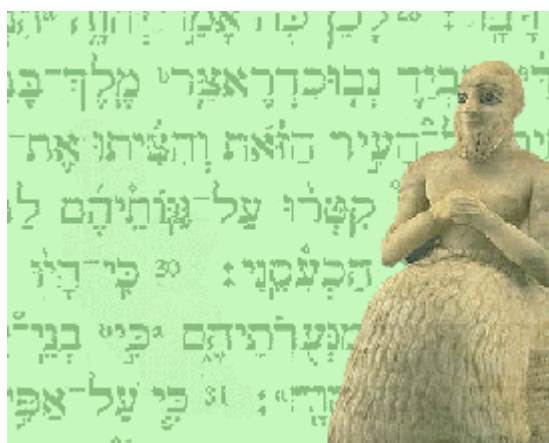


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Greg Doudna, Ostraca KhQ1 and KhQ2 from the cemetery of Qumran: a new edition

Ostraca KhQ1 and KhQ2 from the Cemetery of Qumran:

A New Edition¹

Greg Doudna

1. Introduction.

1.1. In 1997 two ostraca accidentally found in 1996 at the edge of the cemetery at Qumran by the University of South Florida excavations under the direction of James Strange were published by Frank Cross and Esther Eshel in *Israel Exploration Journal*.² Ostracon No. 1 (KhQ1) is the most extensive writing yet known from the site of Qumran itself, as distinguished from manuscripts found in nearby caves. This ostracon has 16 lines (the Cross/Eshel transcription had only 15, but traces of a 16th are visible). The letters are faded and sometimes unreadable, especially in lines 9-16, and the left ends of all lines are missing. Cross and Eshel identified it as a deed of gift of property, dated “the second year” of something. Ostracon No. 2 (KhQ2) contains only a few letters from four lines. According to early reports a third ostracon was found with the first two, containing only traces of ink; this third ostracon has not been published.³ While the find spot for these ostraca at the

¹ I gratefully acknowledge the financial support of the Danish Institute for Advanced Studies in the Humanities, Copenhagen, which made possible the preparation of this article.

² F.M. Cross and E. Eshel, “Ostraca from Khirbet Qumrân”, *Israel Exploration Journal* 47 (1997), 17-28. The find site of the ostraca was described as “the base of the eastern face” of “the eastern perimeter wall separating the buildings and court of the community centre from the cemetery” (p. 17), that is, the cemetery side of the wall. Also, F.M. Cross and E. Eshel, “A New Ostracon from Qumrân”, *Qadmoniot* 30 (1997), 134-36 (Heb.); F.M. Cross and E. Eshel, “The Missing Link”, *Biblical Archaeology Review* 24/2 (March-April 1998), 48-53, 69.

³ A first report in *Archaeology* magazine, 1996: “Three ostraca, one inscribed with 16 lines of Hebrew text from the first century A.D., have been found in Qumran, Israel ... The sherds were spotted on the east side of the Qumran plateau by volunteer excavators led by James F. Strange of the University of South Florida. One ostracon, broken into two pieces, mentions several commodities and is believed to be a list of food and supplies. The text opens, ‘In the second year,’ probably referring to the second year of the First Jewish Revolt, or A.D. 68 ... According to Strange, the pottery pieces were probably thrown out in haste or were part of the debris left after the Roman looting of the site. One of the other ostraca found nearby has parts of four lines of text. The last ostracon shows traces of ink” (S. Stanley, “New Texts from Qumran”, *Archaeology* 49/3 [1996] [<http://www.archaeology.org/9605/newsbriefs/qumran.html>]). *James Strange*, 1996: “... We proceeded to clean up, and behold an ostracon! It was lying on the ground at the base of the east wall east and north of the shade (the sukkah where tourists stand to look at Cave 4.) It was recovered by brushing. The ostracon was in two pieces which mend for a total of 18 lines of text in a clear Herodian hand. We can read the first two lines: ‘In the Second year./By the hand of El‘azar ben Nechumia’ ... We also found a fragment of a second ostracon, which looks like a part of scribal

edge of the cemetery is out of their original context—likely put there from an ancient clearing of debris from the buildings—their origination at the site of Qumran is practically certain, since there is little plausibility in supposing that drafts written on the ancient equivalent of scratch paper would be imported from somewhere else.

1.2. KhQ1 received attention because of early claims that a specific point of contact with 1QS, the Community Rule, was present on the ostrakon. In line 8, Cross and Eshel reported the existence of a reading]**וּכְמַלּוּתוֹ לַיְחָד**], “when he fulfills (his oath) to the Community [*yahad*]”. Cross and Eshel linked this to lines 2-3 in which property is **נָתַן**, “given”, but not, as in other deeds of gift, from a man to a woman family member. This property is given by a man to another man who does not appear to be of his family. Cross and Eshel saw in this a correspondence with 1QS 6.17-23 in which a new member of the *yahad* turned over his wealth to the community in his second year. As Cross/Eshel put it: “The receiver of the grant ... is a certain 'El'azar son of Naḥamanî ... [We] take 'El'azar to be a major official of the *Yahad*, probably the Overseer (**מְבַקֵּר**) or the guardian at the head of the congregation (**פְּקִיד בְּרֵאשׁ הַרְבִּים**) who handled the funds of the sectarian community living at Qumran.”⁴ Cross/Eshel concluded, “The word *Yahad*, which appears on this ostrakon, establishes the connection between Khirbet Qumrân and the scrolls which were found in the nearby

practice or of schoolboy practice. A third was found by dipping. It appears to be merely some ink on a sherd, but it may be a name” (James Strange, forwarded to the Orion discussion list, 13 Feb 1996 (<http://orion.mscc.huji.ac.il/orion/archives/1996a/msg00052.html>)). *James Strange*: “The excavators [Strange and crew] had excavated straight down into virgin soil in the center of the terrace and dumped beside the east wall. When cleaning up the soil, the volunteers were sweeping and scraping lightly with trowels beside the same outside wall of the ruin. The idea was to remove every last trace of the intrusive soil originating from the middle of the terrace. One volunteer [Joseph Caulfield of Everett, Washington] heard the clink of his trowel on a sherd, picked it up, and saw writing. The sherd—and others—therefore came from the top layer of the trench left by Père de Vaux when he excavated outside the wall” (Strange, 4 Oct 1997, quoted in P. Callaway, “A Second Look at Ostrakon No. 1 from Khirbet Qumrân”, *Qumran Chronicle* 7 [Dec. 1997], 145-170 at 156 [the opening reference to “the excavators” refers to Strange's excavation, not de Vaux's]). *James Strange*: “The volunteer was scraping the top of the next layer with a trowel. This sometimes results in what we call ‘overdigging’ in archaeology. While removing the upper, later layer it is preferable to scrape or dig slightly into the lower layer to insure that not one scrap from the upper layer was left to contaminate the lower layer. May I add that our ‘dump’ on top of de Vaux's trench next to the wall was sterile of any human-made object. We were removing sterile marl, gravel, and clay which had been removed from 15 m. deep in the terrace. There was simply no chance that the ostraca came from that deep in the terrace. Since the volunteer was brand new, he did not know to leave a significant find in situ. Therefore, in his first encounter with a sherd in situ (the clink of the trowel), he picked it up to take a look and saw writing. He then took it to me (I was talking archaeology in front of the book shop with an official person), and I immediately recognized it as an ostrakon. When we returned to brush the find spot, he picked up the second half from the small heap of dirt and sherds he had scraped up before he sought me out. In simple brushing we picked up several dozen sherds, mostly Iron II, but had to stop, as we had no permit to dig in that spot” (James Strange, forwarded to Orion, 2 Sept 1997

[<http://orion.mscc.huji.ac.il/orion/archives/1997b/msg00385.html>]).

