Deconstructing What We've Always Been Told About Qumran

It is misleading to speak of a single "main period of habitation" of a single group or community at Qumran which ended at the time of the First Revolt. Analyses of pottery, language, women, dining, animal bone deposits, and scroll deposits surprisingly converge in suggesting a different picture: the true "main period" of activity at Qumran was mid- and late-first century BCE.

[The following is excerpted from Gregory L. Doudna, "Deconstructing the Continuity of Qumran IB and II with Implications for Stabilizing the Biblical Texts", in I. Hjelm and T.L. Thompson, eds., *Interpretation Beyond Historicity. Changing Perspectives 7*, ed. I. Hjelm and T.L. Thompson (New York: Routledge, 2016), 130-154. See full article for bibliography.]

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June 2017

From the introduction (pp. 130-31)

While, on one level, dates for the stabilization of biblical texts and of deposits of scrolls in caves are issues regarding the assessment of evidence—analyses of archaeology, palaeography, radiocarbon datings, and so on—on another level, there has been a strong continuing influence on scholarly thinking of an underlying story which filters perception of external data. This story is of a single community which occupied Qumran from the earlier part of the first century BCE until 68 CE, when Qumran was destroyed by fire by Romans. External evidence or data is made to fit within

this story and, in somewhat circular form, is perceived to reinforce or prove the correctness of the story. As Jean-Baptiste Humbert so well put it: "The narrative takes over and imposes its authority upon the process of archaeological interpretation" (2003b: 426).

The story is that which is alluded to ubiquitously in scholarly language of a "Qumran community", Qumran's "main period of habitation ending 68 CE", Qumran's "period of sectarian habitation", and the like. These expressions are shorthand for a larger story. Scholars know this story and what these terms imply and assume: they refer to the underlying story. This chapter examines several lines of argument relevant to the notion that there was a single community at Qumran through both of what Qumran excavator Roland de Vaux called Period "Ib" (ca. early first to late first century BCE according to current understanding) and "Period II" (?-68 CE). This chapter will argue against the notion that the same community or people continued in both of these two periods. Indeed, there was a "main period" of Qumran's activity and people associated with the site in that period placed the deposits of scrolls in caves near the site, but that "main period" was Period Ib and did not include activity at Qumran after that—neither in Period II, III, or at the time of Bar Kokhba.

The significance of this argument is that it removes the main obstacle to a scholarly realization that stabilization of the Hebrew biblical text happened before, not after, the destruction of the Temple of Jerusalem of 70 CE, otherwise suggested by the character of first-century CE biblical texts before 70 CE found at Judean Desert sites other than Qumran. All biblical texts found at the other Judean Desert sites are identical to MT, whereas none of the Qumran biblical texts are. This description has been established in a large number of studies by many scholars, notably Emanuel Tov (2012), and recently confirmed anew by tables of data with counts of variants of Qumran and Judean Desert biblical texts compared to the medieval *Leningrad Codex* compiled by Ian Young (2002; 2005; 2013).

Getting the date right for the Qumran scroll deposits removes the obstacle to seeing that the transition from the pluralism and variety reflected in the Qumran texts to the uniform, post-stabilization, carefully copied, exact-MT biblical texts found in all cases at all other Judean Desert sites is best dated ca. late first century BCE or early first century CE. In this light, the later

First Revolt and destruction of the Temple had nothing to do with either the stabilization of the biblical text or the deposits of the scrolls of Qumran, both of which were earlier than the disasters of 70 CE.

From section titled "The story of Ib/II continuity at Qumran (pp. 131-32)

A foundational assumption of the excavators of Qumran of the 1950s, which continues in scholarly discourse today, was the belief that a single group, associated with the texts in the caves, inhabited the site—from the beginning to the end, in the midst of the region's political upheavals for close to two centuries, until the site's destruction at the time of the First Revolt. There was one break between the end of Period Ib and the start of Period II (when the site was abandoned, then resettled by the same group). Below are representative statements of this story:

> The essential fact is the communal occupation of Periods I and II … In 31 B.C. an earthquake damaged the buildings which afterwards remained abandoned up to the years just before and just after the beginning of the Christian era. They were then reoccupied by the same community, Period II, and survived until A.D. 68, when they were destroyed by the Roman army … For a period of almost two centuries, therefore, a community lived in this abandoned region. (de Vaux 1973: 48, 86)

> The Qumran settlement existed for over 150 years until its destruction in 68 CE … communal meals and assemblies were a feature of Khirbet Qumran during its entire occupational history … the rebuilding of these dining halls provides strong evidence that Khirbet Qumran not only was inhabited by the same group during all of its main phases (de Vaux's Periods I and II), but that these buildings retained their same functions; in other words, communal living and dining were key components of the lifestyle at this site for over 150 years. (Atkinson and Magness 2010: 340-41)

An alert reader might notice at the outset an incongruity in the language of the "entire occupational history" of Qumran as having ended in 68 CE. The incongruity is: the destruction at the time of the First Revolt was not the end of the site's occupational history, since there was repair, resettlement and

reuse of the site after the fire of 68 CE. Use of the site did not end until some years later ("Period III"). Furthermore, the destruction of Qumran at the time of the First Revolt was simply the second of two major destructions according to the reconstruction of the excavators (the first at the end of Period Ib late first century BCE; the second at the end of Period II 68 CE). That is, the occupation of the site ended twice before the final and third time in the first century CE, according to the reconstruction of the excavators.

However, it was the second time Qumran came to an end that was the focus of this story. The excavators assumed as certain that there was discontinuity between the people of Period II and of III who occupied Qumran after the fire of 68 CE. This interpretation of discontinuity between Periods II and III stands in contrast to the people of Period Ib and of II who had occupied the site at some point after an abandonment at the end of Ib. The excavators were certain that the people of Ib and II were identical, despite some greater differences between Ib and II than between II and III.

