

**David Stacey and Gregory Doudna, with a contribution by Gideon Avni**

*Qumran Revisited: A Reassessment of the Archaeology of the Site and its Texts.* BAR S2520. Oxford: Archaeopress, 2013. Paperback. Pp. 150; 21 figures; 11 plans. £29.00. ISBN 978-1-4073-1138-8.

This book is essentially an anthology comprising three essays which have been authored independently of each other: David Stacey updates his previously published work to provide a re-assessment of the archaeology of Qumran (7–74); Gregory Doudna re-examines and re-writes the history underlying Dead Sea Scrolls (75–124); Gideon Avni presents some observations regarding the cemeteries adjacent to the Qumran settlement (125–36). Stacey and Doudna's long and detailed essays, in particular, present new and challenging ideas, and thus, here I will largely focus on their contributions. Nonetheless, in the space of this review, I can only engage selectively with their ideas.

Stacey presents a very thorough analysis of Qumran's stratigraphy, on the basis of which he argues that the site was fully developed only in the Herodian period; Stacey maintains that substantial parts of the so-called main building and the main aqueduct were built after the earthquake of 31 BCE. This is a radical departure from previous reconstructions of Qumran. Stacey's interpretation of the site is equally radical: he argues that during the Hasmonean period Qumran served as a seasonal industrial settlement connected to the royal estates in Jericho, whereas in the course of the Herodian period, it became the permanent residence of "a small staff of quartermasters," whose task was "to supervise the distribution of the royal stores" to other palatial fortresses in the region, such as Hyrcania, Machaerus, and Herodium (66–69). Following the death of Herod, Stacey sees Qumran's importance waning and the site reverting back to a seasonal settlement. A basic premise that underlies Stacey's hypothesis is the lack of an intrinsic link between the scrolls and the inhabitants of Qumran.

Though Stacey makes some good observations on the site's stratigraphy, his overall reconstruction of Qumran is very problematic. A major flaw is the fact that his hypothesis rests on several questionable assumptions. For example, a substantial part of Stacey's case for the post-31 BCE date of the main aqueduct (and by extension related architectural features, such as cisterns of L.56–58 and pools of L.55–57) rests on the dating of the damage and repair of the northern wall of L.77. Stacey states that "this localised damage was most likely caused by the earthquake of 31 BCE" and that, therefore, because the cisterns of L.56–58 and pools of L.55–57, "which were integral with the earliest stage of the 'main' aqueduct," were built against the repaired northern wall of L.77, the main aqueduct must post-date 31 BCE (18). This pivotal argument is built on

the assumption that this wall was *indeed* damaged by the earthquake of 31 BCE, when there is no tangible evidence that supports this conclusion. Not every destruction event has to be associated with this earthquake, especially not in the Dead Sea zone, which experiences significant seismic activity. In theory, the northern wall of L.77 could have been damaged decades before 31 BCE. In fact, its repair might well relate to the re-occupation of Qumran at the beginning of the first century BCE, since de Vaux rightly noted that this wall already existed in the Iron Age phase of the site (*Archaeology and the Dead Sea Scrolls* [Oxford: Oxford University Press, 1973], 2).

At times Stacey uses highly ambiguous evidence. For instance, he points out that the outlet channel from the stepped cistern at L.117—which is contemporaneous with the raising of the walls of L.117, which is in turn associated with the building of the main aqueduct—lays directly on top of part of the northern dump (de Vaux's Trench A), which contained material dating to the late first century BCE. Consequently, Stacey concludes that the outlet channel must post-date the northern dump and that, therefore, this proves the late first century BCE date of the main aqueduct (14–15). However, it is difficult to accept Stacey's conclusions on the basis of just an outlet channel. It is quite possible, for example, that the channel was extended, or that parts of it were dismantled and rebuilt at a later time and that it came to overlie Trench A only then. Moreover, Stacey's argument is here based on the mere remains of an outlet channel that covered just a minute part of the dump and not on the remains of a large structure that covered the dump in its entirety. Therefore, one must ask: was the late first century BCE material found directly underneath the outlet channel or on one of its sides? We do not know. And the latter instance could reflect a scenario where further dumping would have taken place *after* the channel had been built.

