stretches of Scripture without legitimate doubt, and, in the New Testament at least, may know precisely at what rarely occurring points, and to what not very great extent, doubts as to the genuineness of the text are still possible. 36

Loetscher had implied that it was Alexander—in contrast to Hodge and Warfield—who had played down the accuracy of the autographs. Yet in an 1831 article Alexander remarks that, "while it is evident, that contradictions merely apparent prove nothing against inspiration, it is equally certain, that real contradictions would furnish the strongest evidence against the inspiration of the words in which they were found." 27 Also, instead of blaming discrepancies on textual transmission, Alexander is represented by Loetscher as content in attributing them to the original autographs themselves. But here again Alexander was as ready as Warfield (if not indeed more ready) to blame these on textual transmission:

There are in the Bible apparent discrepancies which can easily be reconciled by a little explanation; and there may be real contradictions in our copies, which may be owing to the mistakes of transcribers. Now, when such things are observed, there should not be a hasty conclusion that the book was not written by inspiration, but a careful and candid examination of the passages, and even when we cannot reconcile them, we should consider the circumstances under which these books have been transmitted to us, and the almost absolute certainty, that in so many ages, and in the process of such numerous transcriptions, mistakes most necessarily have occurred, and may have passed into all the copies extant. 28

V. Conclusion

In his amanuensis illustration, Alexander did not avoid "asserting the literal 'inerrancy' of the original autographs of the New Testament." Quite to the contrary, the Old Princeton understanding of superintendence, shared by Alexander and his successors, was not in any way compromised by it, since the inspiration of superintendence was seen as involved only in so far as was necessary to assure the accuracy of the final product. In his illustration Alexander is simply emphasizing one way in which the autographs might be more problematic for us than a correct copy. The original scribe, into whose hands the manuscript of the autographs marked with the apostle's corrections was entrusted, would not have had to wonder, as Alexander would, whether some or other of the corrections were added by a later scribe, since he was himself the first scribe. Consequently his copy, or a descendant of it, with all the apostle's corrections taken into account, would be of more use to us than the marked-up autographic manuscript itself.

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36 Ibid., 2-5 (382-383).
38 Ibid. (itself added).
would like to make. WTJ readers believe, of proving that 7QS should be accepted as Markan. "Since [P⁶] has similar textual 'glisches. '" That is a kind of fallacy of analogy no papyrologist would commit. However, and this is vital, 7QS does not exist in a vacuum. It is—regardless of its contents—part of our Greek textual tradition. Thus, to understand its palaeographical and textual characteristics, we simply must compare it to other papyri. Comparison is the daily bread and butter of the papyrologist. And, needless to say, while a closer look at P⁶ (and indeed P³, unmentioned by Wallace) is demanded because of their status as early gospel manuscripts, I do also analyze the Masada papyrus of Viegil and the Menander fragment from Oxyrhynchus, and there even are photographs of these "secular" papyri in my book.

Second, I do not "spend an exorbitant amount of space demonstrating that 7QS should be dated no later than c. 50 ce. Why should if? A palaeographical date of c. 50 had been acceptable before, and in any case, the archaological date for all Qumran manuscripts—they must be prior to 408—is quite sufficient. In this context, Wallace asserts (note 6) "that a scribe's handwriting is not going to change very much over the duration of his career." This is plainly wrong, as he could have noticed by looking at a letter or paper he wrote at the beginning of his career, and comparing it to his present style. And I wonder why he thinks that I would be prepared to date Mark's gospel to 90 30 (note 7)? All I am saying, and this is pure logic accessible to all historians, is simply that the Gospel cannot have been written before 30, since the resurrection of Jesus, as the last event recorded in it, is the "terminus post quem." Third, even though Wallace puts this in inverted commas, I am not saying anywhere that "you don't have a right to criticize O'Callaghan's reconstruction because you haven't seen the fragment." This would indeed be a somewhat elitist stance. What I am saying is something different: no photograph, whether enlarged, infrared or whatever, can be a substitute for the original. I even give a couple of—indispensable—examples of published plates where the photographs have led astray. Thus, any statement made on the basis of photographs alone must be provisional. It may be correct, of course, but it remains provisional. I do not think any papyrologist would dispute this statement.

Fourth, Wallace assumes that "most damaging to O'Callaghan's identification is the tie in the place of deka." And he goes on to claim that there are no examples for deka- instead of deka- (as demanded by Q1Q's teseopimases for dakeopimases in Mark 6.55), merely for deka- / deka-. Again, this is wrong. There is a clear contemporary equivalent: the case of teque instead of toque in a text dated 90 42; and later, 90 132, even for tomos instead of tomeos.5


Fifth, Wallace plays the popular game of suggesting alternative identifications. Like Victoria Spottorno immediately before him, he fails because even undisputed aspects of 7QS do not match. Thus, in line 2 of his attempt with Philo, De plantatione 135, he must assume a y preceding the spasmus, whereas there is an y. Above all, there is no room for the unmistakable spasmus. And he wisely omits lines 1 and 5 of 7QS because nothing on them would fit Philo.