The First Revolt was the fulcrum of the story of Qumran and the perceived endpoint of the scrolls in the Qumran caves. This story was established at the time of the original excavation of Qumran in 1951 when first-century CE pottery and coins were discovered and before the excavators had knowledge of the existence of Period III and of Bar Kokhba-era activity at the site or of an earlier Period Ib or a yet earlier Period Ia (de Vaux 1953). When these four additional periods were discovered in 1953-56, these were interpreted within the story already fixed around the hypothesis of a single group at Qumran, ending in the first century CE during the First Revolt (de Vaux 1954; 1956).

From section titled "Date of the end of Period Ib" (pp. 134-35)

[Update: following my sending this article in unpublished form to Dennis Mizzi, Mizzi and Jodi Magness have published an article representing significant modifications of longstanding views of Magness concerning Locus 120 described below and "Period Ib" and which appear to respond to and incorporate several points raised in the present article. See Dennis Mizzi and Jodi Magness, "Was Qumran Abandoned at the End of the First Century BCE?", JBL 135/2 (2016): 301-320.]

De Vaux supposed "Period Ib" ended in 31 BCE with a destruction by earthquake and fire. However, publication of the pottery of the Netzer excavations of Jericho by Rachel Bar-Nathan (2002) has shown that pottery of Qumran's Period Ib was identical to pottery from the first part of the reign of Herod uncovered at Jericho. It became clear that Qumran Ib continued into the reign of Herod later than the 31 BCE date of de Vaux, a conclusion earlier argued by Magness (1995; 1998a) and now confirmed correct.

> The earthquake of 31 B.C.E. did not put an end to the settlement at the two sites nor did it cause any change in the repertoire of vessels. In fact, between 31 and 20 B.C.E. (designated at Jericho as Herodian I), there was continuity in Hasmonaean pottery types while very few new types were introduced. The greatest change in pottery, according to the ceramic evidence from Jericho, occurred with the Romanization of pottery types in Judaea during the middle of Herod's reign, toward 20 B.C.E. (designated at Jericho as Herodian II). (Bar-Nathan 2006: 274)

> … the final dating of Period Ib at Qumran, which seems to be HR1. (Bar-Nathan 2002: 100) [3]

> The revised stratigraphic chronology suggested by Magness is in accordance with our research (Magness dates … Period Ib to 31 [*sic*]-ca. 4 [*sic*] BCE, and Period II to ca. 1-68 CE). (Bar-Nathan 2002: 203 n. 3)

The 4 BCE date for the end of Ib attributed by Bar-Nathan to Magness is not quite accurate. Magness has consistently dated the end of Ib as "9/8 BCE or some time thereafter", sometimes expressed as simply "ca. 9/8 BCE" (1995: 64; 2004: 57). Magness's focus on 9/8 BCE is based on an argument from a hoard of Tyrian silver tetradrachmas found buried at Qumran's Locus 120 of latest date 9/8 BCE. Magness argued that the L120 hoard was buried before the end of Ib and not after the end of Ib, as de Vaux had supposed. In Magness's reconstruction, the hiding of the hoard was prompted by a crisis after the latest coins. The crisis resulted in the end of Ib almost immediately after the hiding of the hoard. After an abandonment of unknown duration of one or more winter flooding seasons following the end of Ib, Qumran was resettled (beginning of Period II). Those who resettled the site did not know of the valuable hoard, hidden under the floor of Locus 120. Since it was

buried under the upper of two floor levels of L120, the upper floor in Magness's reconstruction becomes a second Ib floor (a post-31 BCE floor level above an ealrier, pre-31 BCE floor level), not a floor built in Period II as de Vaux had thought. In Magness's scenario, there is no Period II floor in L120 (Magness 2007: 250).

As a distinct issue, Magness argued that the people who resettled Qumran in Period II, were the same as those who had abandoned the site at the end of Period Ib. This raises the question of why, then, the returning people would have no knowledge of the hoard which Magness proposes had been wealth accumulated and hidden by the community before the end of Ib (Magness 1998b; 2004: 73-9). Magness leaves this question unanswered (2004: 45). One obvious possibility would be that the people of Period II did not know of the L120 hoard's existence because they were not the same people who had buried it; that is, there was discontinuity between Periods Ib and II.

The appeal of Magness's scenario, in addition to its compatibility with downdating the end of Ib to later in the reign of Herod, is that the latest date of the coins (9/8 BCE) is close to the time of the death of Herod (4 BCE). This was a period with upheaval and civil war, as recounted by Josephus, and provided a context for the crisis that, as de Vaux and Magness interpreted it, resulted in the end of Qumran's Period Ib in destruction by fire. (The Ib fire has, however, been questioned.[4])

Although the specifics of the end of "Period Ib" remain disputed, there is no doubt, in the wake of the Bar-Nathan 2002 Jericho pottery publication, that Qumran's Period Ib ended later than de Vaux had thought. Minimally, it extended into some part of Herod's reign after 31 BCE and, possibly, just before the end of the first century BCE. Let us now turn to specific aspects of the assumption of continuity of a "community" at Qumran through both Periods Ib and II until 68 CE.

From section titled "Pottery" (pp. 135-36)

Based on parallels at Jericho, Bar-Nathan reported that the 708 bowls of Qumran L86/89 appear to have ended in Period Ib and not in Period II or III: "In view of the absence of this bowl from first-century CE contexts at Jericho, the dating of the material from Qumran Period II might have to be

revised" (Bar-Nathan 2002: 89; *contra* Humbert 2006: 39: "This pottery [of L86/89] belongs to the final phase of Qumran occupation, *i.e.* to the middle of the first century A.D.", and *contra* Cross 1995: 62 n. 3: "an inscribed bowl from 86/89 seems clearly to be dated, palaeographically, to the first century AD").

Similarly, distinctive wheel-made "Qumran lamps" (called "hellenistic lamps" by de Vaux) were manufactured at Qumran exclusively in Ib and not in II or III. Thirty-nine of these lamps were found in the excavations of Qumran, according to the latest count by Jolanta Mlynarczyk (2013). All are from Qumran's Period Ib, late first century BCE. These lamps also turn up in smaller numbers at Masada, Jericho, and Qumran Cave 1Q all dated narrowly to the time of Herod late in the first century BCE, none from first century CE (Bar-Nathan 2002: 110-12). Mlynarczyk concluded on the basis of the numbers of the distributions that all lamps of this type had been manufactured at Qumran. When Period Ib ends, these lamps end everywhere.