⁴ Cross/Eshel 1997 (*IEJ*), 22, 26.

caves.”⁵ And again in 1998 in *Biblical Archaeology Review* Cross/Eshel stated, “the inscription [KhQ1] for the first time connects the site of Qumran directly with the documents known as the Dead Sea Scrolls”.⁶ The Cross/Eshel publication included hand drawings, transcriptions with commentary, and photographs of the two ostraca taken by James Henderson, an Oregon City, Oregon specialist in photography of faded pigments in rock art.

1.3. Subsequently in 1997 two other editions of Ostracon No. 1 appeared independently. The late Frederick Cryer of the University of Copenhagen, writing in the *Scandinavian Journal of the Old Testament*, argued that no word *yahad* existed on the ostracon and offered alternative readings.⁷ And Ada Yardeni of Hebrew University, Jerusalem, editor of the Dead Sea documents in Hebrew and Aramaic of volume 27 of the *Discoveries in the Judean Desert* (DJD) series (1997), published an edition of KhQ1 in *Israel Exploration Journal* with corrections of a number of the readings of Cross/Eshel.⁸ According to Yardeni line 8 read **וְכֹל אֵילָן אֹתָן**, “and every oth[er(?)] tree”. Yardeni presented a new drawing and transcription of Ostracon No. 1 and a magnified photograph of line 8. (Neither Cryer nor Yardeni dealt with Ostracon No. 2.) In 2000 Cross and Eshel published the two ostraca in DJD 36 essentially unchanged from their versions of 1997.⁹ A new hand drawing by Cross in DJD 36 replaced the former one of Cross/Eshel of 1997 and an excursus by Cross in DJD 36 defended the Cross/Eshel line 8 reading against Yardeni’s. The ostraca received their sigla, KhQ1 and KhQ2, in the DJD publication.

1.4. The present article offers fresh readings and analysis of KhQ1 and KhQ2. Sections 2-14 develop a new edition of KhQ1. First to be examined is line 8 of KhQ1, since that line has been the center of most interest. After that readings of all other lines of KhQ1 are studied leading to a reconstruction of the text in section 10. Sections 11-14 take up interpretive issues, the question of a relationship between KhQ1 and 1QS, and dating. Section 15 is a new edition of KhQ2. The readings were done from the black-and-white photographs of KhQ1 and KhQ2 published in *IEJ* 1997 and in DJD 36 in 2000; study of KhQ1 on public display in the Israel Museum in 1997; a color photograph of KhQ1 published by the Israel Museum in 1997, in A. Roitman, *A Day at Qumran*; and a set of black-and-white and color photographs of KhQ1 taken in 1997 by Bruce Zuckerman and Marilyn Lundberg (West Semitic Research).¹⁰ Also consulted was a photographic

⁵ Cross/Eshel 1997 (*IEJ*), 28.

⁶ Cross/Eshel, “The Missing Link”, 49.

⁷ The present article is dedicated to the memory of Frederick Cryer (1947-2002). F.H. Cryer, “The Qumran Conveyance: A Reply to F.M. Cross and E. Eshel”, *Scandinavian Journal of the Old Testament* 11 (1997), 232-40.

⁸ A. Yardeni, “A Draft of a Deed on an Ostracon from Khirbet Qumrân”, *Israel Exploration Journal* 47 (1997), 233-37. Also Yardeni, “Breaking the Missing Link”, *Biblical Archaeology Review* 24/3 (May-June 1998), 44-47.

⁹ F.M. Cross and E. Eshel, “KhQOstracon”, in P. Alexander *et al.*, *Miscellanea, Part I* (DJD 36; Oxford: Clarendon Press, 2000), 497-507.

¹⁰ The black-and-white photographs in *IEJ* and DJD 36 published by Cross/Eshel were done by James Henderson using a procedure invented by him for photographing rock art. See “The Henderson Cross Polarized Enhancement Procedure: A New Image Enhancement Process for Pictographs and Other Pigmented Artifacts”, at www.rit.edu/~mrppph/HendersonCPE.PDF. A photograph of KhQ1 either identical with the one of the original Cross/Eshel publication of *IEJ* and DJD 36 or similar to it also appears at Cross/Eshel, “The Missing Link” (*BAR*, 1998), 50. My inspection of the original of KhQ1 involved about two hours of study, with notes and drawings, of

enlargement of lines 7-8 of KhQ1 in Yardeni's *IEJ* article and another photograph of KhQ1 by Henderson discovered by accident, heretofore unnoticed and unremarked.¹¹ Cross is quoted by Philip Callaway in a letter of October 1997: "The photos published [of KhQ1/KhQ2, by Cross/Eshel] are the best that exist, and were done by a specialist. The actual ostrakon is much more difficult to read than the photo."¹² I agree with these statements of Cross. Citations of Cross/Eshel refer to the DJD 36 publication of 2000 throughout unless otherwise noted.

2. The non-existent "yahad" reading of KhQ1, line 8.

2.1. As noted, in the original publication of KhQ1 a connection between the ostrakon and the sect described in the Community Rule (1QS) was reported based on what Cross/Eshel claimed was a reading in KhQ1, line 8, of **וּכְמַלְוֹתָו לִיחֹדֵר**, "when he fulfills (his oath) to the Community". The Israel Museum in Jerusalem, where the ostrakon is housed, published a statement from Cross and Eshel which concluded: "This ostrakon is the first find from Khirbet Qumran that provides proof of the link between this site and the scrolls. Its discovery confirms that the site served as the community center of the sect."¹³ A press release announced that the ostrakon had disproved the theory of University of Chicago professor Norman Golb of a Jerusalem origin of the scrolls, mentioning Golb by name. At the international conference, "The Dead Sea Scrolls—Fifty Years after their discovery", held at the Israel Museum in Jerusalem in July 1997, Golb responded by displaying a photograph of the ostrakon and showing from it that the Cross/Eshel reading of line 8 was in error.¹⁴ Golb focused on the Cross/Eshel reading of the third letter from the end of line 8 as *yod*, which is essential to the "yahad" reading. In all published black-and-white photographs but especially clearly in the one displayed by Golb there is a vertical stroke with a foot to the left at the bottom, in the shape of a *nun* and bearing no resemblance to a *yod*. Without the *yod* the Cross/Eshel "yahad" reading collapses. Golb ended by noting (with permission) that both Joseph Naveh and Ada Yardeni also regarded the line 8 reading of Cross/Eshel as mistaken. However while in the view of many Golb had succeeded in showing that the reading of Cross/Eshel was not correct, the distinct issue of what line 8 does read remained unsolved until the publication of Yardeni. (Golb did not propose a reading of line 8.)

2.2. The 1997 study of Cryer in the *Scandinavian Journal of the Old Testament* also criticized the Cross/Eshel "yahad" reading and astutely saw an *aleph* in the fifth letter position of line 8, but

the ostrakon viewed through a glass case in Jerusalem in July 1997. I thank Marilyn Lundberg, Bruce Zuckerman, and the Israel Antiquities Authority for permitting me access to the photographs of KhQ1 in black-and-white and color taken by Zuckerman and Lundberg. One of the Zuckerman/Lundberg black-and-white photos was published in 1998 in *Biblical Archaeology Review* (Yardeni, "Breaking the Missing Link", 44); the others, including an excellent color photo, have never been published. The color photo published by the Israel Museum is at A. Roitman, *A Day at Qumran* (Jerusalem: Israel Museum, 1997), p. 36 of the Hebrew section.

¹¹ Henderson, Fig. 11 on page 10 of "A New Image Enhancement Procedure" (see note 10).

¹² Cross to Callaway, quoted at Callaway, "A Second Look at Ostrakon No. 1", 146 n. 1.

¹³ Cross/Eshel in Roitman, *A Day at Qumran*, 40.

¹⁴ I was present. An open comment from Golb of 31 Aug. 1997 shortly after this event can be viewed at <http://orion.mscc.huji.ac.il/orion/archives/1997b/msg00380.html>. See also N. Golb, "Qadmoniot and the 'Yahad' Claim", *Qumran Chronicle* 7 (1997), 171-73 (available at <http://www-oi.uchicago.edu/OI/PROJ/SCR/Yahad.html>).

