

## MODERN TEXTUAL CRITICISM AND THE MAJORITY TEXT: A SURREJOINDER

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The editor of *JETS* has very kindly allowed this final word of response to Gordon Fee's rejoinder to my paper. The rejoinder is rather disappointing because in the main Fee seems to be merely reasserting positions that are seriously questionable.

For example, his rejoinder claims knowledge of the technical studies on the texts of Chrysostom and Photius but does not seem to be aware that they seriously undermine his previous assertions. Fee had claimed that "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."<sup>1</sup> But with regard to the text of Chrysostom in Mark, Geerlings and New concluded that its variants from the TR were about the same as from Westcott-Hort and that "it is no more a typical representative of the late text (von Soden's K) than it is of the Neutral text."<sup>2</sup> Obviously, Fee cannot claim Chrysostom's influence as an "almost inevitable" factor in the spread of the so-called Byzantine text without calling the work of Geerlings and New into question. Does he really wish to do this?

In the same way, Birdsall's studies in the text of Photius point clearly to the conclusion that no official, ecclesiastical text existed in the eastern empire even by the ninth century. Thus the supposition that the Byzantine text prevailed due to somebody's influence is further undermined. Neither Chrysostom nor Photius is a good candidate for explaining the rise and dominance of the majority text. Is there some other candidate? If so, who?

This leads back to the major point under discussion. There seems to be no viable explanation for the existence of the majority text (even in the east!) other than the one proposed in our paper—namely, that the majority text is the result of a perfectly normal transmissional history from the times of the autographs. The point of quoting Hort in this connection was to show that even this great opponent of the majority form had to admit that "a majority of extant documents is *more likely to represent* [italics ours] a majority of ancestral documents at each stage of transmission than *vice versa*."<sup>3</sup> We are maintaining, therefore, the position that Hort acknowledges as presumptively "more likely." The evidence that Hort felt led to another conclusion has all

<sup>1</sup>G. Fee, "Modern Textual Criticism and the Revival of the *Textus Receptus*," *JETS* 21/1 (1978) 30.

<sup>2</sup>J. Geerlings and S. New, "Chrysostom's Text of the Gospel of Mark," *HTR* 24 (1931) 141.

<sup>3</sup>B. F. Westcott and F. J. A. Hort, *The New Testament in the Original Greek* (Cambridge and London: Macmillan, 1881), 2. 45.

been called into question—most notably, his theory of a Syrian recension. Fee has nothing to put in the place of Hort's famous explanation of the majority text and, until this can be done, the supposition of a "normal transmission" ought to be taken seriously. This idea is not—despite Fee's claim—an "irrelevancy." Pejorative language of this kind is no substitute for a reasoned rebuttal.

Fee continues to be less than fully accurate in the way he describes some of the data. For example, he insists that "in comparison with the Old Latins, the Vulgate manuscripts have 'general uniformity and dominance.'" But this is a very loose assertion when one considers the observation of Metzger that "the more than 8,000 Vulgate manuscripts which are extant today exhibit the greatest amount of cross contamination of textual types."<sup>4</sup> The Vulgate tradition does not exhibit the kind of uniformity found in the majority tradition of the Greek text—precisely because it sought to supersede the older text forms (OL). The kind of inner coherence exhibited by the Greek majority text is scarcely explainable if it was seeking to replace an abundance of older text forms (as modern criticism has to maintain) that were influential and deeply entrenched, as was the OL. Once again, the transmissional phenomena in the majority tradition receive their best explanation if normal transmission from the time of the autographa is postulated.

Despite Fee's claim that we "carefully avoid speaking to" his point about the absence of early examples of the majority text-type, the fact is that several paragraphs were devoted to precisely this issue. All textual scholars recognize that the data of criticism can necessitate the postulation of early, nonextant archetypes for later MSS. That is why we pointed to Silva Lake's study of Family  $\pi$ . One might also add that Josef Schmid, one of the leading researchers in the text of the Apocalypse, holds that the "K-text" of that book goes back into the fourth century, even though it has only one surviving uncial representative (046), and that of the ninth/tenth century!<sup>5</sup> What we maintain is that the extant evidence for the majority text demands a very extended transmissional history, and therefore its existence long before any of its surviving representatives were written must be assumed. The reader is referred back to my paper in this connection, but the assumption involved here is by no means unusual in text-critical studies.