In contrast to the significant numbers of these items produced at Qumran in Period Ib, none of these bowls or lamps were likely from Periods II or III. These two items suggest that the most distinctive pottery from Qumran may have been manufactured exclusively during Period Ib, whereas the pottery of Qumran Periods II and III was either not unique to Qumran or, perhaps, not manufactured there. De Vaux's later writing moved in this direction. In his 1959 Schweich Lectures, he stated:

> The pottery of Period II is very plentiful … There are certain features which underline the autonomy of Khirbet Qumran, where, as we have said, there was a manufacturing center, but, taken as a whole, this pottery [of Period II] has exact counterparts in that found in Jewish tombs of the first century A.D. in the Jerusalem area, in the soundings against the north wall of Jerusalem (the dates of these have been established by coins of the Procurators and Agrippa I), and finally in the excavations of Herodian Jericho. (de Vaux 1973: 33)

To the above, de Vaux added a footnote, in the context of this Period II discussion: "The pottery of Qumran now appears less 'autonomous' or 'original' than I stated it to be at an earlier stage" (1973: 33 n. 2).

Also of relevance on this point, attempts to classify locations of original manufacture of pottery found at Qumran based on INAA (Instrumental Neutron Activation Analysis) were published by Jan Gunneweg and Marta Balla (2003: 3-57; 2010). An alternative analysis has been published, with a differing interpretation, by Jacek Michniewicz (Michniewicz 2009). Michniewicz disagrees with one of the basic claims of Gunneweg and Balla—that pottery found at Qumran manufactured at Jericho can be distinguished by technical means from pottery found at Qumran manufactured at Qumran (Michniewicz: "there are no clues that would allow even a part of the vessels to be ascribed to a workshop in Jericho or Qumran" (2009: 142)). Michniewicz's analyses render equivocal some previously published identifications of non-Qumran sites of origin for pottery found at Qumran. Michniewicz characterizes the disagreements not as disputes over the validity of the published scientific data, but rather as issues of interpretation.

From section titled "Language" (pp. 137-38)

There appears to be a shift in language from an apparently exclusive use of Hebrew/Aramaic by people at the site in Ib to a substantial use of Greek in II and III, as reflected in the inscriptional material of Qumran published by André Lemaire (2003). In Ib, there are practice alphabets and writing exercises in Hebrew/Aramaic and numerous Hebrew/Aramaic inscriptions. In L124 (interpreted by de Vaux as debris from Ib), there were eight inscriptions, all in Hebrew/Aramaic and one with a possible word "priest". The unusual "deed of gift", published in DJD 36 as KhQ1 and the KhQ2 ostracon found with it, with some male names and a possible reading of "priest", found next to the Qumran cemetery in 1996, are probably late first century BCE, in keeping with the palaeographic description of Ada Yardeni of the writing of KhQ1 as "early Herodian semi-cursive".[6]

The situation is very different in Period II. David Hamidović adds inscriptions reported by Magen and Peleg, Émile Puech, and James Strange to the inscriptions from Qumran, the caves, and Ain Feshkha, published by Lemaire, to arrive at 93 total inscriptions of which 21 are in Greek. However, "none of the Greek inscriptions dates from Period I" (Hamidović 2009: 466, 471). Whereas the Hebrew/Aramaic inscriptions, so prevalent at

Qumran in Period Ib, continue to a reduced extent in Period II, the change is dramatic. Of seven certain plus two uncertain Greek inscriptions from the buildings of Qumran published by Lemaire (at L8, L27, L30, L35, L54, L110, L121, and uncertainly L78 and L111), seven are attributed to Period II and an inscribed jar from L8 to either II or III. Lemaire summarizes: "Pour les autres inscriptions grecques, seule une analyse strastigraphique détaillée permettrait de distinguer celles postérieures à 68; mais le plupart sont probablement antérieures" (Lemaire 2003: 381). The only one not given a date by Lemaire is a stone weight with Greek letters found in L110 among debris inluding three coins all of which are first century CE. Therefore, that Greek inscription also appears to be Period II/III.

These Greek inscriptions of Period II and/or III include the name "Joseph" in Greek on a stamp found in L30 apparently in use at the time of the First Revolt. According to Josephus, a "Joseph son of Simon" was made commander of Jericho at the time of the First Revolt of 66-70 CE (*J.W.* 2.567). This "Joseph son of Simon" may be identified with Joseph Cabi b. Simon, a former high priest of the Jerusalem Temple of ca. 62 CE of *Ant.* 20.196. Was this the Joseph of the Qumran L30 stamp? [7]

A small percentage (ca. 3 per cent) of the 900 plus texts in the caves of Qumran are in Greek, and these are probably all or mostly all first century BCE, contemporary with the many Hebrew and Aramaic texts. Out of ca. 930 Qumran texts, 27 are in Greek, compared to ca. 750 Hebrew and ca. 150 Aramaic, according to Tov (2008: 339). However, given the possibility that those Greek texts originated and were used elsewhere than Qumran and were brought to the site for disposal or hiding, it is unclear that that bears on the language in use at Qumran in Ib as indicated by the inscriptional material.

An opisthograph, 4Q460/4Q350, on which a list of quantities in Greek with check marks (4Q350) is written on the back of a Hebrew literary text (4Q460), has occasioned much discussion. The Qumran provenance of 4Q460/4Q350 need not be doubted for reasons discussed elsewhere (for example, Cotton and Larson 2003: 113-26), but what is to be made of the Greek checklist? If the scrolls in the caves were imported for the purpose of permanent disposal, the secondary use of the reverse side of one of the imported texts, 4Q460, may have been to write an ephemeral checklist either before its shipment to Qumran, while en route or at Qumran—in any case,

before the text was put in Cave 4Q. In favor of the first or second possibilities and against the third is the lack of evidence at the site for the use of Greek in daily life at Qumran in Period Ib, the era of the deposits of the scrolls per the present argument. The possibility that the Greek list was written on the back of 4Q460 by a later rummager in Cave 4Q (at some point after the texts had been deposited) is difficult to exclude, however.

