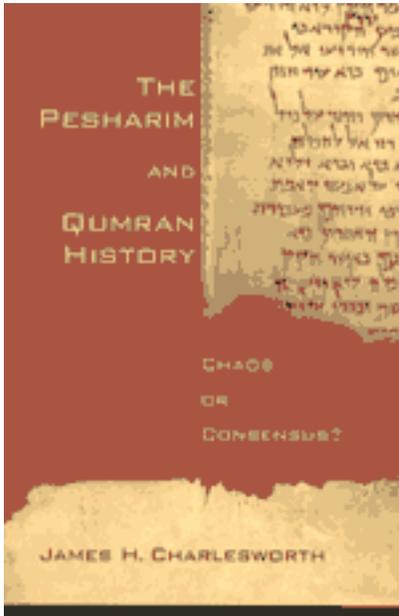


RBL 02/2004



Charlesworth, James H.

The Pesharim and Qumran History: Chaos or Consensus?

Grand Rapids: Eerdmans, 2002. Pp. 171. Paper. \$28.00.
ISBN 0802839886.

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James Charlesworth has a distinguished career of publication of primary sources, most notably and recently the ongoing series of Qumran text publications of the Princeton Theological Seminary Dead Sea Scrolls Project. Volume 6B of this series, published in 2002, is a new edition of the pesharim. Following this publication Charlesworth has produced *The Pesharim and Qumran History*.

This book examines the pesharim “to see if historical episodes are reflected in them, and also to see if it is possible to discern a consensus regarding the most likely reconstruction of Qumran history in light of the critical edition of the pesharim” (17). The book consists of two parts or chapters, not numbered, with twelve sections, also not numbered. In the first part, entitled “The Hermeneutics of the Pesharim,” Charlesworth sets forth issues and problems and presents a historical reconstruction as background to the production of the pesharim. The second part, entitled “The Pesharim and Qumran History: Chaos or Consensus?” addresses the problem of finding history in the pesharim. Here Charlesworth discusses Qumran archaeology, methodological issues in relating texts to history, and specific allusions in the texts. Charlesworth suggests six principles for discerning history in the pesharim (73–76): (1) multiple attestation within Qumran literature; (2) multiple attestation from non-Qumran literature in early Judaism; (3)

coherence within Qumran sectarian literature; (4) coherence with non-Qumran literature; (5) coherence with archaeological insights obtained from excavations at Qumran; and (6) historical allusions not generated by Qumran theological tendencies. The book closes with appendices by Lidija Novakovic: an “Index of Biblical Quotations in the Pesharim, Other Commentaries, and Related Documents” and “Text-Critical Variants in the Pesharim, Other Commentaries, and Related Documents.”

As a central part of his discussion Charlesworth sets forth a historical context for the pesharim, with particular focus on the Teacher of Righteousness, or, as rendered by Charlesworth, the “Righteous Teacher.” As Charlesworth presents it, in the mid-second century B.C.E. the “Righteous Teacher probably had officiated in the Temple cult, and may have served there as high priest” (88). The Teacher led a movement that “was eventually exiled (or exiled itself) from the Temple and resided at Qumran ‘in the wilderness’ ” (36). “There is no evidence to cause us to think his initial group was large; perhaps it numbered only 20 to 50 Zadokite priests and Levites” (37). “[T]he first Hellenistic buildings [of Qumran Period Ia] should be identified as the first work at Qumran by the Righteous Teacher and his followers” (98 n. 293). “No more than 150, and probably less than 100, ever worked and lived near Qumran at any one time” (66). The several dozen inhabitants of Qumran developed their own distinctive linguistic forms: “The language of the Qumranites was distinctly and uniquely developed” (12). The Teacher “probably created the peshar method and taught it to his disciples” (12). It is “likely that the origins of the Qumran Community are to be found ‘within the Essene movement’ which antedates Qumran” (56). “[T]here should be no doubt that Qumran was the conservative branch of the Essenes” (20 n. 11; also 54).

Charlesworth acknowledges that “Jodi Magness contends that Qumran was first inhabited in the early portions of the 1st century B.C.E. . . . her position makes sense if one looks only at the archaeological evidence” (37 n. 97). However, according to Charlesworth, “[a]rchaeology and the study of evolution of 1QS indicate that the first phase of Qumran probably runs from ca. 150 B.C.E. to ca. 102 or 100 B.C.E.” (42). Charlesworth does not explain how a study of 1QS indicates that Qumran buildings were built before 100 B.C.E.

