

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

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I want to thank the editor of *JETS* for this opportunity to make some further comments on this subject in light of Zane Hodges' article. I do not here intend to write another critique of Hodges' methodology but simply to point out what I think are some difficulties and deficiencies in his response to my earlier article.

I was glad that Hodges took the opportunity in his response to clarify his own position, but I was considerably disappointed that that was basically all he did. In fact, apart from some initial more trivial items, the substance of Hodges' paper is a rebuttal of two paragraphs of mine on pages 25-26 and of my historical reconstruction of the reasons for the dominance and general uniformity of the Byzantine text (pp. 29-30). But anyone who reads my article will recognize that those items are scarcely the main thrust of my critique.

It is abundantly clear from Hodges' latest paper that his entire textual methodology finally rests on two items: (1) his "truism" that an immediate child of a parent will probably produce more offspring than a great-great-great-grandchild, and (2) the alleged inability of modern textual criticism to account for the precise origins of the majority text as a text-type. What Hodges apparently is not hearing is that modern textual criticism considers item one to be an irrelevancy. The second item, on the other hand, is a historical problem to be sure. But it is no more so than is the problem of accounting for the origin of the so-called Western or Egyptian texts, if one argues that neither of these represents the original. Hodges' answer to this problem is simply too glib and does not take seriously enough the only *extant* evidence from the second and third centuries.

These two items on which Hodges' methodology rests call for some further comments.

1. I repeat. The probability that "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" is an irrelevancy to NT textual criticism—for two reasons. First, this is simply a theoretical possibility that has nothing to do with probabilities in terms of manuscript transmission.

Hodges has argued that I have failed to note that Hort, at least, acceded to this point. But this is simply not true of Hort. He calls it a "theoretical presumption" only, and never allows that it is "antedecently more probable than its opposite." Precisely because we are

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dealing with manuscripts, not with human genealogy, Hort's next sentence was: "But the presumption is too minute to weigh against the smallest *tangible* evidence of other kinds."<sup>1</sup>

The point is that even if Hodges' "theoretical presumption" could be shown to have antecedent probability (and all the statistics and computers in the world do not count here), it can also be shown—theoretically—how it might *not* work out. My previous illustration may have been somewhat misleading (because of the "geographical" factor);<sup>2</sup> but my point is that anything done at the theoretical level is just that—theoretical, and nothing more. And Hort was right: Such a theoretical possibility is too minute to weigh against the actual historical evidence.

Second, as I pointed out in my article, it is a matter of historical record that time and again Hodges' "theoretical presumption" has *not* happened in manuscript transmission. It is not true of the Vulgate;<sup>3</sup> it is not true in the classics;<sup>4</sup> it is not true of countless works of the Church fathers; and, as most of us think, it is not true of the NT. One simply cannot make a case for NT text-critical methodology on a mere theoretical possibility.

2. The majority of Hodges' response is taken up with my all-too-brief description of how dominance and general uniformity probably came about. Nothing Hodges has said has seriously challenged my suggestions. The fact that Greek may have been known for a time in the west is simply irrelevant. The western fathers from Tertullian on used the Latin Bible, not Greek manuscripts, even if Greek was known by some of them. Furthermore, it is a matter of fact that the great majority of manuscripts for the Greek NT come from the monasteries and churches of the Byzantine empire—not from anywhere else. The powerful dominance of the Greek liturgy, with its lectionary tradition of a very Byzantine type of text, simply cannot be gainsaid.

<sup>1</sup>B. F. Westcott and F. J. A. Hort, *The New Testament in the Original Greek* (Cambridge and London: Macmillan, 1881), 2. 45. Italics mine.

<sup>2</sup>The "geographical" factor, however, is more important than Hodges allows. Let me try it again. Let us assume that two very Swedish parents (tall, blond, blue eyes, and so forth), Olaf and Helga Olson, have two boys, Karl and Sven. These two boys also marry Swedish girls, and each has two children. One of Karl's sons emigrates to the United States and marries a very Italian girl (short, black hair, dark eyes, and so on), and this son happens to be prolific and has twelve children. Of these children seven marry Italians, one marries a Swede and four marry American "mixtures." Meanwhile in Sweden catastrophe has struck the house of Olson and the two children of Sven are killed in an accident. The other son of Karl had one child, a daughter, who turned out to be barren. Thus the only descendants of Olaf Olson are in America, and the vast majority are now assuming very Italian features.

<sup>3</sup>Hodges (n. 33) still seems to miss the point here. Of course it is true that the "wild" Old Latin tradition continued to influence the Vulgate. But in comparison with the Old Latins, the Vulgate manuscripts have "general uniformity and dominance." And my statement that the copies of the Vulgate are "*far more like one another than they are like Jerome's original*" is not "wildly untrue." One might try collating any ten late-medieval Vulgate MSS with Wordsworth and White's edition and see the results. After all, the Sixtine and Clementine editions, for all their differences, agree more often with one another than either does alone with Wordsworth and White.

<sup>4</sup>This point was made long ago by F. G. Kenyon, *The Text of the Greek Bible* (rev. ed.; London: Duckworth, 1949) 202-203.