There is no reason to suppose from the absence of Greek inscriptions at Qumran in Ib that there were scruples against using Greek, or that absolutely no one knew Greek. But it does suggest that Greek was not much in use by the people of Qumran in Ib. It heightens the question of whether the non-Greek users of Ib and the heavy Greek users of II were the same group as commonly supposed.

From section titled "Women" (pp. 138-140)

For Period Ib, there seems to be no evidence for women at Qumran. There are no women's names among the names of the ostraca from Period Ib, nor are there any known clear cases of gendered finds from Period Ib suggesting the presence of women. But it is certain that there were women at Qumran in both Periods II and III in the first century CE.

Evidence of the presence of women is based on gendered finds such as jewelry, beads, and spindle whorls, as discussed in the studies of Taylor (1999: 317-21) and Magness (2004: 113-49). Whereas none of these items seem to have been found in Ib contexts at Qumran, the situation in II and III is very different. A spindle whorl (No. 633) was found in 1953 in de Vaux's L99 "Railway" Trench in a fill of Period II items. Two years later in the same area the excavators found an earring (No. 2003), presumably also from Period II. For Period III, a spindle whorl (No 401) was found in L20, and a bracelet and a green bead were found in L43 in a Period III context. A bead was found at L35 in a context which has a Vespasian 69-70 CE coin, and therefore belongs to III. A bead from L44 is either II or III.

Fibulae, possibly from women, were found in several II/III contexts, including L44, L56, L96. A fourth is possible from the "Railway" Trench. That some of these fibulae had belonged to women is suggested by the fibula from L44 (No. 1020), found with two bronze rings (Nos. 1018, 1019). In the

"Railway" Trench, a "rod or fibula of bronze" (No. 2007) was found with the earring mentioned above. In addition to the de Vaux finds, Magen and Peleg also reported finding "bracelets" and "rings (some with stone insets)", though without disclosure of locus or stratum (2007: 21).

A wooden comb was found in Cave 1Q. That comb might have belonged to a woman. However, no other women's item was found with it and it is difficult to exclude the possibility that men also used combs. Tom Derks and Wouter Vos comment on the comb/gender issue of the era in the wider Mediterranean world:

> there is a wealth of contextual data that supports the association of combs with women … representations of combs and other grooming tools, for instance, seem to figure nearly exclusively on memorials for women … long list of comb finds from more prosaic burials may be cited as references to the same virtue of female beauty … comb finds and iconographic depictions of combs provide strong arguments for the comb as a marked symbol of female identity, especially in the ritual context of the funeral. Against this background, the apparent absence of combs, real or depicted, on male funerary monuments or in tombs for men, is most striking. (Derks and Vos 2010: 62-3)

However, Derks and Vos also cite literary allusions to upper-class Roman men using combs (for example, a mocking question to a bald man: "of what use will be this piece of box-wood, cut into so many teeth, and now presented to you, seeing that you have no hair?" (Martial, *Epigrams* 14.25). A remark that a worthy centurion had broad shoulders and wild, uncombed hair may imply that some men did comb their hair (Juvenal, *Satires* 14.194). Taylor's study characterizes combs as a gendered women's item (1999: 318), but Katharina Galor disagrees: "combs do not elucidate any matters of gender as both archaeological and literary evidence suggest that combs were used by both male and female (Whitehouse 200[6]: 748)" (Galor 2010: 32). Possibly weighing against Galor however, Magness cites an excavation at Khirbet ed-Deir of a sixth-seventh century CE Byzantine monastery inhabited by a male population published by Hirschfeld in which "no spindle whorls or shafts, beads, or jewelry, combs, hairnets, mirrors, or cosmetic items" were found; note the absence of combs (Magness 2004: 131, citing Hirschfeld 1999).

It is intriguing to consider a woman associated with the comb and the domestic pottery stored in Cave 1Q at the time of the First Revolt. The woman (?) of Cave 1Q probably would not have been alone, based on the two knife-pared, bow-spouted "Herodian" lamps found in Cave 1Q, as reported in DJD 1. The trapezoidal shape of, and double line incisions on, those lamps' nozzles, indicate those lamps belong to Smith's Type 2 class of "Herodian" lamps, believed to have begun in Judea ca. 35 CE and the most common type of lamps in Judea at the time of the First Revolt (de Vaux 1955: 11, Fig. 3.1; Mlynarczyk 2013: 108-10). So this is plausibly activity dated to the time of the First Revolt (based on the Smith Type 2 "Herodian" lamps), activity possibly including a woman (based on the comb) in Cave 1Q at that time.

A bronze earring or nose-ring, with domestic pottery from the approximate time of the First Revolt, was found in Cave 24, about 50 meters north of the scroll-bearing Cave 11Q.[8] Unlike Caves 1Q and 11Q, there were no scrolls or scroll jars found in Cave 24. There was only an indication of the use of that cave by refugees or possibly shepherds in the latter half of the first century CE. This activity appears to have included the presence of at least one woman, analogous to the possible presence of a woman in Cave 1Q at approximately the same time, and perhaps for similar reasons, as Cave 24.

Therefore, it seems likely that women were among the refugees or fugitives using caves around Qumran for temporary habitation or storage of personal supplies at the time of the upheavals of the First Revolt, and this is consistent with the finds indicating the presence of women at the site of Qumran in the first century CE. Again, there is an apparent qualitative difference between Ib and II/III at Qumran. All of the known signs of women's presence appear only in Periods II and III, with no sign of women at Qumran in Period Ib.

It is true that no women's names have turned up in the ostraca from Qumran Periods II or III, just as is the case with Ib. There seem to be no names securely identified from Qumran's Period III at all, and the absence of women's names in the finds from II may be attributable to the smaller database of such names and accident, in contrast to the larger number of 100 percent men's names in Ib which do give an impression of an actual absence

of women at the site in Ib (with both of these interpretations of Ib and II in keeping with the gendered finds pertaining to those periods).