Sixth, I do not see why Wallace is so eager to refute other NT identifications in Cave 7, against a nonexistent opponent. Not even O'Callaghan claimed that other fragments are "certain"—they are too small for that. The exception, of course, is 7QS, identified as 1 Tim 3.16-3. But again, who says that 7QS should be dated "no later than 50 ce."? I certainly do not. Palaeographically, it could be c. 90-50, but as with 7QS, the archaological terminus ante quem, ce 68, is quite sufficient. And a pastoral letter, even if not by Paul himself—who was executed in ce 64 perhaps, or in ce 67 at the latest—could easily fit into this time scale. By the way, I wonder why Wallace thinks that an early date is "impossible," even in italics, for any pastoral epistle. Has he read, among others, Gordon D. Fee's commentary, written, incidentally, by a fellow skeptic as regards 7QS?6

Seventh, who says that there was essentially "a friendly familiarity" between the Essenes and the Christians for the former to "hide" the Christian texts in Cave 7? Such a scenario, in any case, is only one of several possibilities. We have to respect the fact that the first Christians continued to see themselves as Jews. Thus, the first Christian writings would still reflect an essentially Jewish frame of mind, telling the story of the Messiahship of the Jews, as foretold by the Jewish Tanach. Any such texts would have been a legitimate addition to the Qumran library. That the Essenes collected Greek language material from outside is an acknowledged fact.7

Finally, a papyrologist's plea: Can we stop wheeling on the so-called argument of the smallness of 7QS again and again? Even Wallace cannot resist this temptation (p. 179). Equally small or smaller fragments with—occasionally—much more severe textual problems have been identified without anyone raising as much as an eyebrow. The Oxyrhynchos Menander which I describe in my book is one obvious example, and 7Q2 from the "Markan" case, identified as Letter of Jeremiah 6.43-44, is another. In fact, the textual problems caused by 7Q2 are considerably more difficult.


than those in TQ5. Should one suspect the unpleasant spectre of double standards? Daniel B. Wallace asks for a correct attitude and an end to party lines. Here, we do set eye to eye.

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REVIEWS OF BOOKS


Watson’s book develops a synchronic, canonical, and theological approach to interpreting the Bible. Historical-critical study of the Bible does not provide satisfaction, and in fact is reaching a kind of dead end: it just multiplies possibilities without ever coming to rest (pp. 59-55, 58, 225). Hence, we need specifically theological interpretation. The book is intelligent, creative, and sensitive to nuance. As such it represents something of the “true of the art” in modern theology, at the same time that it pushes the boundaries forward.

The book interacts creatively with a host of key ideas in hermeneutics. It takes its start from Haas Frei and Brevard Childs, both of whom express principal dissatisfaction with the cul-de-sac into which the historical-critical method has led. But the book rightly complains that both Frei and Childs fail to provide a fully satisfactory alternative. For Frei the text remains a literally self-contained object that interests with neither church nor world (pp. 19-29). Childs through his canonical emphasis begins to set the text in the context of the church, but the church is still conceived ideally. Canonical interpretation is pictured unproblematically, as if it were not subject to both internal and external conflicts (pp. 44-45). Watson’s book wants to engender a kind of canonical interpretation that takes seriously the canon as a literary unity, but that is also not afraid on this basis to engage a full spectrum of modern issues, political, economic, social, and sexual.

Watson then goes on to interact further not only with the historical-critical tradition but with various strands of deconstruction, postmodern views of meaning and truth, and feminist interpretation. He shows that he can use methodological tools from these traditions, but in the end he finds deficiencies as well. Using Jürgen Habermas and Alison McFadyen as a starting point, he develops a view of human beings as communicators, not merely as carriers of a language system (pp. 107-25).

Genesis 1 can then be understood in the light of this communicative capacity. Postmodernism, in viewing truth claims as merely relative to particular communities, is at odds with the universal claims of Christian truth underlined in Genesis 1. God, not human beings, created a world through language. God’s language, and the things made by him, form a stable reference point countering the relativism of postmodernist approaches (p. 151).

The book then wrestles with the feminist claim that the Bible is oppressive. Unaware of D. J. A. Clines’s argumentation (in What Does Sex Do? To Help? and Other Rude Questions to the Old Testament [Sheffield: JSOT, 1990] 25-48), it attempts to understand Genesis 1-2 as an egalitarian prologue to a fallen patriarchal situation (pp. 188-89), and to show how the Sun’s revelation of the Father in the Gospels criticizes and surmounts patriararchism (pp. 202-10).

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