

The oldest MSS are not the best

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What follows has been copied from my book, *The Identity of the New Testament Text* (first published in 1977), with some additions. Anyone wishing to check the references should consult that book.

Burton recognized the "antecedent probability" with these words:

The more ancient testimony is probably the better testimony. That it is not by any means always so is a familiar fact. . . . But it remains true, notwithstanding, that until evidence has been produced to the contrary in any particular instance, the more ancient of two witnesses may reasonably be presumed to be the better informed witness.¹

This **a priori** expectation seems to have been elevated to a virtual certainty in the minds of many textual critics of the XIX century. The basic ingredient in the work of men like Tregelles, Tischendorf and Hort was a deference to the oldest MSS, and in this they followed Lachmann.

The 'best' attestation, so Lachmann maintained, is given by the oldest witnesses. Taking his stand rigorously with the oldest, and disregarding the whole of the recent evidence, he drew the consequences of Bengel's observations. The material which Lachmann used could with advantage have been increased; but the principle that the text of the New Testament, like that of every other critical edition, must throughout be based upon the best available evidence, was once and for all established by him.²

Note that Zuntz here clearly equates 'oldest' with 'best'. He evidently exemplifies what Oliver has called "the growing belief that the oldest manuscripts contain the most nearly original text". Oliver proceeds:

Some recent critics have returned to the earlier pattern of Tischendorf and Westcott and Hort: to seek for the original text in the oldest MSS. Critics earlier in the 20th century were highly critical of this 19th century practice. The return has been motivated largely by the discovery of papyri which are separated from the autographs by less than two centuries.³

But, the "contrary evidence" is in hand. We have already seen that most significant variants had come into being by the year 200, before the time of the earliest extant MSS, therefore. The **a priori** presumption in favor of age is nullified by the

¹ Burton, *The Traditional Text*, p. 40. I disagree. Great age in a manuscript should arouse our suspicion: how could it have survived for over 1,500 years? Why wasn't it used and worn out?

² Zuntz, *The Text*, pp. 6-7.

³ Oliver, pp. 312-13.

known existence of a variety of deliberately altered texts in the second century. Each witness must be evaluated on its own. As Colwell has so well put it, "the crucial question for early as for late witnesses is still, 'WHERE DO THEY FIT INTO A PLAUSIBLE RECONSTRUCTION OF THE HISTORY OF THE MANUSCRIPT TRADITION?'"¹

It is generally agreed that all the earliest MSS, the ones upon which our eclectic ('critical') texts are based, come from Egypt.

When the textual critic looks more closely at his oldest manuscript materials, the paucity of his resources is more fully realized. All the earliest witnesses, papyrus or parchment, come from Egypt alone. Manuscripts produced in Egypt, ranging between the third and fifth centuries, provide only a half-dozen extensive witnesses (the Beatty Papyri, and the well-known uncials, Vaticanus, Sinaiticus, Alexandrinus, Ephraem Syrus, and Freer Washington).² [To these the Bodmer Papyri must now be added.]

But what are Egypt's claims upon our confidence? And how wise is it to follow the witness of only one locale? Anyone who finds the history of the text presented herein to be convincing will place little confidence in the earliest MSS.

Their quality judged by themselves

Quite apart from the history of the transmission of the text, the earliest MSS bear their own condemnation on their faces. P⁶⁶ is widely considered to be the earliest extensive manuscript. What of its quality? Again I borrow from Colwell's study of P⁴⁵, P⁶⁶, and P⁷⁵. Speaking of "the seriousness of intention of the scribe and the peculiarities of his own basic method of copying", he continues:

On these last and most important matters, our three scribes are widely divided. P⁷⁵ and P⁴⁵ seriously intend to produce a good copy, but it is hard to believe that this was the intention of P⁶⁶. The nearly 200 nonsense readings and 400 itacistic spellings in P⁶⁶ are evidence of something less than disciplined attention to the basic task. To this evidence of carelessness must be added those singular readings whose origin baffles speculation, readings that can be given no more exact label than carelessness leading to assorted variant readings. A hurried count shows P⁴⁵ with 20, P⁷⁵ with 57, and P⁶⁶ with 216 purely careless readings. As we have seen, P⁶⁶ has, in addition, more than twice as many "leaps" from the same to the same as either of the others.³

¹ Colwell, "Hort Redivivus", p. 157.