Cryer's proposed line 8 reading was also incorrect. Cryer's reading was **וּכְמַלְאוּ לְנִאחֲזוּ**, “and when/if he completes taking”. Cryer commented: “The verb is *ml*’, ‘fill’; here probably inf. cs. plus suffix, all rendered either temporally or factually conditional by the preceding *k*. The ’Aleph is faint and awkward, but seems to be present on the high-definition image”.¹⁵ This was followed by what Cryer characterized as a niphil infinitive construct form **נִאחֲזוּ** from the verb **אָחַז**. The problems with Cryer's solution are: (a) A form *nqtl* for niphil infinitive construct is wholly unattested—not simply for **אָחַז** but for any verb in the entire Hebrew lexicon. Cryer argued that it was plausible that the form existed even though it is not attested in a single known case, but given the extent of the database this seems highly unlikely.¹⁶ (b) The *kaph* prefix of **וּכְמַלְאוּ** is implausible (see below). (c) The infinitive construct **וּכְמַלְאוּ** has no object for the verb and therefore is also implausible (see below). And finally (d) the reading of the #3 letter of the line as *mem* is incorrect palaeographically, yet without that letter the reading of the first word collapses. The basic problem with Cryer's reading is he accepted Cross/Eshel's first word of line 8 as correct and challenged only the second. In fact both words of Cross/Eshel's line 8 reading are incorrect.

2.3. The correct reading of line 8 was shown by Yardeni in 1997 in *Israel Exploration Journal* (except for the last letter of line 8; discussed below). Yardeni read **וְכֹל אֵילָן אֲחִי**, “and every oth[er(?) tree”. Yardeni cited parallels from Dead Sea documentary texts. As cited and rendered by Yardeni (without uncertainty markings in the letter readings):

Mur 30.18 (deed of sale, Heb.)

הַתְּאֵנִים הַזֵּיתִים הָעֵץ

“the fig trees, the olive trees, the tree(s)”

Naḥal Ḥever 44.2 (deed of lease, Heb.)

וְתִכַּל אֵילָן שְׁבָהֶם

“and every tree within them”

Naḥal Ḥever 46.4 (deed of lease, Heb.)

תְּדַקְלִים וְתִשְׂאָר אֵילָן שְׁבָהֶם

“the palms, and the rest of the tree(s) within them”

Naḥal Ḥever 2.6 (deed of sale, Nabataean)

וְתַמְרֵי וְשִׁקְמִין וְאֵילָן

“and palms and sycamores and tree(s)”

Naḥal Ḥever 7.48 (deed of gift, Aramaic)

¹⁵ Cryer 1997 (*SJOT*), 235 n. 16.

¹⁶ Cryer 1997 (*SJOT*), 236 n. 17, citing a previous note he had published in *Scandinavian Journal of the Old Testament* 6 (1992), 211 n. 36. In the 1992 note Cryer suggested that *nqtl* for the niphil infinitive absolute, which is attested, was an artificial written form created to assist readers (i.e. not taken from spoken speech). In the 1997 note Cryer suggested it was plausible that the same phenomenon (an identical *nqtl* form) could have occurred for the same reasons in the infinitive construct as well, even though it is unattested.

וכות כל תמרין ואילן

“and, in like manner, all palms and tree(s)”

Yardeni concluded, “The reading *wkwl 'yln 'h[r* in Line 8 of the Qumran ostrakon thus fits perfectly into the context.”¹⁷ Yardeni was convincing in her reading of the third letter of line 8 as *waw* (not Cross/Eshel’s uncertain *mem*); her reading of the structure of the fourth letter, *lamed*; her identification of the fifth letter as *aleph* (not Cross/Eshel’s *waw* plus *taw*); the eighth letter as *nun* (not Cross/Eshel’s *yod*); and the N-shaped ninth letter as *aleph* (not Cross/Eshel’s anomalous *het*).¹⁸ With undisputed readings of *waw* (#1), *kaph* (#2), and *lamed* (#4) at the start of the line (i.e. וכל), the word וכול following a list of types of trees or fruit in line 7 is an obvious reading. And if so, the next letters, אילן, give the known “tree” word. Or the reasoning can work in reverse: once אילן is recognized in letters #5-#8, the preceding וכול in turn emerges easily, proceeding from and connecting the trees of line 7. As for the *plene* spelling of וכול, in DJD 27 examples of *plene* versus defective orthography in uses of *waw* or *yod* are cited as a routine feature of the Dead Sea documentary texts (texts which in any case display a general lack of uniformity in orthography); כול in KhQ1 is not unexpected.¹⁹ (For example כול appears at XHev/Se 8a, line 7, a text which,

¹⁷ Yardeni 1997 (*IEJ*), 233-34.

¹⁸ Yardeni: “Palaeographically, the medial *waw* in *wkwl* is composed of two strokes, while the additional stroke appearing in the editors’ [1997] drawing of this letter—which changes it into a *mem*—is actually part of the right downstroke of the *lamed* which touches it. The ‘roof’ of the *lamed* is damaged by a scratch running down near its upper right-hand corner and is actually wider than it appears in that drawing. The *lamed* is followed by an *alef*, drawn by the editors as two letters: a small *waw* and a *taw*. The construction of this *alef*, the first letter of the word *'yln*, is similar to other occurrences of this letter in the ostrakon. It is composed of three strokes: a right downstroke somewhat slanting to the left, a diagonal slanting downward to the right which occasionally extends beyond its meeting point with the right downstroke, and a left downstroke which is either vertical or somewhat slanting to the left. In the case under discussion, the diagonal of the *alef* is damaged near its crossing point with the right downstroke, leading to the editors’ interpretation of the form as two letters. The following letter, read as *waw* by the editors, is actually the *yod* preceding the *lamed* of the word *'yln* (*waw* and *yod* are identical in structure). Although very faint on the ostrakon, the *nun* following the *lamed* is clear in the photograph. Its form resembles a large medial *nun*, which curves to the left at its bottom. There is no stroke descending leftward from the top of the downstroke as drawn by the editors. The *nun* at the end of *ntn* in Line 2 also seems to curve to the left at its bottom ... The letter which the editors read as a cursive *het* is obviously an *alef*, which apparently begins a new word. The remains of the last letter surviving in this line may be the right part of a *het* (the letter may be compared to *het* in the word *byrhw* in Line 2); if so, the reconstructed word may perhaps be *'hr* ‘other’ ... In my view, the editors’ hand copy is based on a mistaken reading of the text and is, therefore, misleading, especially to those unfamiliar with the variety of letter forms, as well as the formulae, in the Hebrew and Aramaic deeds from the Judaean desert. Accordingly, their transcription and translation cannot be accepted. This means, in turn, that the identity of those who wrote the Qumrân scrolls and their place of residence cannot be determined on the basis of the present ostrakon” (Yardeni 1997 [*IEJ*], 234-36).

¹⁹ Yardeni: “Two linguistic phenomena stand out in the documents, in spite of the relative paucity of material: (a) variant forms of individual words and particles and (b) lack of uniformity in orthography. Both of these are characteristics of a change-over period in the development of the

like KhQ1, also has a defective spelling,]תהנ, a little later.) Yardeni's reading of the two fully visible words of line 8 is clearly correct.

2.4. To the astonishment of many Cross/Eshel published their original transcription of the ostrakon in DJD 36 in 2000 without accepting a single one of Yardeni's corrections, neither in line 8 nor any other line. In the bibliographic list termed "previous discussion" which appears at the start of KhQ1, Cross/Eshel cite their own journal publications and popular presentations in Roitman 1997 and *Biblical Archaeology Review*. But oddly, neither Yardeni's nor Cryer's studies are listed in this bibliographic list. There is an excursus of Cross appended to the end of the presentation of KhQ1 of DJD 36 addressing Yardeni's reading, and the studies of Yardeni and Cryer are alluded to in the main discussion.²⁰ But neither Yardeni nor Cryer are in the starting listing of "previous discussion" where it is customary in DJD editions to list previously published scholarly studies of texts.