After noting that J.-B. Humbert and J. Magness have argued independently that Qumran was first built in the first century B.C.E. (42–44), Charlesworth comments: “It is obvious why these criticisms [of de Vaux’s second-century B.C.E. start date] appear. . . . Those who want the Wicked Priest and Righteous Teacher to be someone later in history [than the second century B.C.E.] obviously are biased against the possibility of Period Ia” (44). Because Humbert and Magness are the only critics of de Vaux’s dating named by Charlesworth, and because Charlesworth does not clarify otherwise, the reader is given the impression that Charlesworth is saying that the archaeological arguments of Humbert

and Magness were influenced or motivated by theories concerning a first-century B.C.E. Wicked Priest or Teacher of Righteousness. However, that is both undocumented in either of these scholars' published work and is certainly baseless. Charlesworth also appears to believe that, if there was a Period Ia, then a pre-100 B.C.E. habitation at Qumran is more likely than not (37 n. 97), although there is nothing archaeologically that requires such a dating of Ia. (Humbert dates Period Ia entirely after 100 B.C.E. Magness, in her most current statement in *The Archaeology of Qumran and the Dead Sea Scrolls* [2002], while questioning that de Vaux's Period Ia exists, leaves open the possibility that it could exist, but if it does, "the currently available evidence suggests that it should be dated to the early 1st century B.C.E." [Magness, 64–65].)

Charlesworth argues that the end of Qumran Period Ib is to be dated to 40 B.C.E. Whereas de Vaux dated the end of Ib at 31 B.C.E. (and Magness and R. Bar-Nathan later still), Charlesworth thinks it "more likely that the Qumranites left the site during the devastating invasion by the Parthians in 40 B.C.E. Since de Vaux's work the additional evidence of Parthian destruction, especially on the Western shores of the Dead Sea from Qumran to Ein Gedi, has been impressive" (50). A footnote appended to this intriguing last sentence refers the reader to "excavation reports published by Yizhar Hirschfeld." Based on an analysis of internal references in the pesharim, Charlesworth concludes: "No sure dates before 103 or after 55 [B.C.E.] are preserved in the pesharim. . . . All the pesharim were composed at Qumran during Phase II (Archaeological Period Ib) or ca. 100 to 40 B.C.E., with the possibility that one or two pesharim were composed [earlier]" (117–18; also 49). "[M]ost [of the pesharim] were written late in the life of the Community" (18). As for the resettling of Qumran after the pesharim texts ended in Period Ib: "we do not know what percentage of those who fled Qumran ca. 40 [B.C.E.], or later, returned to Qumran and lived there until the destruction of 68 C.E." (65).

On page 49 Charlesworth notes a new radiocarbon date that he obtained on a Qumran text named XJoshua. This radiocarbon date was first reported in J.H. Charlesworth et al., *Miscellaneous Texts from the Judaean Desert* (DJD 38; Oxford: Clarendon, 2000). In that publication Charlesworth, who edited the text, described XJoshua paleographically as equivalent to the latest-known texts at Qumran: "in almost every respect the handwriting is similar to either 4QDeut^l (c. 50 CE) or 4QPs^b (c. 50 to 68 CE) . . . an example of the late Herodian formal bookhand (40 BCE [*sic*]-68 CE, most likely near the end of the range)" (DJD 38, 232–34).

In the present book Charlesworth writes: "my dating of the Joshua fragment to the turn of the era was later confirmed [in 1999] by Professors Douglas J. Donahue and A. J. T. Jull of the NSF-Arizona AMS Facility (University of Arizona; the same laboratory as used by Emanuel Tov et al.). They reported that the probability distribution of 2 ranges at 95

percent accuracy, from '118 B.C. to 73 AD' " (49 n. 128). This radiocarbon date obtained by Charlesworth is the first radiocarbon date published for a Qumran text identified as written in so-called "late Herodian formal," the typologically latest scribal hand among the Qumran texts. Charlesworth's suggestion that a Qumran text written in "late Herodian formal" is from "the turn of the era" may be of interest in light of questions that have been raised concerning whether the palaeographic dating system currently in use is correct in dating some Qumran texts later than Qumran's Period Ib.