² Clark, "The Manuscripts of the Greek New Testament", p. 3.

³ Colwell, "Scribal Habits", pp. 378-79.

Colwell's study took into account only singular readings—readings with no other MS support. He found P⁶⁶ to have 400 itacisms plus 482 other singular readings, 40 percent of which are nonsensical.¹ "P⁶⁶ editorializes as he does everything else—in a sloppy fashion."² In short, P⁶⁶ is a very poor copy and yet it is one of the earliest! P⁷⁵ is placed close to P⁶⁶ in date. Though not as bad as P⁶⁶, it is scarcely a good copy. Colwell found P⁷⁵ to have about 145 itacisms plus 257 other singular readings, 25 percent of which are nonsensical.³ Although Colwell gives the scribe of P⁷⁵ credit for having tried to produce a good copy, P⁷⁵ looks good only by comparison with P⁶⁶. (If you were asked to write out the Gospel of John by hand, would you make over 400 mistakes?⁴ Try it and see!) It should be kept in mind that the figures offered by Colwell deal only with errors which are the exclusive property of the respective MSS. They doubtless contain many other errors which happen to be found in some other witness(es) as well. In other words, they are actually worse even than Colwell's figures indicate.

Nor is that all; Colwell goes on to say:

In general, P⁷⁵ copies letters one by one; P⁶⁶ copies syllables, usually two letters in length. P⁴⁵ copies phrases and clauses.

The accuracy of these assertions can be demonstrated. That P⁷⁵ copied letters one by one is shown in the pattern of the errors. He has more than sixty readings that involve a single letter, *and* not more than ten careless readings that involve a syllable. But P⁶⁶ drops sixty-one syllables (twenty-three of them in "leaps") and omits as well a dozen articles and thirty short words. In P⁴⁵ there is not one omission of a syllable in a "leap" nor is there any list of "careless" omissions of syllables. P⁴⁵ omits words and phrases.⁵

Now that is very instructive. As a linguist (PhD), I have no qualms in affirming that the persons who produced P⁶⁶ and P⁷⁵ did not know Greek. Had they understood the text they would not have made the number and sort of mistakes that they did. Just stop and think: when copying a language that you know, you copy phrase by phrase, or at the very least, word by word. Those MSS are old, but they are very

¹ *Ibid.*, pp. 374-76.

² *Ibid.*, p. 387.

³ *Ibid.*, pp. 374-76.

⁴ I am probably being unfair to the scribe who produced P⁷⁵—some or many of those errors may have been in his exemplar. The fact remains that whatever their origin P⁷⁵ contains over 400 clear errors and I am trying by the suggested experiment to help the reader visualize how poor these early copies really are. Carson takes a different view. "If P⁷⁵, a second-century papyrus [?], is not recensional, then it must be either extremely close to the original or extremely corrupt. The latter possibility appears to be eliminated by the witness of B" (p. 117). How so? If P⁷⁵ is "extremely corrupt" and B was copied from it, or something similar, then B must also be extremely corrupt. (Hoskier supplies objective evidence to that effect in *Codex B and its Allies*.)

⁵ *Ibid.*, p. 380.

poor examples of how to make a copy. They demonstrate that the age of a MS does not guarantee quality in copying; nor does it guarantee the quality of its exemplar.