2.5. Cross/Eshel's conservatism in rejecting changes to their transcription did not apply only to 100 percent of the corrections of Yardeni. In several cases the new hand drawing of KhQ1 of Cross in DJD 36 supports readings of Yardeni which differ from the 1997 Cross/Eshel transcription. But it made no difference: in not a single case is a change in the drawing of Cross in DJD 36 reflected in the Cross/Eshel transcription in DJD 36. That is, not even new information from Cross reporting what his eyes were seeing was enough to budge Cross/Eshel from their 1997 transcription. These instances are:

- At line 2, #8 Cross/Eshel 1997 (*IEJ*) mistakenly transcribed a final-form *nun*. Yardeni 1997 correctly drew and transcribed a medial *nun*. In DJD 36 Cross also correctly has a medial *nun*. But the Cross/Eshel transcription in DJD 36 repeats the erroneous final-form *nun*.
- At the end of line 11 Cross/Eshel 1997 had nothing in their drawing. In their transcription they reported an unidentified letter with this comment: "near the edge of the sherd there appears to be a *mem* or a *kaph*. No reading is possible" (p. 25). Yardeni 1997 drew the shape of a *tsade* in this position, transcribed as an uncertain *tsade*. In DJD 36 Cross has drawn a clear *tsade*, and Cross/Eshel now also comment in DJD 36: "near the left edge [of line 11] perhaps an awkward *šade*". However the Cross/Eshel transcription of this letter in DJD 36 remains unchanged (an unidentified letter).
- At line 13, #2 Cross/Eshel 1997 drew and transcribed nothing but commented: "Line 13 reads ל]גן ... We may reconstruct ל]גן?גן, 'to guard' or 'to garden' ... Another possible reconstruction is ל]גן?ח]גן" (p. 25). Yardeni 1997 drew in the #2 position a *lamed*-like ascender plus what looks like part of a *lamed*'s hook, transcribed as an unidentified letter. In DJD 36 Cross has drawn a clear, unmistakable *lamed* in the #2 position. But in their only change of a letter in transcription between *IEJ* in 1997 and DJD 36 in 2000,

language. The following examples may be noted. ... [8th of 11 listed] Plene vs. defective orthography in use of *waw* or *yod*" (Yardeni, DJD 27, 11 and 13).

²⁰ Cross/Eshel refer to their line 8 transcription as "the preferred reading among those suggested" (DJD 36, 499), and note "a rival reading is taken up in the Excursus" (p. 503 n. 32). A point in Cryer's study (his argument that]נתן means sale, not gift) is objected to in a footnote (p. 501 n. 16).

Cross/Eshel now transcribe line 13, #2 as an uncertain *gimel* (!) with the comment: “after *lamed* [#1] appears a damaged *gimel* [#2]” (p. 500)—even though what Cross draws for #2 in DJD 36 is a crystal clear *lamed* and bears not the slightest resemblance to a *gimel*.

- At line 14, #1-4 Cross/Eshel 1997 transcribed חסדי, “Ḥisday”. The Cross/Eshel drawing in 1997 agrees with this transcription at letters #2, #3, and #4. Yardeni 1997 drew unidentifiable traces at #2, #3, and #4 transcribed as unidentified letters. In DJD 36 Cross has now also drawn unidentifiable traces at #2, #3, and #4 (no longer readable as letters as in the former drawing). Nevertheless the Cross/Eshel transcription in DJD 36 remains חסדי unaltered (letters #2 and #3 still reported as certain).
- At line 14, #7 Cross/Eshel 1997 drew a *dalet*, transcribed as a certain *dalet*. Yardeni 1997 drew an unidentifiable trace, transcribed as unidentified. In DJD 36 Cross has drawn for this letter a lightly outlined shape of a *resh* (no right ear), yet the Cross/Eshel transcription of this letter in DJD 36 is unchanged from 1997, still reporting a certain reading of *dalet* even though Cross’s drawing in DJD 36 suggests a different letter.

2.6. The Cross/Eshel transcription in DJD 36 in 2000 was not *exactly* the same as before. In 1997 Cross/Eshel transcribed all letters of their reading ליהד in line 8 as certain. They characterized their readings of these letters in 1997 as “without serious objection”.²¹ In 1998 they wrote: “even though the bottom of the [final line 8] letter has been cut off in our ostracon, it can *only* be a dalet. If that is so, no plausible reading other than Yahad can be suggested”.²² But in the DJD 36 publication of 2000 these claims are omitted, and three of those final four letters, the *yod*, *het*, and *dalet*, are now marked as palaeographically uncertain: וכמליותו ליהד. The excursus from Cross characterizes Yardeni’s study as having proposed “new plausible readings”.²³ At the same time Cross/Eshel no longer claim three out of four letters of their own reading of ליהד are palaeographically certain. Cross and Eshel maintain now only that their line 8 reading “is to be preferred” over Yardeni’s reading.²⁴ Cross explains:

Yardeni reads וכול at the beginning of the line [8]. We read a *mem* not *waw* as the third letter. Clearly, on both our photographs and hers, there is a head on the letter, not part of the *lamed*. We read the entire phrase as *wkmlwtw lyhd*, ‘when he fulfills (his oath) to the Community’. Yardeni reads *wkwl ’yln ’h[r]*. She runs together *waw* and *taw* to make an anomalous *’alep*. In fact, the two letters do not touch. The so-called *’alep* has a high right stroke that does not touch the putative diagonal as in all other *’aleps* in these texts. Even more awkward is the unusually broad space between *yod* (Yardeni) or *waw* (Cross and Eshel) and the following *lamed*. It resembles a word-space. Yardeni reads *kwl ’yln ’h[r]*, ‘and every oth[er(?) tree’. The word *’ilân* is questionable. These ostraca are written in ‘biblicizing’ Qumran Hebrew, but *’ilân* is an Aramaic form, cognate with Hebrew *’elôn*. To

²¹ Cross/Eshel 1997 (*IEJ*), 25.

²² Cross/Eshel, “The Missing Link”, 52.

²³ Cross, DJD 36, 505.

²⁴ Cross, DJD 36, 507; Cross/Eshel, DJD 36, 499.

be sure, *ʾlān* is an Aramaic loan-word (or loan-form) in Middle Hebrew, as early as the era of the Second Revolt. However, it is not expected here.²⁵

2.7. None of these points of Cross stand. To begin with, Cross's claim that the word **אילנ** is "questionable ... not expected here" in line 8 of KhQ1 is simply astonishing. As noted above, Yardeni cited numerous uses of the word **אילנ** in other Dead Sea documentary texts, two in Hebrew. One of the texts in Hebrew, NahḤev 44.2, reads **ותכל אילן שבהם**, "and every tree within them", the identical sequence and sense as **וכול אילנ** in KhQ1. **אילנ** is considered a classical Hebrew word by the editors of *The Dictionary of Classical Hebrew* published by Sheffield Academic Press, who give it an entry and cite still another use in a Dead Sea documentary text in Hebrew: 5/6 Hēv13BA 46.9, **של הירק ושל האילן**, "of the vegetable(s) and of the tree(s)".²⁶ The Qumran ostrakon, KhQ1, speaks of "figs" in the immediately preceding line 7 according to both Cross/Eshel and Yardeni, followed by what Cross/Eshel read as "olives" and Yardeni suggests is "palms". Therefore to find *continuing in the next line* (line 8) a routine expression for "trees" attested in identical contexts in other Dead Sea documentary texts in Hebrew from the same era surely *is* expected.