From section titled "Animal bone deposits" (pp. 140-41)

Distinctive animal bone deposits found inside and under pottery in open areas of the site of Qumran were said by de Vaux to be "the clearest proof" that the people of Periods Ib and II were identical (de Vaux 1973: 120). De Vaux acknowledged that most of the animal bone deposits were Ib ("the majority belong to Period Ib") (de Vaux 1973: 13). But de Vaux claimed that the same kind of deposits were also found in Period II at loci 73, 80, 130, and 132 (de Vaux 1973: 13 n. 1).

Followed and cited by countless scholarly expositors has been de Vaux's view that Period II animal bone deposits were evidence that the same people of Period Ib returned to Qumran to inhabit the site throughout Period II.

However, after detailed analysis of Locus 130 by Robert Donceel based on hitherto unpublished pencil drawings and a topographical map, as well as stratigraphy, Donceel concluded that de Vaux had erred in saying that any of the animal bone deposits of L130 date to Period II: "nothing justifies the supposition according to which the so-called practice of 'under-jar bone-deposits' continued after the end of de Vaux's 'Period I' (end of the 1st cent. B.C.), at least in locus 130; the evidence of the pottery, lamps, and the coins is unequivocal" (Donceel 2005: 69). Donceel allowed for the possibility that the animal bone deposits could have continued at later times elsewhere at the site. "Rien n'interdit d'ailleurs *a priori* d'envisager que le processus ait pu être continué ailleurs sur le cite (c à d. en d'autres endroits de la périphérie des bâtiments) à une époque plus récente" (Donceel 2005: 46). However, Donceel noted that it would raise the question of why the animal bones stopped at the end of Period Ib in L130, if they continued in Period II elsewhere.

Turning to L132, which adjoins L130, de Vaux claimed that *only* Period II animal bone deposits were found in L132 (de Vaux 1973: 13 n. 1). But in his field notes he noted that L132's pottery was like that of L130 ("cleaning and removal of the pottery items with [bone deposits] 1, 2 and 3 in the southwest corner. The ceramics are analogous to those of locus 130" (L132, 9/3/55)),

and de Vaux understood that L130's pottery was from Period I (L130, 2/3/55). De Vaux's field notes also reported two coins (Nos. 2425, 2426) found with one of the animal bone deposits at L132 at his entry for 14/3/55. Those two coins are now identified, albeit with question marks, as being from Alexander Jannaeus and [John] Hyrcanus [I] (Pfann 2003: 57-8), which point to Period Ib, not II.

As early as 1998, Robert Donceel reported that, on the basis of the de Vaux excavation materials, all of the animal bone deposits in the northern enclosure (L130, L132, L135) belonged to Ib (Donceel 1998: 99-104).[9] Yet, to the present day Donceel's detailed correction of the dating of the animal bone deposits remains almost completely unacknowledged and unaddressed in relevant secondary literature as if it were unknown.

In a 2001 lecture, Humbert also reassessed de Vaux's interpretation of L130 and concluded that de Vaux had been mistaken, suggesting a "halting of bone deposits" in the first century BCE by new settlers at Qumran, possibly reflecting a change in religious practice (Humbert 2003b: 435-6). These analyses concerning L130 and L132—and their conclusions that the animal bone deposits in L130 and L132 ended in Period Ib—call for critical scrutiny of the two remaining loci in which de Vaux claimed to have found Period II animal bone deposits: namely, L73 and L80. Unfortunately the publication of L73 and L80 has been inadequate.

Yitzhak Magen and Yuval Peleg reported animal bone deposits in the first century CE (Period II) from their excavations in 1993-2004: "vessels containing bones dating from the Hasmonean period were found in the southern dump, and elsewhere from later periods, up to the site's destruction. The disposal of bones within the site was thus a permanent feature" (Magen and Peleg 2007: 43). Unfortunately, Magen and Peleg did not disclose where "elsewhere" was. Nor did they offer any other information concerning their claim. The excavations of Randall Price and Oren Gutfeld of 2002-2012 found still more animal bone deposits, but none, in their estimation, were of first-century CE date. These deposits were "concentrated along (and even under) the eastern wall marking the eastern edge of the settlement … Price assigns the animal bone deposits that he found to the pre-31 BCE phase of Period Ib" (Magness 2011: 358-9).

The currently available information can be summarized as follows: the animal bone deposits are extensive, certain, and verified in Period Ib (possibly the remains of some industrial process carried out at the site in addition to meals, suggest Donceel (2005: 49, 72) and Stacey (2013: 55)). There are, moreover, unverified claims of a few more such deposits from Period II. Is such lack of published documentation and verification for Period II animal bone deposits a solid foundation for an assumption of Period Ib/Period II continuity (de Vaux's "clearest proof")?

From section titled "Quantity of tableware" (pp. 141-44)

De Vaux's, now traditional, view was that Qumran had communal dining through both Periods Ib and II, ending in 68 CE. There was, however, no such dining in Period III. The destruction of 68 CE was the point at which, as de Vaux put it, "community life at Qumran no longer exists" (de Vaux 1973: 43). This understanding is not supported by actual evidence. The argument for the practice of communal dining at Qumran, even in Period Ib in which large quantities of tableware were found, has been contested:

> The idea of "community" was the intuition that very early on determined the historic-archaeological characterization of the site … The collective installations [at Qumran] that [de Vaux said] support his idea of community are not at all convincing … An impartial archaeological examination of the entire ruin does not detect the morphology necessary to shelter a large society … The housing of a large group is not practical here … To suggest that the most important members of the community lived in the cliffs' caves and in "camps" set up near the site is surprising. The caves are merely erosion pockets or crevices and one cannot imagine that, even in antiquity, a group of sedentary people would have lived for two centuries in tents … We must accept that Qumran does not present any of the necessary criteria for an identification of a communal life … The community at Qumran does not have substance. The two hundred or two hundred fifty members that one wishes to place there are a fabrication of the mind, and the archaeology does not lead in this direction. (Humbert 2003b: 425-32)