P⁴⁵, though a little later in date, will be considered next because it is the third member in Colwell's study. He found P⁴⁵ to have approximately 90 itacisms plus 275 other singular readings, 10 percent of which are nonsensical.¹ However, P⁴⁵ is shorter than P⁶⁶ (P⁷⁵ is longer) and so is not comparatively so much better as the figures might suggest at first glance. Colwell comments upon P⁴⁵ as follows:

Another way of saying this is that when the scribe of P⁴⁵ creates a singular reading, it almost always makes sense; when the scribes of P⁶⁶ and P⁷⁵ create singular readings, they frequently do not make sense and are obvious errors. Thus P⁴⁵ must be given credit for a much greater density of intentional changes than the other two.²

As an editor the scribe of P⁴⁵ wielded a sharp axe. The most striking aspect of his style is its conciseness. The dispensable word is dispensed with. He omits adverbs, adjectives, nouns, participles, verbs, personal pronouns—without any compensating habit of addition. He frequently omits phrases and clauses. He prefers the simple to the compound word. In short, he favors brevity. He shortens the text in at least fifty places in **singular readings alone**. But he does **not** drop syllables or letters. His shortened text is readable.³

Of special significance is the possibility of affirming with certainty that the scribe of P⁴⁵ deliberately and extensively shortened the text. Colwell credits him with having tried to produce a good copy. If by 'good' he means 'readable', fine, but if by 'good' we mean a faithful reproduction of the original, then P⁴⁵ is bad. Since P⁴⁵ contains many deliberate alterations it can only be called a "copy" with certain reservations.

P⁴⁶ is thought by some to be as early as P⁶⁶. Zuntz's study of this manuscript is well-known. "In spite of its neat appearance (it was written by a professional scribe and corrected—but very imperfectly—by an expert), P⁴⁶ is by no means a good manuscript. The scribe committed very many blunders My impression is that he was liable to fits of exhaustion."⁴

¹ Colwell, "Scribal Habits", pp. 374-76.

² *Ibid.*, p. 376.

³ *Ibid.*, p. 383.

⁴ Zuntz, *The Text*, p. 18.

It should be remarked in passing that Codex B is noted for its 'neat appearance' also, but it should not be assumed that therefore it must be a good copy. Zuntz says further: "P⁴⁶ abounds with scribal blunders, omissions, and also additions".¹

. . . the scribe who wrote the papyrus did his work very badly. Of his innumerable faults, only a fraction (less than one in ten) have been corrected and even that fraction—as often happens in manuscripts—grows smaller and smaller towards the end of the book. Whole pages have been left without any correction, however greatly they were in need of it.²

Hoskier, also, has discussed the "large number of omissions" which disfigure P⁴⁶.³ Again Zuntz says: "We have observed that, for example, the scribe of P⁴⁶ was careless and dull and produced a poor representation of an excellent tradition. Nor can we ascribe the basic excellence of this tradition to the manuscript from which P⁴⁶ was copied (we shall see that it, too, was faulty)."⁴

It is interesting to note that Zuntz feels able to declare the **parent** of P⁴⁶ to be faulty also. But, that P⁴⁶ represents an "excellent tradition" is a gratuitous assertion, based on Hort's theory. What is incontrovertible is that P⁴⁶ as it stands is a very poor copy—as Zuntz himself has emphatically stated.

Aland says concerning P⁴⁷: "We need not mention the fact that the oldest manuscript does not necessarily have the best text. P⁴⁷ is, for example, by far the oldest of the manuscripts containing the full or almost full text of the Apocalypse, but it is certainly not the best."⁵

Their quality judged between themselves

As to B and Aleph, we have already noted Hoskier's statement that these two MSS disagree over 3,000 times in the space of the four Gospels. Simple logic imposes the conclusion that one or the other must be wrong over 3,000 times—that is, they have over 3,000 mistakes between them. (If you were to write out the four Gospels by hand do you suppose you could manage to make 3,000 mistakes, or 1,500?) Aleph and B disagree, on the average, in almost every verse of the Gospels. Such a showing seriously undermines their credibility.

Burgon personally collated what in his day were 'the five old uncials' (א, A, B, C, D). Throughout his works he repeatedly calls attention to the *concordia discors*, the

¹ *Ibid.*, p. 212.

² *Ibid.*, p. 252.

³ H.C. Hoskier, "A Study of the Chester-Beatty Codex of the Pauline Epistles", *The Journal of Theological Studies*, XXXVIII (1937), 162.

⁴ Zuntz, *The Text*, p. 157.

⁵ Aland, "The Significance of the Papyri", p. 333.

prevailing confusion and disagreement, which the early uncials display between themselves. Luke 11:2-4 offers one example.