2.8. Cross's suggestion in DJD 36 that the Qumran ostraca use a different kind of language than other Dead Sea texts ("these ostraca are written in 'biblicizing' Qumran Hebrew") also is asserted without any disclosed or actual basis or evidence. In 1998 Cross/Eshel wrote in *Biblical Archaeology Review*: "This [line 8] reading (as well as the Qumran dialect of the ostrakon) is a staggering blow to the many theorists who do not accept the Qumran settlement as that of a communal sect holding common property."²⁷ But again in that article no explanation was given for the claim that a "Qumran dialect" is reflected in the ostrakon. One has to go back to the original Cross/Eshel publication of 1997 to find the only example claimed by Cross/Eshel in support of this statement: it is the mistaken reading of line 8 itself.²⁸ That is, the mistaken reading is cited as a reason to expect the mistaken reading. Once the incorrect reading of line 8 is removed from consideration there is nothing even claimed by Cross/Eshel in support of a notion of a unique "biblicizing" Hebrew as the language of KhQ1/KhQ2, as distinct from the alternative that the language of these ostraca is simply Hebrew, contemporary and idiomatic, of the kind in use in other Dead Sea documentary texts of the era.

2.9. As for the line's third letter, Yardeni's reading of **וכול** is certainly correct rather than Cross/Eshel's **וכמל**. What Cross/Eshel read as *mem* is unparalleled as a *mem* structure anywhere in KhQ1 or KhQ2. The space between the *yod* and *lamed* which Cross describes as "unusually broad ... resembles a word space" is no different than the spacing between the *yod* and *resh* of **ירדהו** of

²⁵ Cross, DJD 36, 506.

²⁶ D.J.A. Clines (ed.), *The Dictionary of Classical Hebrew* (Vol. 1, א; Sheffield: Sheffield Academic Press, 1993), 213.

²⁷ Cross/Eshel, "The Missing Link", 52.

²⁸ *Cross/Eshel*: "**כמל**אותו" is written [in line 8] for **כמל**אותו ... The use of this form and of this construction suggests strongly that the writer of the document uses the Hebrew of the Qumrân community with its close biblical ties" (1997 [IEJ], 24 and n. 36).

line 2, or the *kaph* and second *waw* of וכול of line 8. All of these letter spacings are within the range of routine writing in KhQ1.²⁹

2.10. On reading the N-shaped letter of line 8, #9, a routine *aleph*, as *het*, Cross writes:

The penultimate letter of the line is unlike the other *hets* on the ostrakon. It is cursive while the others are more formal. There are, however, many parallels to this cursive *het* in contemporary inscriptions. This example is easily confused with *'alep*. The two can be distinguished, however, on the basis of the direction of the strokes. *'Alep* is penned with the middle diagonal first, moving on to the left leg and then to the right arm. The *het* here is drawn right leg first, then an oblique stroke ascending from the bottom to the top of the left downstroke. Despite the difference in the way the letters are made, they often resemble each other, especially when the lower parts of the legs are broken off the ostrakon.³⁰

It is not clear how Cross can know line 8, #9 was penned with the right leg drawn first as opposed to last in *alephs*. Compare the *alephs* at line 3, #2; line 6, #2; and line 7, #4. The present letter at line 8, #9 is indistinguishable from those *alephs* in structure; that is because line 8, #9 is another *aleph*. The same words of Cross could be applied equally well to e.g. line 6, #2, to argue that that *aleph* is a *het*, except in that case context confirms that letter is *aleph* (as Cross/Eshel correctly read). The line 8, #9 letter is in agreement with the N-shape of all nine other cases of *aleph* in KhQ1 and both of two cases of *aleph* in KhQ2.³¹ On the other hand, as Cross/Eshel note, there is no case of an N-shaped *het* elsewhere in either of the ostraca.³² Cross/Eshel cite examples of N-shaped *hets* in other texts but the way *het* is written in KhQ1/KhQ2 is known and it is not N-shaped.³³ To read a letter in the routine shape of an *aleph*, which is always N-shaped in these ostraca, as another letter, *het*, which never is, is unwarranted.

2.11. For the preceding letter #8, Cross draws a massive tent-shaped blot of ink and defends reading a *yod*.

The *yod* (third letter from the left) resembles an inverted 'V'. If the right leg were not broken off at the base, it would be longer than the left leg. The legs are blotted together at

²⁹ As for the distinct issue of the absence of word-separation spacings in line 8 between וכול and אילנ, and אילנ and the word which follows it, compare similar lack of word-separation spacings in line 4 between -המקומהל; line 6 between ואתחמו הבית; and line 10 between לו את. There are word separations in KhQ1 in lines 1, 2, 3, 5, and 7 but not in line 4, most of line 6, and line 8. In KhQ2 word separations are clear in line 2 but are not in lines 3 and 4. Naveh comments on how common this variability is: "in the Copper Scroll which was written in the vulgar semi-formal style, as well as in the cursive documents from Murabba'at, many spaces were omitted; but these are the results of neglect rather than *scriptio continua*" (J. Naveh, "Word Division in West Semitic Writing", *Israel Exploration Journal* 23 [1993], 206-8 at 207).

³⁰ Cross, DJD 36, 506.

³¹ *Alephs* in KhQ1: line 3, #2; line 4, #1; line 5, #1; line 6, #2; line 7, #2, #4; line 8, #5, #9; line 10, #3; line 11, #2. In KhQ2: line 1, #2 and line 4, #6.

³² Cross/Eshel: "the *het* of *lyhd* is 'N'-shaped, a cursive form ... not used elsewhere in this ostrakon" (DJD 36, 499).

³³ *Hets* in KhQ1: line 2, #4, #9; line 3, #10 (?); line 6, #4. In KhQ2: line 2, #2.

the top but begin to diverge at the bottom just before the ostrakon breaks off. Yardeni does not see the upper left side of the letter, but draws a medial *nun*. In her photograph, the upper left portion of the letter is visible, but much dimmer than in our earlier pictures, and on the ostrakon when we examined it. I suspect that the repeated wetting of the ostrakon to aid in its photography has in fact faded the ink here. The *yod* that we see is not greatly different from the *yod* in *šty*m (line 1), and the *yod* in *hny* (line 2).³⁴

All published black-and-white photographs show at line 8, #8 a vertical stroke and a left foot at the bottom, a routine *nun*. This is visible in the photograph published by Cross/Eshel in *IEJ* and in DJD 36, as well as the photograph accompanying the article of Cross/Eshel in *Biblical Archaeology Review* in 1998. Cross/Eshel have never acknowledged the existence of the base horizontal foot in either their drawings or discussions, which is puzzling since it is clearly visible. If the ink *were* as Cross has drawn and described, no other *yod* on the ostrakon is remotely comparable in looking that huge. Cross's drawing in DJD 36 shows the downward left stroke of the alleged *yod* running practically down to the line's base level, curving out farther to the left and still going as the ostrakon breaks off—but no actual *yod* in KhQ1 or KhQ2 does this. Contrary to Cross's claim, the *yods* in lines 1 and 2 do not resemble the shape Cross draws at line 8, #8. In fact there is no inverted-V appearance of any *yod* on either ostrakon, KhQ1 or KhQ2.³⁵ Also, as noted later there is no graphically discernible difference between *waw* and *yod* in this text (see below at section 4.4). In their 1997 hand drawing Cross/Eshel drew at line 8, #8 only the lined edge of an inverted-V or tent-like shape (transcribed as *yod*). In DJD 36 Cross has drawn the same inverted-V shape now filled in solidly with ink, rather than only the outline of the tent. Cross's DJD 36 drawing now both includes and removes from perception the foot of the *nun*, since the solid, filled-in tent includes the foot.