> De Vaux, influenced by the concept of Qumran as a communally-eating, sectarian settlement, extravagantly identified this room [L77] as a "refectory" and "a place where the president of the assembly would have taken his stand", although evidence from Masada, already available to de Vaux before his death, shows that such long, comparatively narrow, rooms were used as storerooms. Similar storerooms have since been found at Herodium and Jericho. (Stacey 2013: 41)

Atkinson and Magness's response in defense of the traditional view paradoxically seems only to further weaken the argument for Qumran's uniqueness underlying the sectarian community interpretation:

> The sectarian practice of dining from individual dishes accounts for the discovery of hundreds of small plates, cups, and bowls in pantries attached to the communal dining rooms at Qumran (L86 and L114) … this custom seems to have been widespread in the first century BCE, judging from the large numbers of plates, saucers, and bowls found at sites around the country. Andrea Berlin describes as follows the evidence from Gamla: "Each person had a small plain buff fabric bowl for his own portion." (Atkinson and Magness 2010: 332)

However that debate is ultimately resolved with respect to Period Ib, the important point is that the two large inventories of dining pottery found at the site at L86/89 and L114—the main basis for the argument for the interpretation of communal dining at Qumran—*both ended at the end of Ib* (for L86/89: de Vaux 1973: 11-12; Magness 2004: 91-2; Bar-Nathan 2002: 203-4; 2006: 266-74. For L114: de Vaux 1973 5 n. 1; Magness 2004: 103-4; Atkinson and Magness 2010: 332 n. 48; Bar-Nathan 2002: 89 (J-BL5 bowls at L114)).

There is nothing found at Qumran from either Period II or III comparable to the scale of the tableware of L86/89 and L114 of Period Ib. At the destruction of Qumran during the First Revolt, there were multiple loci with small amounts of dining pottery used by people living in rooms, reflective of individual or nuclear-family/small household-scale or refugee levels in different rooms, with no evidence of communal or large-scale dining. Period II is essentially indistinguishable from Period III with respect to quantities of

tableware per person. Period Ib differs dramatically from both II and III in quantity of tableware. The large quantity of cups and bowls are specific to Period Ib.

Magness reconstructs an upper-storey room built over L77 used for communal dining in Period II. Her main arguments for the existence of communal dining in Period II appear to be two. First, that communal dining would be expected to have continued in II on the assumption of continuity of the same group from Ib. And second, the animal bone deposits claimed by de Vaux to have continued in Period II are interpreted to mean that communal dining continued in Period II. After arguing for the existence of the L77 upstairs room, Magness cites tableware in Period II debris in nearby L58 as having come from the communal dining conjectured to have taken place in L77 upstairs (Magness 2004: 101; Atkinson and Magness 2010: 341).

The problem in Magness's argument is in the numbers. In L86/89 of Period Ib were found, according to Magness, 279 shallow bowls, 798 hemispherical cups, 150 deep cups, 37 deep bowls and 11 table jugs for pouring wine. The other main tableware find of Ib, L114, had 39 shallow bowls, 111 hemispheric cups, 9 deep cups, 1 large bowl, and 1 table jug.

In L58 of Period II, however, no cups, and only 3 goblets, 7 bowls, 2 plates, 18 jugs, and 1 flask were found. The numbers are hardly comparable between the Ib loci on the other hand, and L58 from II on the other. The low total numbers of dishes in L58 (found in two separate piles) are not distinguishable from the domestic pottery of a household, as in the cooking pottery and dishes found at other loci in II and III. There is no evidence from L58 for the existence of communal dining in Period II, whether in upstairs L77 or at any other locus.

Magness conjectured the existence of another upper-storey room used for communal dining on the western side of the site in Period Ib associated with the pottery of L114 (2004: 81, 112). Even though the L114 pottery ended in Ib and there is no comparable find of large numbers of tableware from Period II on the western side, Magness nevertheless suggested that communal dining on the west side also continued in Period II. In support of this, Magness cited de Vaux's claim of animal bone deposits in Period II in

the northern enclosure (but that has been convincingly argued by Donceel and Humbert to have been unsupported and mistaken). Magness also conjectured that de Vaux's description at L126 of "much pottery, several bowls or plates, a flask, etc." (23/2/55) could have been from the conjectured Period II communal dining on the western side (Magness 2004: 104-5 n. 110).

In short, Magness has made an argument for communal dining at Qumran based on an interpretation of the real finds of Ib and then conjectured its continuation in Period II without evidence. As pointed out by Bar-Nathan, disposals of huge numbers of bowls in *miqveh*s or pools at Jericho during the Hasmonean era and HR1 appear to have some relationship to L86/89 of Ib at Qumran. The same kind of bowls appear in large numbers—so many similar kinds of bowls at the two nearby sites, with possible evocation of ritual uses, that the question was raised by Bar-Nathan whether the finds from Qumran Ib and the Hasmonean Palaces at Jericho reflect practices of the same sect at the two sites:

> the finds from Qumran (Period Ib) which are, in fact, identical to those exposed in the Hasmonean palace complex at Jericho … the abundance of *mikva'ot* [at the Hasmonean palace complex at Jericho] and the many bowls and plates in them may be connected with the laws of impurity and cleanliness in force in Jericho at this time. (Bar-Nathan 2002: 5)

> the lack of imported pottery, and the profusion of bowls and their relationship to the *miqvaot* [at Jericho], might all be related to unwritten Sadducee laws and customs … the great resemblance of the pottery in Hasmonean Jericho to that of Qumran Period Ib is notable. (In this frame we shall not discuss whether this similarity indicates a similar sect in both sites, or common Jewish laws or habits, which were practiced throughout Judea.) (Bar-Nathan 2002: 198)

The large numbers of cups and bowls at Qumran in Period Ib are therefore neither unusual nor unique. What is unusual are scholarly claims that distinctive dining practices or purity concerns, correlating to the profligate use of tableware common to the Hasmonean Palaces at Jericho and at Qumran in Period Ib, were present at Qumran in the first century CE and in

Period II, and differed from practices at other first-century CE Judea sites as well as in Qumran Period III. That notion is without evidence.