Charlesworth addresses many interesting topics and details, of which only a few can receive comment here. For example, Charlesworth claims, "After fifty years it is now certain that the pesharim were composed at Qumran" (6). This claim that all of the pesharim were authored at the physical site of Qumran is repeatedly asserted (2, 57, 77, 78, 106). However, as Charlesworth correctly recognizes elsewhere, at least some of the pesharim show evidence of a scribal transmission history (78–80), yet none of the earlier stages of scribal transmission of the pesharim, or indeed of any other text, have been found at Qumran—not a single author's draft with errors and corrections prior to a later copy of the same text has been identified among the tens of thousands of fragments in the caves at Qumran. How did the earlier authors' drafts and compositions disappear without a trace, if authorial activity was happening at Qumran? Until this question, so frequently raised by Norman Golb, receives some reasonable answer, this reviewer suggests more modest language is called for than that the claim is "certain."

On page 65 Charlesworth states that the Qumran text collection "surprisingly contains works somewhat anathema to Qumran theology." This claim is supported by reference to only a single text: 4Q448, the Prayer of Jonathan. Charlesworth calls this "the document most antithetical to Qumran theology" (but no other texts are named). But how, exactly, is 4Q448 opposed to "Qumran theology"? Charlesworth gives only this in explanation: "This document [4Q448] refers favorably to [Alexander] Jannaeus; hence it . . . is not a Qumran composition" (104–5). However, Charlesworth's reasoning is puzzling. One page earlier Charlesworth writes, "The Jews, especially those living in Jerusalem, and probably some Qumranites would have been pleased with Jannaeus—at this early period in his kingship" (103). But if some Qumranites were pleased with Jannaeus at the time 4Q448 was composed, how then is Charlesworth certain that the text could not have been composed by one of those Qumranites pleased with Jannaeus?

In a few places other scholars have not been represented carefully. For example, on page 117, "The [Seekers-after-Smooth-Things group] is synonymous with 'Ephraim' and along with it most likely denotes the early Pharisees." A footnote says, "This judgment is well received . . . by Saldarini," but a check of the pages cited for Saldarini shows that Saldarini did not say this but instead the opposite (Saldarini: "the Qumran polemics

against their opponents [including the epithet ‘seekers after smooth things’] testify to the diversity and conflicts in Jewish society but not that their opponents were Pharisees” [A. J. Saldarini, *Pharisees, Scribes and Sadducees in Palestinian Society* [Edinburgh: T&T Clark, 1988], 280).

This reviewer was also surprised to read on page 49, note 128, an erroneous description of his own “single-generation hypothesis” proposed in 1998 (G. Doudna, “Dating the Scrolls on the Basis of Radiocarbon Analysis,” in *The Dead Sea Scrolls after Fifty Years* [ed. Flint and VanderKam; Leiden: Brill, 1998], 1:430–71 at 463–64). Charlesworth writes: “Doudna dates the Qumran scrolls before the 1st century C.E. There are many reasons to doubt his conclusion that all the Qumran scrolls were composed within one generation.” The first sentence is accurate, but the second is not. Charlesworth consistently uses the word “compose” to mean authorship, as distinguished from scribal copying (40, 49, 80, 85, 106, 118). I never said that all texts found at Qumran—including the biblical texts, the pseudepigraphic texts, and so forth—were composed within one generation! (My “single-generation hypothesis” of 1998 dealt with dates of scribal copies—not dates of authorship—and it did not propose that all scrolls found in the Qumran caves were copied within one generation either. The proposal was that most of the scribal copies found in the caves at Qumran were produced in the latest generation of scribal copying represented in the finds in the caves—with “generation” understood loosely, not rigorously, and situated in the first century B.C.E. That is: if the true dates of scribal copies were plotted on a graph, the suggestion is the line might look like a large bubble at the late end [first century B.C.E.] with a tail going back in one direction perhaps a century or so earlier [second century B.C.E.]. This is as opposed to the conventional view, which has—so to speak—a wavy horizontal line over a three-century period [second century B.C.E. to first century C.E.]. “Generation” should be understood loosely and not narrowly [e.g., perhaps a fifty- or seventy-year “generation”]; the hypothesis has always allowed for earlier text copies, and the point is the shape of the graph.)