2.12. Cross's claim in support of the *yod* reading that more of the ink was visible in the photograph published by Cross/Eshel in 1997, and in the ostrakon itself, prior to wetting of the ostrakon for further photography, unintentionally provides an insight into what went wrong. Indeed there is a difference in the photographs but for a different reason than suggested by Cross. A comparison of a photograph of Henderson used by Cross/Eshel and a photograph of Zuckerman/Lundberg, positioned next to one another in the Yardeni *Biblical Archaeology Review* article, makes clear that Cross is commenting on slightly different contrast levels in the photographs having nothing to do with wetting or removal of ink. In the ostrakon itself and the color photos the line 8, #8 letter is virtually invisible. The earliest known photograph of the ostrakon—the Henderson black-and-white photo published by Cross/Eshel in 1997—shows the *nun* and its distinctive left foot. In the Henderson photograph published in the 1998 Cross/Eshel *Biblical Archaeology Review* article the *nun* is even clearer. Yet the Henderson photograph also shows a tent-like shading in this position which is not letter but which Cross/Eshel read as the letter. This shading at line 8, #8 should be compared to similar shadings in the same photograph surrounding the *waw* of line 9, #1, the letter at

³⁴ Cross, DJD 36, 507.

³⁵ Neither the drawing of Cross nor that of Yardeni accurately represents the shape of the *yod* of line 2, #2. Two blots not part of the letter but touching the letter on either side distort the letter's appearance. These two blots are comparable to two others of the same kind above the upper left of the *yod*. The left one of the two marks which touch the *yod* was mistakenly seen and drawn by both Cross and Yardeni as part of the left stroke of the *yod*, giving an inverted-V appearance. In fact the actual letter is a routine *yod* like that of line 6, #9, or line 7, #6. An inverted-V in line 4 of KhQ2 read by Cross/Eshel as *yod* is an incorrect reading; that letter is an *aleph* (see discussion of KhQ2 below).

line 5, #7, and in the margin to the right of the start of line 5. The letters in the Cross/Eshel (Henderson) photo are darker and easier to read than in the Zuckerman/Lundberg photo due to enhancement, which has had the effect of making these non-letter shadings also appear darker in the Henderson photos.³⁶ Cross/Eshel misread a shading at line 8, #8 in their black-and-white photographs as if the shading was the letter, whereas the shading is an artifact of the enhancement process. In any case the strokes of the *nun* within this shading are clear and visible in all black-and-white photographs whether from Henderson or Zuckerman/Lundberg.

2.13. Furthermore, the Cross/Eshel reading of **וּכְמִלּוֹתוֹ** has a *kaph* prefixing an infinitive construct instead of an expected *bet* prefix for “when”. Although it has not been remarked in previous discussions of the ostrakon, such a use of *kaph* is nonexistent in Qumran texts or any other known text of the era, and therefore it would be highly odd for it to appear here. Cross/Eshel wrote in *IEJ* in 1997: “Qumrân Hebrew ... is a continuation of Biblical Hebrew ... and the biblical use of *be* and presumably *ke* plus an infinitive persists”.³⁷ But Cross/Eshel cited no example in a Qumran text for such a use of *kaph*. In *DJD* 36 the “presumably” qualifier is omitted and Cross/Eshel state more firmly and wrongly: “the biblical use of *be-* and *ke-* plus an infinitive persists”.³⁸ But still Cross/Eshel give no contemporary example of the structure with **-כ** (*kaph*) which they propose to read in KhQ1. In fact, according to Elisha Qimron’s study, *The Hebrew of the Dead Sea Scrolls*, “forms with *kaf* (**וּכְמִלּוֹ** etc. - temporal) do not occur”.³⁹ The expected preposition for this structure and the one used in the 1QS parallels cited by Cross/Eshel is the ubiquitous *bet* preposition (**-ב**), not *kaph*.

2.14. Apart from the *kaph/bet* issue, the Cross/Eshel line 8 reading also is not grammatical. **וּכְמִלּוֹתוֹ לִיְחִיד** has the literal sense: “when he fulfills/completes (to) (the) community”, with no direct object. Cross/Eshel read **וּכְמִלּוֹתוֹ** “either as a *Qal* infinitive construct or, more likely, as the *Pi‘el* infinitive construct of the root **מלא** with the preposition *k-* used in a temporal sense”.⁴⁰ In either case **מליות** should have a direct object, but it does not in Cross/Eshel’s reading. The late Shelomo Morag, the noted semitist, in public oral comment at the 1997 Jerusalem conference following Golb’s lecture stated that the syntax of the Cross/Eshel reading, “and when he completes *yahad*”, is not possible in any known form of Hebrew.

2.15. Only in the last letter of line 8 is there reason for contesting Yardeni’s line 8 reading, at the one point where Yardeni characterized her reading of line 8 as uncertain. Yardeni read the final word as **אח**, “perhaps” **אחר**, with a question mark (**אח**), “and every oth[er(?) tree”.⁴¹ The view here is that the last letter of line 8, where only the top of the letter is visible, was read correctly by Cross/Eshel as *dalet*. Cross comments:

³⁶ Compare Henderson on digital enhancement at “A New Image Enhancement Procedure”, 5, 9-10.

³⁷ Cross/Eshel, 1997 (*IEJ*), 24 n. 36.

³⁸ Cross/Eshel, *DJD* 36, 503 n. 34.

³⁹ E. Qimron, *Hebrew of the Dead Sea Scrolls* (Atlanta: Scholars Press, 1986), 72 n. 12.

⁴⁰ Cross/Eshel, *DJD* 36, 503.

⁴¹ For Yardeni’s “perhaps” suggestion, see note 18.

A key letter is the *dalet* at the end of line 8. Compare the example in line 12. Yardeni suggests a *het*. However, I believe that enough remains of the left side of the letter to preclude either *het* or *he* ... The final letter of the line is *dalet*.⁴²

Although this letter is a very close call palaeographically, Cross appears here to be correct. If the letter is *dalet*, there would not be a vertical line below the visible roof at the left, whereas with a *het* there would be. In the color photos, whereas a stroke goes above the roof in agreement with *dalet*, *kaph*, and *het*, there appears to be no stroke continuing below the roof. If this visual analysis is correct the letter can be only *dalet* or *kaph*—*het* is excluded. In addition, as discussed later, most *hets* in KhQ1 do not have left and right verticals above the crossbar at all, and in no case in KhQ1 as prominently as the present letter, although a *het* of KhQ2 (line 2) does and the *het* of KhQ1, line 2, #4 comes close. Therefore this second point is not decisive, but the first point does appear to be. This has no effect on the first two words of Yardeni's line 8 reading (וכול אילנ) however; those two words are correct whether or not the third word is identified correctly, incorrectly, or not at all. Enough of the third word is identified, however, that the indicated reading of line 8 can be suggested: כול אילנ אד[מה], “and all trees of the ea[rth]”, on analogy with 4Q418 Instruction^d 107.5, כול צמחי ארמה, “all sprouting plants of the earth”.⁴³

2.16. To conclude, the Cross/Eshel reading of line 8 is palaeographically incorrect, syntactically odd, and not an expression which occurs in any known text. Yardeni's reading, on the other hand, continues from line 7 about trees with a reading in line 8 about trees using expected wording familiar from other Dead Sea documentary texts. It is palaeographically correct (for all but the final letter of the line) and grammatically routine. It must be emphasized that there is no reason from context calling for the unusual reading of Cross/Eshel. Although the public may be under the impression that the reading is disputed, the fact is not a single other scholar experienced with editing Qumran texts has defended the Cross/Eshel line 8 reading after the publication of Yardeni's correction in 1997.⁴⁴ The confusion is caused entirely by the continued advocacy by Cross and Eshel of their mistaken reading and their publication of it in DJD 36 where it is now immortalized for all time, three years after the correction was published. After this necessary opening focus on line 8, attention will now turn to the other lines.

⁴² Cross, DJD 36, 506.

⁴³ The Sheffield *Dictionary of Classical Hebrew* defines אֵילָן as “n. [m.] tree—used collectively”.

⁴⁴ This statement is supported not only by the lack of a known published statement from an editor of a Dead Sea text endorsing the Cross/Eshel line 8 reading, but also anecdotally. Compare the comment of Cryer in 1997: “an unofficial canvass performed by myself of a fair number of [Dead Sea scroll] scholars who attended the 50th anniversary celebration and stock-taking in Jerusalem in July 20-25, 1997, failed to discover *any* [Cryer's emphasis] who would have read line 8 as did Cross and Eshel” (1997 [*SJOT*], 239). I know of no information in the years since then that would argue against the accuracy of a comparable statement said today.