From section titled "Ownership of the site" (pp. 144-45)

An interpretation that Qumran Ib was an extension of the Hasmonean and Royal Estate in Jericho is implicit in the Jericho Netzer excavations' pottery volume of Bar-Nathan of 2002. That interpretation has been developed explicitly by David Stacey (2007: 237: "Qumran must be considered an integral, though outlying, part of the Royal Estate"; 2013). If that interpretation is correct, Qumran may have been owned or controlled by high priests during much of the first century BCE. As Humbert put it, "Qumrân, par proximité appartient à Jericho et qui tenait Jéricho tenait Qumrân" (Humbert 2003a: 469). Historian Samuel Rocca refers to the first-century BCE high priest "Hyrcanus II who was, in fact, the owner of the palatial environs [at Jericho]" (Rocca 2008: 115).

Ownership of the site of Qumran would be expected to have changed with political changes. There is no reason to suppose the same ownership continued in Period II as in Period Ib. It follows that the assumption that the people of Qumran of II were the same as the people of Qumran of Ib is further weakened.

From section titled "Scrolls" (p. 145-46)

In all of the scrolls found in Qumran's caves, nearly one thousand texts in all, there is not one allusion to an event, name, ruler, figure, or historical context as late as the first century CE. Nor is there a single text composed later than the first century BCE. On the other hand, there are numerous allusions to earlier figures and contexts of the first century BCE, as well as many scrolls among the finds which were written in the first century BCE (Atkinson 2007; Vermes 2007; Wise 2003).

Oddly, the most obvious conclusion from such distribution is hardly considered: namely, that the Qumran texts had been deposited by the end of the first century BCE. The latest formal scribal hands in the Qumran texts are "late Herodian formal" and were, accordingly, dated to the mid-first century CE by Frank M. Cross (1961)—a date which has been applied to the

Qumran texts ever since. However, the naming and dating of the scripts of the Qumran texts originated in the prior belief that the dating of the text deposits had already been established in the First Revolt. On that only apparently secure starting point, it was assumed that scribal copies among the Qumran texts had been produced continuously until just before the First Revolt. In this way, the latest scribal hands in the Qumran texts became "late Herodian formal" and dated to ca. 50 CE.

The origin of the dating of "late Herodian formal" script has been lost in collective scholarly memory to such an extent that such palaeographic datings are today often regarded as evidence that Qumran texts were produced as late as ca. 50 CE and that the interpretation which had created those dates is therefore confirmed, without awareness of the circularity in the reasoning. In fact, such palaeographic dating provides no independent verification that Qumran texts were produced as late as the first century CE, since that result had been based on the premise that the First Revolt was the endpoint. First-century CE Qumran text palaeographic datings are no stronger than the archaeological grounds for the First Revolt scroll deposits.

The radiocarbon datings, carried out on 19 Qumran texts at Zurich and Tucson in the 1990s, confirmed first-century BCE dates of scribal copying of many Qumran texts and also that a smaller percentage of second-century BCE texts is plausible or likely, but those radiocarbon datings have not confirmed Qumran cave literary text activity from the first century CE. Compare this assessment from two radiocarbon scientists: "The dates suggest a possible range from the third century BC to the first century AD for texts from caves near Qumran, with a strong concentration of probable dates in the second or first century BC" (van der Plicht and Rasmussen 2010: 111). A revised palaeographic chronology, in which neither the latest formal not semi-cursive scribal hands among the literary texts of the Qumran caves postdate the first century BCE, does not create a problematic gap in typological development in the first century CE. The gap is filled by "late Herodian formal" writing, recognized to have begun earlier than previously supposed, and recognition of post-Qumran-scrolls' formal and semicursive scripts which are pre-70 CE.

This direction of revision in palaeographic dating seems to be already underway. Two of the three biblical texts used by Cross in 1961 to define

post-70 CE "post-Herodian formal" scripts (5/6HevPs, MurXII, MurGen[a]) are now dated before the First Revolt: namely, the Nahal Hever Psalms Scroll and the Murabba'at Minor Prophets scroll. The Psalms Scroll, characterized from the beginning as postdating the latest Qumran scribal hands and listed in Cross's charts as such, was dated in DJD 38, "*c.*50-68 CE" (Flint 2000: 143). In 2006, Cross redated the Minor Prophets Scroll of Murabba'at (MurXII), which also had been understood from the beginning to postdate all Qumran texts and to be of ca. early second-century CE date, before the First Revolt (Cross 2006: 67: "the great *Minor Prophets Scroll* from Murabba'at, dating to *ca* 50-70 CE") The existence of a well-attested class of semicursive writing of the first century CE prior to the First Revolt which postdates even the latest semicursive scripts in all of the Qumran texts, meanwhile, has already long been recognized by Cross (1961: 190 n. 9).

From section titled "Toward a better understanding of the ending of the scroll deposits" (pp. 147-48)

We may well imagine that the scroll deposits were texts from Jericho or from synagogues in the vicinity of Jericho and that they were taken to Qumran for deposit in the caves during an era that came to an end within a context of change in the control of the Palaces in Jericho. It was a time when the palaces were placed under Herodian ownership, a situation with new relationships between Herod and the priests associated with the last Hasmoneans. Under a scenario of discontinuity in ownership of the site or interruption in the source of the scrolls as the reason for the end of the scrolls in the caves of Qumran, the scroll deposits might have been an activity directed by priests or officials associated or formerly associated with the Hasmonean Estate in Jericho. Local seasonal workers engaged in industries at Qumran need not have been involved in decisions to deposit scrolls in the caves, or in the circumstances in which those deposits ceased.