Stacey makes a number of questionable claims to support his contentions that the main aqueduct could not have been freestanding, that it must have been associated from the very beginning with the Period II (and not Ib) floors, and that, therefore, it must post-date 31 BCE. Jodi Magness has already written a detailed reply to Stacey on this issue ("A Response to D. Stacey, 'Some Archaeological Observations on the Aqueducts of Qumran,'" *DSD* 14 [2007]: 244–53); here I add, as another example, Stacey's claim that "there indubitably was . . . an upper floor [i.e., a floor flush with the top of the main aqueduct in L.115 and L.116] from which the 'silos' in L.115 and L.116, which de Vaux recognised as being 'late', were dug," despite de Vaux's lack of reference to such an upper floor (12; note in square brackets is mine). The absence of an upper floor (flush with the top of the aqueduct) in these two loci contradicts Stacey's

contention that the aqueduct could not have been freestanding. Therefore, Stacey has to invent the existence of an upper floor on the basis of his assertion that silos could never have been freestanding; however, this is contradicted by such evidence elsewhere.

In other instances, Stacey misreads de Vaux. For example, one of Stacey's arguments for the late dating of the main aqueduct is anchored around the description of the north wall of L.130 as "not so thick and seemingly without a foundation" (14), but de Vaux actually made this description in connection with the northern wall of L.135! (See J.-B. Humbert and A. Chambon, eds., *Fouilles de Khirbet Qumrân et de Aïn Feshkha I* [NTOA.SA 1; Fribourg/Göttingen: Fribourg University Press/Vandenhoeck & Ruprecht, 1994], 336.)

Several interpretations concerning the site are especially dubious. Essentially, Stacey replaces a well-established hypothesis that has the merit of providing the best explanation of the extant data with one which is completely unfounded. While Stacey is certainly correct in asserting that the population of Qumran might have experienced seasonal fluxes, his claim that Hasmonean Qumran lay uninhabited for most of the year is utterly unconvincing. The assumption that only Herod would have had the resources and the expertise for a project like the water system at Qumran is debatable, considering that attempts to build water storage systems in the region go back hundreds of years (15–18). Stacey's assertion that Qumran's importance waned during the first century CE flies in the face of evidence; in addition to glass and stone vessels as well as several first century CE coins, there is also a large amount of unpublished first century CE pottery, which I was able to access at the ÉBAF—all this material is indicative of the site's continued importance during the first century CE. The problem is that Stacey focuses solely on architecture and fails to consider other evidence from Qumran. Most significantly, Stacey ignores the interpretative significance of the scrolls, which as *physical artefacts* that were actually found at Qumran cannot be simply brushed aside.

Indeed, on this matter, Stacey is not consistent. His essay contains a good discussion on possible industries at Qumran, but he chooses to emphasize the processing of wool and leather, which would have been "smelly, ritually polluting... processes" (72), the practice of which would conflict with a sectarian presence at Qumran. However, since Stacey could find no evidence at all for such industries in the buildings at Qumran, he turns to the caves in an attempt to adduce this evidence—namely skins and various pieces of leather from caves 8Q, X35, and X42 (54–55). The presence of skins and leather in these caves in no way signifies their local production; but more importantly, here Stacey is admitting evidence from the same caves he is eager to disassociate

from the Qumran settlement! If we can associate these pieces of leather with the settlement, why cannot we also associate the scrolls with the site's inhabitants?

The aforementioned examples are symptomatic of Stacey's essay in general. They are also sufficient to underscore that any reconstruction/interpretation that aims to replace a well-established hypothesis (and one which best explains the evidence currently at our disposal) needs to be founded on solid, unambiguous evidence. And this is not the case here.