3. Lines 1-3.

3.1. *The name of the giver: Honi.* The proper name following **נתנ** in line 2 is read by Cross/Eshel and Cryer certainly as **הני** and Yardeni uncertainly as **הני**. The final two letters as *nun* and *yod/waw* are clear. The blurred first letter in the available photographs seems too unclear for certainty, but Cross/Eshel's *het* yielding the known name must be correct.⁴⁵ Although previously unrecognized, the same proper name may appear also in KhQ2, with *plene* spelling: **הוןני** in line 2.

3.2. *The recipient: Eleazar.* There is no dispute over the name of the recipient; **לאלעזר**, “to Eleazar”, is a clear reading beginning line 3. The second word of line 3, introducing the patronymic of the recipient, is read correctly in all editions as **בן**, with *bet* and final-form *nun* joined with a ligature, not **בת** which would indicate a woman recipient—despite a palaeographic similarity in the way **בן** and **בת** are written and an expectation from other known Jewish deeds of gift that the recipient might be a woman.⁴⁶

⁴⁵ The first letter is not likely to be a blurred *aleph* giving **אני**, “I”, because that would disagree with the third-person form of **נתנ**. Nor is it likely to be a blurred *he*, since **הני** is not known as a proper name, but a proper name is expected following **נתנ**. To Cross/Eshel's note that both *plene* and non-*plene* spellings of the names **הני/הוני** occur in Mur 30 as read by Milik can be added **הני** read by Puech in the Tomb of Jason inscriptions (E. Puech, “Inscriptions funéraires Palestiniennes: Tombeau de Jason et ossuaries”, *Revue Biblique* 90 [1983], 481-533 at 488).

⁴⁶ Cross/Eshel note that in all other known cases of Jewish deeds of gift the recipients are women. *Cross/Eshel*: “Three other deeds of gift from the Roman period have been found in the Judaean Desert ... Jewish deeds from antiquity were written only to female members of a family (wives or daughters) who needed such deeds in order to receive family property. They were not counted legal heirs. The ostracon from Qumran is the first Jewish deed of gift found in Israel in which the recipient is a man” (DJD 36, 503-4). The visible ink at KhQ1, line 3, #7-8 has a *bet* followed by a vertical stroke and another stroke curving or bending down from the top of the vertical to the base of the preceding *bet*. In light of the recipients of deeds of gift usually being women, it must be asked whether the reading is actually **בת**, not **בן** with a ligature, and the recipient of KhQ1 also a woman. Palaeographically, this question might be reinforced by the observation that other *nuns* in word-final position in KhQ1 and KhQ2 are persistently medial, not final, forms (compare **נתנ** and **אילנ** in lines 2 and 8 of KhQ1, and **בנ** in line 3 of KhQ2), and in no other instance in KhQ1/KhQ2 is there a ligature. Palaeographically, *taw* in these ostraca is written with a vertical stroke and then a slanted stroke which starts at or near the top of the vertical stroke, going out very slightly horizontally to the right and then angling downward. But sometimes, as in KhQ1, line 6, #3, the angle downward looks like it starts immediately. Note that the preceding masculine proper name in line 3, **לאלעזר**, “to/of Eleazar”, is not proof *against* reading the next word as **בת**, since the antecedent of **בת** could be in the lacuna at the end of line 2. Compare the following word orders from other Dead Sea texts:

Deed of sale of land: Mur 30 i 6 and ii 25-26 (Heb., as read by Milik in P. Benoit, J.J. Milik, and R. de Vaux [eds], *Les Grottes de Muraba'at* [DJD II; Oxford: Clarendon Press, 1961], 145):

3.3. The proper name at the end of line 3, the name of the father of Eleazar, is difficult to decipher except for the first letter which is without dispute *nun*. However the Cross/Eshel reading]גחמני, “Naḥamani”, must be wrong; so also is the reading of Cryer, גחשני, “Naḥšony”. (Yardeni suggests no readings of letters after the *nun*.) The *nun-yod* combination read by Cross/Eshel and Cryer at the

ואני שלום אשת דוסתס בת חני בר יהונתן

“and I, Salome, wife of *Dositheus*, daughter of Ḥoni son of Yehonathan...”

ואני שלום אשתו של דוסתס זה בת חוני בר יהונתן

“and I, Salome, his wife which is to *this Dositheus*, daughter of Ḥoni son of Yehonathan...”

Deed of gift: *Papyrus Yadin 7*, line 3 (Aram., from Y. Yadin, J.C. Greenfield, and A. Yardeni, “A Deed of Gift in Aramaic Found in Naḥal Ḥever: *Papyrus Yadin 7*”, *Eretz-Israel* 25 (1996), 383-403 [Heb.]):

מרים אנתתי ברת יוסף בר מנשה

“Give and confirm, of my own free will ... a gift in perpetuity ... I, Simon the son of Menahem, who lives in Maḥoza, to you, Miriam wife of *me*, daughter of Joseph son of Manasseh, all that I possess ... I give to you...”

Deed of sale of land: XḤev/Se 8a, line 12 (Aram., as read by Yardeni in H.M. Cotton and A. Yardeni, *Aramaic, Hebrew and Greek Documentary Texts from Naḥal Ḥever and Other Sites* [DJD 27; Oxford: Clarendon Press, 1997], 34-37):

ואנה שלם ברת שמעון אננתת |חדר דנה

“I, Salome, daughter of *Simeon*, wife of this Hadad...”

By analogy, lines 2-3 might be proposed to run something like this:

בירחו נתנ חני בן <PN> ל <PN> אשה

לאלעזר בת נ]...

“in Jericho, Honi the s[on of <PN>] gave [to <PN>, wife] to *Eleazar*, daughter of <PN>[_____]”

However a reading of בת in line 3 must be excluded for the following reasons. First, the downstroke of the second letter is lengthy, longer than expected for a left vertical of a *taw* but in agreement with a final *nun*. Second, there is a lack of a “hump” or “angle” in the slanting stroke which otherwise characterizes the start of the right stroke of *taw*, whereas this line's stroke and angle is consistent with being a ligature. Third, the sloping line touches the preceding *bet* in agreement with its being a ligature, whereas usually letters in these ostraca do not touch. Fourth, לאלעזר preceding בת would mean the giver to the woman would be a man other than her father or husband, but who else would it be? Other deeds of gift are from men to wives or daughters. Fifth, it is questionable that reconstructions such as <PN> אשה ל- or <PN> אשה אשר ל- at the end of line 2 are natural as ways by which the sense “wife” would be expressed. And sixth, a sporadic appearance of בן with a ligature is plausible based on comparative parallels from Dead Sea documentary texts, e.g. Yardeni in *The Book of Hebrew Script: History, Palaeography, Script Styles, Calligraphy & Design* (Jerusalem: Carta, 1997), 188, notes that common or short words such as בר and מן are frequently ligatured in cursive; these are seen in passing in the documentary texts of DJD 27. Therefore the published readings of בן of line 3 are correct; KhQ1 indeed refers to a conveyance of property from a man to a man.

end of the line cannot be correct because a *yod* would not be that close to a *nun*, directly above its foot. The final letter seems readable only as a *mem* or *kaph*. Was the name of Eleazar's father e.g. נחוש on analogy with נחש at 2 Sam. 10.2? Or some variant of נקסן, "Naqson" (or "Nixon"), read by Milik at Mur 19 iii 26? Was it "Naḥum" spelled defectively, i.e. נחמנו, "Naḥum from [--]"? Unfortunately this puzzle must be left unresolved.