Finally, some of the text editions of the pesharim of volume 6B relied upon by Charlesworth for analysis are not always reliable. Following are twelve errors I noted in four passages from 4QpNah, cited and discussed by Charlesworth on pages 99–107. (1) At page 99 the restoration “th[at (are) in Jerusale]m” of 3–4 i 10 is in error; the visible ink strokes following the lacuna cannot be restored as the letter *mēm*, from which the rest of the word “Jerusalem” was then mistakenly reconstructed. (2) At page 99 it is not plausible that *yq]r*⁷ of 3–4 i 8 can be rendered “[it] reads.” First, there is no other case in a Qumran text of a form of *qr*⁷ used as an introductory formula for a quotation. Second, 4QpNah nowhere else uses any introductory formula to introduce quotations. Third, *qr*⁷ as used in biblical Hebrew and Qumran Hebrew connotes oral reading or other orality of persons. It is an English idiom that “a book says,” “a book reads,” “it says,” “it reads,”

and the like followed by a quotation from a literary source, but that is not an idiom in biblical Hebrew or Qumran Hebrew. (3) At page 99 there is no textually sound justification for rendering *ytlh* of 3–4 i 7 as iterative past tense, “he would hang men up alive” instead of expected “will hang” or “hangs.” The rendering is tendentious; no other imperfect verb from 4QpNah or any other pesharim passage cited by Charlesworth is rendered “would <verb>”. The reason *ytlh* of 3–4 i 7 was rendered this way is because of influence from an external historical construction. Then the translation is used in support of the historical construction. (4) At page 106, “when the glory of Judah is [re]vealed” of 3–4 iii 4 is not the correct rendering of *wbh[g]lwt kbwd yhw dh*; the sense is, “when the glory of Judah will be taken away” (compare Isa 16:14; 17:4; 21:16; Hos 9:11). (5) At page 106, “will join [I]srael” of 3–4 iii 5 is incorrect; the correct restoration is “will join the G[od of I]srael.” (6) At page 107, the restoration “the wicked one[s of Manass]eh” of 3–4 iv 1 is incorrect; the spacing of the lacuna will not permit restoration of that many letters in the lacuna. (7) At page 113, “Where the lion went to enter” of 3–4 i 1 reflects an underlying erroneous reading **lbw*’, where the correct reading is *lby*’. (8) At page 113 there is no *o* (*wāw*) or trace of ink from any other letter following a lacuna at the end of 3–4 i 1. (9) At page 113 “the lion’s cub,” while a possible reading of *šm gwr ’ry* in the quotation from Nah 2:12b at 3–4 i 1, was not read that way by the ancient author of Peshar Nahum. The word *gwr* was read by the ancient author not as “cub” but as an infinitive construct of the verb “sojourn, dwell”; that is, “there was the dwelling of the strong lion.” (10) At page 113 the restoration of the introductory formula in the lacuna at 3–4 i 2, “Its interpretation concerns Deme]trius,” is counterindicated on grounds of spacing and almost certainly wrong. The correct restoration appears to be: “Its interpretation: ‘the lion’—this is Deme]trius.” (11) At page 113 *bqš lbw’ yrwšlym* of 3–4 i 2, rendered “sought to enter Jerusalem,” should more accurately be rendered “sought to come to Jerusalem” (i.e., sought to head directionally toward Jerusalem from someplace else, e.g. 2 Kgs 18:17). (12) At page 113 “into the hand of Greece” at 3–4 i 3 should be “into the hand of the kings of Greece.”

Of these twelve, six (1, 2, 3, 4, 5, and 6) are relied upon by Charlesworth for arguments. That is, errors at the level of the database (volume 6B) have entered into and contaminated the exegetical argument presented by Charlesworth. A clean and error-free database is needed as a first order of business.

Conclusion

Charlesworth brings out much interesting information and bibliography in his discussions and footnotes. Nevertheless, this reviewer found this book marred by unsupported assertions and citation of scholarly consensus in lieu of argument from primary data. Despite the criticisms of this review, this reviewer wishes to thank Charlesworth for his

many publications of primary texts and his obtaining of scientific data, such as radiocarbon datings, and to acknowledge that the issues raised and addressed by Charlesworth in the present work are important and germane.