Like Stacey's essay, Doudna's questions long-held viewpoints, in this case about the history behind and of the scrolls. Doudna argues that in the scrolls there is actually no polemic against the Hasmoneans, the Romans, or even the temple, and concludes that the scrolls represent a community of priests very much aligned with the Hasmoneans. Indeed, Doudna identifies Hyrcanus II as the Teacher of Righteousness, whereas the Wicked Priest is Antigonus Mattathias. He also underscores that the history of the scrolls should be located firmly in the first century BCE, at the end of which the scrolls were deposited in the caves of Qumran. Doudna's hypothesis is thought-provoking and, undoubtedly, it will generate much debate. Here, I will limit myself to some brief remarks.

Doudna's identification of the Teacher and the Wicked Priest is compelling within the wider narrative that he creates, but it runs into the same problems of earlier such attempts: basically, it still entails mining extant sources in an attempt to correlate one with the other. Doudna concludes that his is "a picture which dismantles the romanticized notion of a parallel history of a Qumran sect, running alongside known history, constructed solely from the texts themselves, invisible to external history" (124), but this assumes that our extant sources tell us everything that we need to know about the late Second Temple period and that the world of the scrolls *must* be somehow reflected in these sources. In the end, Doudna's reading might be plausible but not decisive; it also fails to engage with the literary function of sobriquets and their dynamic nature.

Some of the conclusions are also debatable. For example, does the lack of temple criticism in IQS mean that the *Yahad* was therefore the "ruling sect in Jerusalem" (75–76)? Polemic does not need to be explicit, especially if it is far removed, historically, from the origins of the conflict. Doudna also disassociates the sobriquet "Wicked Priest" from the earlier Hasmonean priestly-kings, thereby strengthening his claim that the scrolls are not ambivalent to the Hasmoneans (Mattathias Antigonus being the only exception). Quite correctly, he remarks that the sobriquets are probably contemporaneous to the time of

writing, and he states that it is “odd that the great majority of scholars today can simultaneously fully acknowledge that the Kittim of Peshar Habakkuk and Peshar Nahum are contemporary 1st century BCE Romans . . . yet hold that these texts nevertheless depict figures set in the mid-2nd century BCE” (81–83); but this ignores the possibility that the pesharim might contain a mixture of earlier and later/contemporary traditions. The argument that 1QM is not an anti-Roman text—since the Kittim in 1QM might actually refer to the Seleucids (104–5)—negates reader agency in the creation of meaning and the possibility that 1QM was re-read in light of new historical realities. And possible anti-Samaritan polemic in the scrolls does not necessarily make the scrolls pro-Hasmonean (101); certainly, opposing groups can share similar viewpoints on some aspects.

Doudna makes several astute observations about the methodological pitfalls behind the paleographic dating of the scrolls and about radiocarbon dating (108–17), but his conclusion is fraught by his equation of the date of composition with the date of deposition. Should we accept Doudna’s conclusion that no scrolls were written/copied during the first century CE, the fact remains that texts probably remained in circulation for several decades, well into the first century CE. Something might indeed have happened (within the movement behind the scrolls) close to the end of the first century BCE—indicated also by the lack of identifiable allusions to first century CE historical figures/events—but this can be explained in various ways; Doudna’s explanation of this phenomenon is not necessarily the most obvious solution.

A final remark about the book as a whole: since three essays have been collected in one volume, with the shared intention to revisit the scrolls and the archaeology of Qumran, one would have expected more coherence. Gideon Avni’s paper fits well with Stacey’s hypothesis, but Doudna’s does not. While Stacey clearly believes that Qumran was, respectively, a seasonal settlement and a Herodian outpost in the Hasmonean and Herodian periods, and that it was in no way related to the scrolls, Doudna presupposes a sect that used the site and the caves—“at some point the lights went out at Qumran for the people associated with the text deposits” he declares in connection with the end of the first century BCE (117).

While this volume contains a number of good insights, it would have benefited from more methodological awareness, especially considering the radical reassessment it proposes vis-à-vis both the archaeology of Qumran and the Dead Sea Scrolls. Stacey’s section, in particular, lacks a more open engagement with other scholars’ ideas, which Stacey is too ready to dismiss in favor of less plausible interpretations. I remain unconvinced by the main hypotheses

presented herein, but the essays do challenge us to go back and revisit the primary evidence. And this—even if ultimately we still disagree with such newly proposed readings—is an important part of the scholarly process.

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