But Hodges' point about Chrysostom is most interesting. As he should know, I am well aware of the Chrysostom and Photius studies he mentions. His attempt to dissociate these two fathers from the Byzantine text seems to underline the point I had made: "These fathers [Chrysostom *et al.*] had a NT only about ninety per cent along the way to the full Byzantine text of the later Middle Ages" (p. 28). But Chrysostom's text is still closer to the "majority text" than it is to any text of the second or third century.<sup>5</sup>

My further point is that even the so-called majority text is a text that evolved.<sup>6</sup> As the extant evidence clearly indicates, and as I pointed out in my article (pp. 28-29), there is no full example of this text until after the eighth century, a point that Hodges carefully avoids speaking to. The later medieval manuscripts simply have a greater degree of the type of textual corruption found as early as the scribe of P<sup>66</sup> and found to predominate as early as Chrysostom; and anyone who suggests that Chrysostom did not have a powerful influence on the eastern Church plays loose with history.

Finally, let it be said again that if modern textual criticism has difficulty explaining the precise origin of the Byzantine text, Hodges' problems of explanation are far greater. Some of us would insist that he stop talking in theories and generalities and demonstrate by numerous and substantial examples from the extant data itself that his theories are also historical. In this regard we would urge that he stop treating the actual evidence so cavalierly and start taking it seriously.<sup>7</sup> He is urged to show where in the early centuries up to A. D. 350 there is a single witness to the majority text as a full member of the majority

<sup>5</sup>See my study, "The Text of John in Origen and Cyril of Alexandria: A Contribution to Methodology in the Recovery and Analysis of Patristic Citations," *Bib* 52 (1971) 357-394, esp. 369.

<sup>6</sup>A good analogy of this kind of evolution may be found in theology with the concept of purgatory. Two things appear historically certain: (1) It is not an "original" doctrine to the NT; (2) by the late Middle Ages it is the "majority" view. As with the majority text it is difficult to determine its precise origins; nonetheless, as with the majority text one can easily account for its emergence and its eventual predominance. But the doctrine can be shown to be secondary to primitive Christianity just as the majority text can be shown to be secondary to the original NT text.

<sup>7</sup>As examples, see his treatment of the patristic evidence and the evidence from Egypt. Thus Hodges says 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 P<sup>75</sup> and B." He supports this by citing Wilbur Pickering's recent study, which only weakens Hodges' case since Pickering ignores a whole bibliography of patristic studies that disprove his assertions. Pickering, for example, includes Origen in this list of fathers. But that is to fly full in the face of the actual data. (See my study mentioned in n. 5.) In the gospel of John, at least, Origen knows nothing of the readings peculiar to the Byzantine tradition.

The evidence from this study also calls into question Hodges' suggestion that P<sup>75</sup> and B are the "rivulet(s)" while Codex A represents the "mainstream" in Egypt (see his n. 33). If so why does Origen, who knew of texts and readings unlike his own, not even refer to Byzantine readings in such instances? Hodges chides me for saying that "the Byzantine text simply did not exist in Egypt" (n. 37). The context of that sentence makes it obvious that I meant in the second and third centuries. But how can I say that? Because of Clement, Origen, P<sup>66</sup>, P<sup>75</sup>, P<sup>46</sup> and the Coptic Versions—that is, from *all* the extant evidence from Egypt up to A. D. 300. When Byzantine readings do begin to appear in greater number, they do so only gradually and sporadically (Didymus, Athanasius, Cyril, A, W).

All of this is spelled out in detail in my study on the text of Origen and Cyril (see my n. 5). Hodges is called upon not to ask rhetorical questions but to respond in *Biblica* to the data of that study, showing it to be an incorrect interpretation of the facts.

text, and he should do this by a carefully controlled methodology over large sections of text. He is urged to show *inter alia* how harmonized readings are more likely to be original than nonharmonized ones and to show how a *nu*-movable before a consonant (a common feature in all kinds of early texts) is less likely to be original than the "correct" usage of a later time. He is urged to go through a large section of text and explain how *all* the variations came about and to show *why* the readings of the majority text are superior. In plain words, what is urged upon Hodges is that he do textual criticism, not merely count noses.

If this seems too harsh, it is not intended to be. I am only urging that if Hodges wishes to overturn Hort he must do so with real evidence, not with hypothetical but up to now nonexistent "manuscripts."<sup>8</sup>

<sup>8</sup>There are several minor points in Hodges' response that could also be noted. (1) My point on the theological concern still holds. Let one read Hodges' first two articles for himself, plus the fact that he and Pickering allowed their papers to be included in David Otis Fuller's books. As to rationalism, it is Hodges himself who tries to connect "unbelieving" rationalism with Westcott and Hort's methodology. But what he demonstrated instead was that they were simply good historians—as, hopefully, Hodges himself is trying to be. (2) Hodges' "whole school of eclectics" (n. 41) headed by Kilpatrick and Elliott happens to be a "whole" school of only two people! To be sure there are other eclectics who emphasize internal principles more than others do, but no one else to my knowledge offers such a blanket disregard of the MS evidence as they do. (3) Hodges' views may have many friends, but apparently none of them is actually working in textual criticism. What is needed is a whole production of scholarly materials in the leading journals—challenging, refuting and demonstrating on the basis of firm data.