"The five Old Uncials" (Ⲱ ABCD) falsify the Lord's Prayer as given by St. Luke in no less than forty-five words. But so little do they agree among themselves, that they throw themselves into six different combinations in their departures from the Traditional Text; and yet they are never able to agree among themselves as to one single various reading: while only once are more than two of them observed to stand together, and their grand point of union is no less than an omission of the article. Such is their eccentric tendency, that in respect of thirty-two out of the whole forty-five words they bear in turn solitary evidence.¹

Mark 2:1-12 offers another example.

In the course of those 12 verses . . . there will be found to be 60 variations of reading. . . . Now, in the present instance, the 'five old uncials' **cannot be** the depositories of a tradition—whether Western or Eastern—because they render inconsistent testimony **in every verse**. It must further be admitted, (for this is really not a question of opinion, but a plain matter of fact,) that it is unreasonable to place confidence in such documents. What would be thought in a Court of Law of five witnesses, called up 47 times for examination, who should be observed to bear contradictory testimony **every time**?²

Hort, also, had occasion to notice an instance of this *concordia discors*. Commenting on the four places in Mark's Gospel (14:30, 68, 72a,b) where the cock's crowing is mentioned he said: "The confusion of attestation introduced by these several cross currents of change is so great that of the seven principal MSS Ⲱ A B C D L Δ no two have the same text in all four places".³ He might also have said that in these four places the seven uncials present themselves in **twelve** different combinations (and only A and Δ agree together three times out of the four). If we add W and Θ the confusion remains the same, except that now there are thirteen combinations. Are such witnesses worthy of credence?

Recalling Colwell's effort to reconstruct an "Alexandrian" archetype for chapter one of Mark, either Codex B is wrong 34 times in that one chapter or else a majority of the remaining primary "Alexandrian" witnesses is wrong (which does nasty things to the pretensions of the "Alexandrian" text), and so for Aleph and L, etc. Further, Kenyon admitted that B is "disfigured by many blunders in transcription".⁴ Scrivener said of B:

¹ Burgon, *The Traditional Text*, p. 84.

² Burgon, *The Revision Revised*, pp. 30-31.

³ Westcott and Hort, p. 243.

⁴ Kenyon, *Handbook*, p. 308.

One marked feature, characteristic of this copy, is the great number of its omissions. . . . That no small portion of these are mere oversights of the scribe seems evident from the circumstance that this same scribe has repeatedly written words and clauses **twice over**, a class of mistakes which Mai and the collators have seldom thought fit to notice, . . . but which by no means enhances our estimate of the care employed in copying this venerable record of primitive Christianity.¹

Even Hort conceded that the scribe of B "reached by no means a high standard of accuracy".² Aleph is acknowledged on every side to be worse than B in every way.

Codex D is in a class by itself. Said Scrivener:

The internal character of the Codex Bezae is a most difficult and indeed an almost inexhaustible theme. No known manuscript contains so many bold and extensive interpolations (six hundred, it is said, in the Acts alone). . . . Mr. Harris from curious internal evidence, such as the existence in the text of a vitiated rendering of a verse of Homer which bears signs of having been retranslated from a Latin translation, infers that the Greek has been made up from the Latin.³

Hort spoke of "the prodigious amount of error which D contains".⁴ Burgon concluded that D resembles a Targum more than a transcription.⁵

Their quality judged by the ancient Church

If these are our best MSS, we may as well agree with those who insist that the recovery of the original wording is impossible, and turn our minds to other pursuits. But the evidence indicates that the earliest MSS are the worst. It is clear that the Church in general did not propagate the sort of text found in the earliest MSS, which demonstrates that they were not held in high esteem in their day.

Consider the so-called "Western" text-type. In the Gospels it is represented by essentially one Greek MS, Codex Bezae (D, 05), plus the Latin versions (sort of). So much so that for many years no critical text has used a cover symbol for "Western". In fact, K. and B. Aland now refer to it simply as the "D" text (their designation is objective, at least). The Church universal simply refused to copy or otherwise propagate that type of text. Nor can the Latin Vulgate legitimately be claimed for the "Western" text—it is more "Byzantine" than anything else (recall that it was translated in the 4th century).

