that the ante-Nicene patristic evidence gives more support to the majority form of text than to the kind exhibited by P⁷⁵ and B.³⁵

Nevertheless, the point I chiefly want to make is that the surviving evidence is itself a demonstration that the majority text reaches back into the most remote period of textual transmission. If the textual relationship between P⁷⁵ and B may be justifiably presumed to push their common ancestor back into the second century, what can be said of the majority form which, despite its relative internal consistency, can nevertheless be subdivided into numerous families and subfamilies?

For example, quite some time ago Silva Lake made a study of Family π and Codex Alexandrinus in which it was concluded that π (a ninth-century uncial) headed a subfamily related to, but not descended from, the fifth-century Codex A. This required the postulate that the ninth-century codex and the fifth-century codex were descended from a common ancestor older than either of them, and surprisingly the ninth-century descendant was adjudged closer to this mutual ancestor than the fifth-century descendant.³⁶ Thus it can be said that the so-called Byzantine text found in Codex Petropolitanus (π) in the ninth century existed substantially in an ancestor that must have been coeval with Aleph and B, if it was not actually older than they. That we have not found this ancestor in no way sets aside the fact of its existence. But let it be emphasized that this was an ancestral document for only one small branch of the majority text. Undoubtedly we must hypothesize many such documents stretching back to the remotest periods of copying.

It has often been pointed out that only the climate of Egypt is favorable to the preservation of the most ancient documents, especially from the papyrus period. But even here we should resist drawing the conclusion that we have an adequate sampling of the texts current there in the earliest period. The most highly prized and most commonly used papyrus texts are precisely those most likely to perish from repeated use, just as your favorite Bible will wear out more rapidly than a version you consult only occasionally. As Silva Lake's study suggests, the Egypt-

³⁴Ibid., p. 25.

³⁵Cf. Pickering, *Identity*, pp. 62-77.

³⁶S. Lake, *Family π and the Codex Alexandrinus: The Text According to Mark* (London: Christophers, 1937).

tian Codex A, which is considered to be Byzantine in the gospels, was descended from an earlier exemplar. But whatever Egyptian ancestors it might have had, they have not survived, while B and Aleph have. Ironically, it seems not improbable that the very best codices known in ancient Egypt are not the ones we now possess.³⁷ But even so, let it be pointed out that every new papyrus find—even in Egypt—brings with it a fresh accession of evidence for majority text readings previously found chiefly in later manuscripts.

Indeed, as long ago as Zuntz' celebrated study of the Pauline corpus in P⁴⁶ it could be observed:

A number of Byzantine readings, most of them genuine, which previously were discarded as 'late,' are anticipated by P⁴⁶.³⁸

The fact of the matter is that even P⁷⁵, despite its close relationship with B, can be found supporting readings also attested in the majority tradition *against* the testimony of its famous uncial relative.³⁹ There is nothing surprising about all this. If the majority text ultimately antedates all forms of the text, then all manuscripts will reflect it to a greater or lesser degree. The sporadic appearance of so-called late Byzantine readings in early papyrus texts are, on this view, quite comprehensible as survivals of the *Ur-text* that lies behind the entire tradition.

I must make one final point before drawing this discussion to a conclusion. In a study that has not received the attention it merits Kirsopp Lake, R. P. Blake and Silva New carried out an investigation in which they collated the eleventh chapter of Mark in all the available manuscripts at three great monastery centers in the east. I give their results in their own words:

This collation covers three of the great ancient collections of MSS; and these are not modern conglomerations, brought together from all directions. Many of the MSS, now at Sinai, Patmos, and Jerusalem, must be copies written in the scriptoria of these monasteries. We expected to find that a collation covering all the MSS in each library would show many cases of direct copying. But there are practically no such cases. What does this mean?

Before answering the question, it may be well to put another. Why are there only a few fragments (even in the two oldest of the monastic col-

³⁷How then can Fee affirm that "the point is that the Byzantine text simply did not exist in Egypt" ("Criticism," p. 27)? Did Codex A, in the gospels, have no Egyptian ancestors? But if it did, then the "Byzantine" text existed there at a time hardly distinguishable from the date of Aleph and B. Furthermore, despite recent papyrus accessions the data base for pronouncements about even the texts current in Egypt is much too small to warrant dogmatism. Who anticipated texts like P⁴⁵ and P⁶⁶ before they appeared?