It has not been maintained by us, as Fee assumes, that "Codex A represents the 'mainstream' in Egypt." It is entirely possible that the earliest form of text to reach Egypt was indeed the kind of text found in the papyri discovered so far. But the data to which Fee

<sup>4</sup>B. Metzger, *The Text of the New Testament* (2d ed.; New York and Oxford: Oxford University Press) 76.

<sup>5</sup>Schmid's statement is relevant to this whole discussion: "Die griechische Überlieferung bezeugt uns vielmehr nicht einem kirchlichen Normaltext (=K), der dann auf griechischem Boden alle anderen Textformen verdrängt hat, sondern vier Textformen, AC, P<sup>47</sup> S, Av, und K, und alle vier reichen nachweisbar ins 4 Jh. hinauf." Cf. J. Schmid, *Studien zur Geschichte des griechischen Apokalypse-Textes*, Teil 2: *Die alten Stämme* (München: Karl Zink Verlag, 1955) 9.

appeals will really go no further than to suggest that the text of the papyri was an early, local text in this part of the world. Reliable evidence for its existence outside of Egypt is scanty to say the least. What must be insisted upon, however, is that the denial of the early appearance of the majority form even in Egypt is without an adequate basis. Why must we exclude the strong possibility that the fifth-century Codex A, for example, had ancestors as old as P<sup>75</sup> that have not survived or have not yet been found? If, as Lake suggested, Codex  $\pi$  had an ancestor older than A (i. e., a good four hundred years older than  $\pi$ ), why may not this mutual ancestor of A and  $\pi$  have had an ancestor going back into the second century at least? Nothing that Fee has said renders such a suggestion inherently improbable.

Of course if the Egyptian texts we now possess represent the Egyptian mainstream—and the data base needs enlargement before that is quite certain—then there is nothing to surprise us that Origen and Cyril used it.<sup>6</sup> But must it not also be wondered whether early Egyptian copyists might not have often conformed their texts to the favored Egyptian form, just as Byzantine copyists are alleged to have done later in the patristic writings they transmitted? The problems of reliably using the data from the fathers for textual criticism are very complex and are often marred by presuppositions related to textual theory in general.

Fee urges that we “do” textual criticism rather than talk in terms of a general theory. But naturally it was not possible within the scope of a brief paper to get deeply involved in a mass of details. However, the forthcoming critical edition of the majority text and the planned discussions of its character that will follow should give ample scope for Fee and others to come to grips with how we handle the specific data. In the meanwhile, I can only urge him to be patient with us until the heavy task of editing the NT is completed.

Finally, Fee has complained that we have ignored the main thrust of his critique. But it was necessary, as my paper indicated, to pass by his entire section on “The Problem of Theological Perspective” because his strictures did not at all apply to the position I actually hold. In his final footnote he seems to wish to continue to tag me with a theological slant that I have explicitly disavowed. The fact that I allowed an article of mine to be reprinted in a volume all of whose perspectives I did not share should not be used against me. I am sure Fee’s writings have appeared in journals and books along with materials to which he could not subscribe. It seems more likely that the present

<sup>6</sup>Thus, despite Fee’s call, there seems little that requires a response on my part to his studies on Cyril and Origen to which he refers (*Bib* 52 [1971] 357-394). I urge the reader of these studies to be cautious, however, because Fee’s data base is limited to John 4. Thus he computes Cyril’s agreement with B at 71.9 and with TR at 64.6 (see the chart between pp. 366 and 367). But on p. 365 he indicates that Cyril agrees with B against TR a total of 22 times, but with TR against B, 19 times. With a difference of only three variations here, how significant is the percentage spread? I think a much more extended study of Origen and Cyril over a much greater expanse of text is called for before Fee draws far-reaching conclusions from his data about textual history.

discussion can be carried on with greater profit if all parties—whether on my side or on Fee's—will abjure the *argumentum ad hominem* and will concentrate on examining the facts in a dispassionate and charitable way. Both sides can afford such an admonition.