¹ Scrivener, *A Plain Introduction*, I, 120.

² Westcott and Hort, p. 233.

³ Scrivener, *A Plain Introduction*, I, 130. Cf. Rendel Harris, *A Study of the Codex Bezae* (1891).

⁴ Westcott and Hort, p. 149.

⁵ Burgon, *The Traditional Text*, pp. 185-90.

Consider the so-called "Alexandrian" text-type. In more recent times neither the UBS nor the Nestle texts use a cover symbol for this "text" either (only for the "Byzantine"). F. Wisse collated and analyzed 1,386 MSS for chapters 1, 10 and 20 of Luke.¹ On the basis of shared mosaics of readings he was able to group the MSS into families, 15 "major" groups and 22 lesser ones. One of the major ones he calls "Egyptian" ("Alexandrian")—it is made up of precisely four uncials and four cursives, plus another two of each that are "Egyptian" in one of the three chapters. Rounding up to ten, that makes ten out of 1,386—less than 1%!

Again, the Church universal simply refused to copy or otherwise propagate that type of text. Codex B has no 'children'. Codex Aleph has no 'children'—in fact, it is so bad that across the centuries something like 14 different people worked on it, trying to fix it up (but no one copied it). Recall Colwell's study wherein he tried to arrive at the archetype of the "Alexandrian" text in chapter one of Mark on the basis of the 13 MSS presumed to represent that type of text. They were so disparate that he discarded the seven "worst" ones and then tried his experiment using the remaining six. Even then the results were so bad—Codex B diverged from the mean text 34 times (just in one chapter)—that Colwell threw up his hands and declared that such an archetype never existed. If Colwell is correct then the "Alexandrian" text-type cannot represent the Autograph. The Autograph is the ultimate archetype, and it did indeed exist.

Consider one more detail. Zuntz says of the scribe of P⁴⁶: "Of his innumerable faults, only a fraction (less than one in ten) have been corrected and even that fraction—as often happens in manuscripts—grows smaller and smaller towards the end of the book. Whole pages have been left without any correction, however greatly they were in need of it."²

A similar thing happens in P⁶⁶. Why? Probably because the corrector lost heart, gave up. Perhaps he saw that the transcription was so hopelessly bad that no one would want to use it, even if he could patch it up. It should also be noted that although many collations and discussions of MSS ignore errors of spelling, to a person in the year 250 wishing to use a copy, for devotional study or whatever, errors in spelling would be just as annoying and distracting as more serious ones. A copy like P⁶⁶, with roughly two mistakes per verse, would be set aside in disgust.

I recently collated cursive 789 (Athens: National Library) for John, having already done so for Luke. Although the copyist made an occasional mistake, I judge that his exemplar was a very nearly perfect representative of Family 35. However, 789 is presently lacking John 19:12 to the end. A later hand, 789^s, has 19:26 to the end, but that copyist was a terrible speller, averaging nearly one mistake per verse—

¹ F. Wisse, *The Profile Method for Classifying and Evaluating Manuscript Evidence* (Grand Rapids: Eerdmans, 1982).

² Zuntz, *The Text*, p. 252.

reminiscent of P⁶⁶ (although P⁶⁶ is worse, averaging around two mistakes per verse). I found myself becoming angry with the copyist—I was prepared to call down curses on his head! Assuming that the cause of the mistakes was ignorance, rather than perversity, the copyist should not have undertaken a task for which he was so pitifully unqualified. It would be psychologically impossible for me to use 789^s for devotion or study. I would become too angry to continue.¹

Further, how could the early MSS survive for 1,500 years if they had been used? (I have worn out several Bibles in my short life.) Considering the relative difficulty of acquiring copies in those days (expensive, done by hand) any worthy copy would have been used until it wore out.

¹ I continue to insist that most of the early MSS survived because they were intolerably bad; it was psychologically impossible to use them, besides being a criminal waste of good parchment to copy them (is not uncial 06 the only one with an extant copy?).