The argument that the scrolls in the caves were long-term permanent disposals has been well made by Joan Taylor (2012a; 2012b: 273-303; cf. Stacey 2013: 63). Probably the best recent argument for a different theory of an emergency hiding (with the intent of retrieving the scrolls never realized) is that of Mladen Popović (2012). Whether the scrolls were deposited wić or without intent to recover them, the scroll-bearing caves close to the site of

Qumran show signs of significant disturbances and intrusions, subsequent to the deposits. For example, it is unlikely the scrolls in Cave 4Q had originally been deposited unrolled, opened, and strewn about. Yet at the same time, as has often been remarked, the disturbed conditions of the scrolls in Cave 4Q were ancient and not far removed in time from the deposits of those scrolls, since the strewn and scattered scrolls in Cave 4Q were found on the cave's floor, below ca. 1,900 years of dirt and animal dung (Cross 1995: 34 n. 1). In light of the known phenomenon of temporary habitation and refugee activity around the time of the First Revolt in the caves near Qumran and at the site itself, such activity is one possibility for the early disturbances of the scrolls in the caves. Another possibility would be from first-century CE persons earlier in Period II.

In the case of Cave 4Q, the unwrapped and unrolled scrolls strewn on the cave's floor could suggest that scrolls, originally placed in the cave in an orderly manner wrapped and tied, had been successively torn open and looked at, then discarded, that is, rummaging by someone literate. An hypothesis of ancient rummaging by literate scroll readers could account for the presence of the unattached leather straps and tabs found in Cave 4Q and 8Q of the kind used to secure rolled-up scrolls. That is, they were torn off and discarded in the process of opening scrolls for examination, anciently—by people at Qumran of the first century CE, not depositing those texts but accidentally finding them there.

As the tables of data of Ian Young have confirmed, the Qumran biblical texts are pre-stabilization and not exact-MT texts, whereas the biblical texts found at other Judean Desert sites are post-stabilization exact-MT texts. In agreement with Young in the interpretation of these data distributions, we conclude that the exact-MT stabilized Judean Desert biblical texts reflect the true circumstances of Hebrew biblical texts *in first-century CE Judea before the First Revolt*, while the Temple of Jerusalem was still standing. All of the Qumran scrolls are earlier than this. They are, therefore, associated with the end of Qumran's Period Ib, and the dating of the stabilization of the biblical text seems best dated late in the first century BCE or early first-century CE, perhaps related to the new temple of Herod.

From the Conclusion (p. 149)

This chapter has surveyed several issues bearing on the assumption of continuity between Qumran's Periods Ib and II, including pottery, language, women, dining, animal bone deposits, ownership, and scrolls. Such considerations suggest that it is misleading to speak of a single "main period of habitation" of a single group or community at Qumran, which ended at the time of the First Revolt. Qumran's Periods Ib and II should not be conflated in scholarly discourse as if they imply a single context. A less inaccurate picture may be that the true "main period" of Qumran was simply "Period Ib" of the first century BCE.

ENDNOTES [from excerpts above]

3. Note that Bar-Nathan's dating here of the end of Qumran Ib to the time of Jericho HR1 is not clearly a misprint and is in keeping with Bar-Nathan's logic and argument, despite Bar-Nathan's expressed agreement later in the volume with Magness's later dating of the end of Qumran Ib to the time of HR2.

4. Following the publication of de Vaux's excavation notes in 1994, I noted that evidence seemed lacking in those notes for a Qumran Ib fire, and I suggested that "the notion that Qumran Period Ib ended with a destruction by fire should probably be dispensed with for lack of evidence. As basic as a Ib fire has been in almost all discussions of Qumran archaeology up to now, I was surprised to discover in de Vaux's excavation notes in Humbert & Chambon 1994 that in not a single locus can a Period II fire layer be identified over a Period Ib fire" (Doudna 1999: 35-6). This interpretation was subsequently taken up by Humbert (2003b: 436) and Hirschfeld (2004: 88). Atkinson and Magness, however, without noting these discussions or offering new argument, continue to refer to the end of Qumran Ib by fire as if it is an uncontested fact (2010: 325).

[Update: In the 2016 JBL article of Mizzi and Magness noted above, Magness has repudiated the Ib fire/destruction. I was the first to discern and publish this insight which has now become mainstream understanding of archaeologists who work with Qumran. Mizzi and Magness do not cite me (or Humbert or Hirschfeld) in making their abandonment of the Ib fire.]

6. Rather than the Period II/First Revolt dating of Cross and Eshel in DJD 36 (Cross and Eshel 2000; Doudna 2007: 106-11 (Ib dating); 2007: 112-14 (possible "priest" reading); Puech 2007: 21 ("sa datation paléographique peut s'étaler sur un siècle auparavant, avec une forte préference dans la période hérodienne ancienne et même hasmonéenne tardive"); Yardeni 1997 ("early Herodian semi-cursive")). No inscription in Greek was found at Qumran from Period Ib.

7. With regard to L30, in light of the often-remarked pattern of an absence of major new building features in Period II at Qumran in contrast to Ib, it may be that the plastered furniture from the upper storey of L30, which has occasioned so much discussion, was from Period Ib, that is, inherited already there in L30 by the people of II. Note the plastered furniture debris item (No. 2465) found in the sediment of L130 of Period Ib which Robert Donceel observed was "de même apparence que le mobilier tombé de l'étage du locus 30" (Donceel 2005: 36).

8. "the entire ceramic corpus from this cave [Cave 24]—jars (at least ten), cooking-pots (at least six), juglets (at least five), and the oil-lamp, suggest the last phase of the Herodian period or even a post 70 CE date" (Patrich 1994: 90).

9. "Tout en travaillant sur les lampes de terre-cuite qui en proviennent nous avons tenté de recomposer la stratigraphie de cet emplacement du N-O du site à partir de toutes les informations disponibles, et sommes arrivés à la conclusion qu'aucune de ces dépositions n'y est postérieure à la phase Ib du R.P. de Vaux (c'est-à-dire la fin du 1er s. av. J.C.) … la date la plus récente pour ces dépositions dans ce locus 130 d'ossements d'animaux dans les vases ou des tessons (date étayée notamment par les monnaies) est la fin de la période Ib du R.P. de Vaux" (Donceel 1998: 99-104).