³⁸G. Zuntz, *The Text of the Epistles* (London: Oxford University Press, 1953) 55.

³⁹Among many that might be cited, the following are places where P⁷⁵ and the majority text are in agreement against B: Luke 24:47 *arxamenon* P⁷⁵ MAJ. // *arxamenoi* Aleph B, al.; John 1:15 *Outos ên on eipon* P⁷⁵ P⁶⁶ MAJ. // *Outos ên o eipôn* B* C*; John 6:52 *-autou* P⁷⁵ vid Aleph MAJ. // *+autou* p P⁶⁶ B al.; John 7:4 *autos en parresia* P⁷⁵ Aleph MAJ. // *auto en parresia* P⁶⁶ B W.

lections, Sinai and St. Saba) which come from a date earlier than the 10th century?

There must have been in existence many thousands of manuscripts of the gospels in the great days of Byzantine prosperity, between the fourth and the tenth centuries. There are now extant a pitifully small number. Moreover, the amount of direct genealogy which has been detected in extant codices is almost negligible. Nor are many known MSS sister codices. The Ferrar group and family 1 are the only reported cases of the repeated copying of a single archetype, and even for the Ferrar group there were probably two archetypes rather than one. . . . There are cognate groups—families of distant cousins—but the manuscripts which we have are almost all orphan children without brothers or sisters.

Taking this fact into consideration along with the negative result of our collation of MSS at Sinai, Patmos, and Jerusalem, it is hard to resist the conclusion that the scribes usually destroyed their exemplars when they had copied the sacred books.⁴⁰

I think the information I have just cited points plainly toward two conclusions: (1) The ancestry of the majority text, if we could reconstruct it, would be long and complex; and (2) beside the other factors we have mentioned we must also add that the nonsurvival of many ancient exemplars may well be directly traced to the scribal practice of destroying them.

From all I have presented here, I trust it may be concluded that there is much to be said in favor of the form of Greek text found in the great mass of extant witnesses. There is much more that could be said, including detailed arguments for the superiority of individual majority readings over their rivals adopted in modern critical editions.⁴¹ But suffice it simply to add that a re-examination of the claims of the majority text seems to me to be an idea whose time has come. My own correspondence and contacts indicate that the views expressed in this paper have many friends both in America and abroad. With the appearance shortly of an edition of the Greek NT reflecting the majority form of text, discussion is bound to increase. I sincerely hope that no informed person will pretend that there is nothing to discuss and I also hope that the dialogue that is sure to follow will be carried on at a high level of accuracy and fairness and with a maximum of Christian grace.

⁴⁰K. Lake, R. P. Blake and Silva New, "The Caesarean Text of the Gospel of Mark," *HTR* 21 (1928) 348-349.

⁴¹Fee argues in his paper, as he has elsewhere, that there are adequate grounds for concluding that P⁷⁵ and B are among our "best manuscripts" ("Criticism," pp. 30-33; see also "P⁷⁵, P⁶⁶, and Origen," esp. pp. 31-45, and "Eclecticism," *passim*). But here his argument is not simply with me but also with the whole school of eclectics headed by E. D. Kilpatrick and J. K. Elliott. It goes without saying that his views on this matter have by no means won universal assent. Indeed, he seems to be insisting that there are certain kinds of internal criteria that are more decisive than other kinds in determining the basic character of a text, but in making this claim Fee will not escape the charge of circularity that is easily levelled against it—viz., the "better" manuscripts are *still* those that contain the "better" readings. Obviously, different critics will have differing views about what internal criteria are important or decisive, and if the eclectics have taught us nothing else they have at least taught us that almost any reading (even those in the majority text!) can be defended by a critic who possesses adequate sophistication. In the absence of a satisfactory overview of the history of transmission, Hort's famous dictum about the "internal evidence of readings" remains a dead-end street.