4. The identity of the gift in line 4: what is being conveyed.

4.1. *He and het*. Cross/Eshel wrongly read the #3 letter of line 4 as a *het*, although the letter is in the shape of a routine *he*.⁴⁷ Cross/Eshel claim that "the reading of *he* and *het* is a problem throughout KhQ1. It is clear that these letters are confused or interchanged."⁴⁸ Cross/Eshel claim comparative parallels: "This is not a phenomenon peculiar to this scribe. Late Herodian scripts frequently confuse these two letters [*he* and *het*]. This is true of many ossuary scripts, the Uzziah Plaque, and the so-called Copper Scroll, all from this period."⁴⁹ In some of the cases cited the letters look similar and difficult to distinguish. In the Copper Scroll, confusions of *he* and *het* seem to involve both errors in copying and similarity in appearance.⁵⁰ (That is, the Copper Scroll seems to have been copied from an exemplar—i.e. it is not an autograph—with letters read and punched into the metal and some errors from misreadings.) But in KhQ1 *he* and *het* are distinguishable in form. In *he* of KhQ1/KhQ2 the crossbar by intention extends left beyond the left vertical and the right downstroke starts from above the crossbar. In *het* the crossbar does not extend intentionally to the left beyond the left vertical. The crossbar of *het* can be either at the top of both verticals (KhQ1 line 6, #4; line 14, #1; line 15, #1 [and if it is a *het*, line 3, #10]), or the crossbar can be a little lower giving the appearance of two "ears" (KhQ1 line 2, #4; compare KhQ2 line 2, #2).⁵¹ The difference

⁴⁷ Compare the other *he*'s in KhQ1 at line 4, #8; line 5, #5; line 6, #7 (?); line 7, #8; line 10, #5; line 12, last letter. In KhQ2 there may be a left end of a crossbar of a *he* at the start of line 2. For *het* compare in KhQ1 at line 2, #4 and #9; line 3, #10 (?); line 6, #4; line 14, #1 (?); line 15:1; and in KhQ2, line 2, #2.

⁴⁸ Cross/Eshel, DJD 36, 497.

⁴⁹ Cross/Eshel, DJD 36, 497-98.

⁵⁰ As summarized by Lefkovits the following groups of letters are confused in the Copper Scroll. First group: ך, ם, and ן; second group: ך, ך, ך, and ך; third group: ך, ך, and ך; fourth group: ך, ך, and ך (J.K. Lefkovits, *The Copper Scroll. 3Q15: A Reevaluation. A New Reading, Translation, and Commentary* (Leiden: Brill), 16. Confusions between "open" and "closed" *hes* and *hets* in the Copper Scroll are also discussed in A. Wolters, "Palaeography and Literary Structure as Guides to Reading the Copper Scroll", in G.J. Brooke and P.R. Davies (eds), *Copper Scroll Studies* (Sheffield: Sheffield Academic Press, 2002), 311-333 at 316-17.

⁵¹ There is a difference with Yardeni here in description. Yardeni also comments (correctly) that "contrary to the editors' observation, the scribe clearly distinguished between *het* ... and *he*" (Yardeni 1997 [IEJ], 235). According to Yardeni the difference is that in *het* "the left downstroke starts above the 'roof'", whereas in *he* "the left downstroke begins at the 'roof'". But there are at least three counterexamples, and perhaps four, in KhQ1 in which both left and right downstrokes of *het* do not start above the roof. These are at 6.4, 14.1, and 15.1, plus 3.10 if that letter is a *het*. (Of these, Yardeni considers only 6.4 a possible *het*. Yardeni's reading of 10.5 as *het* would be another example if her reading were correct, but that letter is a *he*. Concerning the other letters see discussion elsewhere in this study.) Only at 2.4 is there a clear case of *het* in KhQ1 in which the

is that in *het* the verticals are symmetrical whereas in *he* the verticals are asymmetrical; in *he* the left vertical is never above the crossbar, whereas the right one always is. Line 4, #3 is an unambiguous *he*, which is not surprising for the start of a word following **את**.

4.2. Line 4 identifies what it is that Ḥoni gives Eleazar, which is pivotal in interpreting the text. Cross/Eshel incorrectly read **את הסדי מחולן**, “Hisday from Ḥolôn[”], identifying the property given as a slave named “Hisday” (identified by Cross/Eshel as a slave based on another mistaken reading in line 14).⁵² Cross/Eshel read the #5 letter wrongly as *dalet*; that letter is clearly *qof*, as read certainly by Cryer and uncertainly by Yardeni.⁵³ Compare the *qof* of KhQ2, line 1, like the present letter, and the *dalets* of KhQ1, line 11, #4; line 12, #4; and line 8, #10 which are unlike the present letter. Yardeni did see a difference in form between *qofs* in the two ostraca (“the third letter [of Yardeni’s **הסקים** of KhQ1] being a semi-cursive *qof*” whereas “a more formal variant of *qof* appears on ostrakon No. 2, Line 1”).⁵⁴ In any case the letter of KhQ1, line 4, #5 is *qof*. The Cross/Eshel reading of the proper name **הסדי**, “Hisday”, does not exist in the text, either here or in line 14.

4.3. The #4 letter of line 4 is either *samekh* (Cross/Eshel, Yardeni) or *mem* (Cryer). If *samekh* is correct the reading would be **את הסקים**, “the sacks”, as Yardeni suggests (Yardeni: **הסקים**), a variant spelling for biblical Hebrew **שקים** in accord with *sin/samekh* variation in Dead Sea

downstrokes do start from above the crossbar. Even there, in the color photo in Roitman 1997 the appearance of the top of the letter is more like cat’s ears than a goalpost or a capital “H”. Yardeni’s uncertain reading of the letter at line 8, #10, which has very prominent verticals above its crossbar or roof, as *het* is argued in the present study to be *dalet* in agreement with Cross/Eshel. In KhQ2 there is one *het* (incorrectly read as *he* by Cross/Eshel) at line 2, #2, which does have prominent verticals above the roof just as Yardeni describes for the *het* of KhQ1. As discussed later this is one of several minor differences between KhQ1 and KhQ2.

⁵² Cross/Eshel: “The mention of a gift of a slave” (1997 [IEJ], 26); “the grant of the slave and estate of Ḥoni” (DJD 36, 502).

⁵³ Cross responded to Yardeni’s reading of the line 4, #5 letter as a “semi-cursive *qof*” by arguing against reading the letter as “extreme cursive [with] head little more than a tick”. But that was not Yardeni’s claim. According to Yardeni the letter is “a semi-cursive *qof* with a small triangular upper part” in a text that Yardeni characterizes as “semi-cursive” and which Cross/Eshel characterize as “penned in a vulgar, semi-formal style, with an occasional cursive lapse” (DJD 36, 497). According to Cross, Vulgar semiformal hands “fluctuat[e] between formal and semicursive traditions” (F.M. Cross, “Excursus on the Palaeographical Dating of the Copper Document”, in M. Baillet, J.T. Milik, and R. de Vaux, *Les ‘Petites Grottes’ de Qumrân* [DJD 3; Oxford: Clarendon Press, 1962], 217-221 at 217). Cross did not respond to Yardeni’s actual claim. (Cross’s full comment: “In the case of *dalet* versus *qof* [KhQ1, line 4] neither [Yardeni’s] drawing nor ours is entirely accurate. She shrinks the left side of the head of the letter; our drawing distorts the right side of the head. KhQ2 1 exhibits this scribe’s *qof* with its relatively large and distinct head. To be sure, there are extreme cursive forms in which the head is little more than a tick, but the ostraca are not in the extreme cursive tradition. As we have noted, they are inscribed in a vulgar semi-formal script” [DJD 36, 505].)

⁵⁴ Yardeni 1997 (IEJ), 236.

economic texts generally and attested directly for this word in Mishnaic Hebrew.⁵⁵ This word occurs in biblical Hebrew in contexts of bags of dry goods such as grain (Gen. 42.25, 27, 35). Cryer read the line 4 word, however, as **את המקום**, “the place”.

4.4. It is no argument in favor of Yardeni’s **הסקים** (and is not claimed by Yardeni) that the fourth letter of the word (line 4, #6) seems short, more like *yods* in many Qumran texts compared to *waws* which tend to be longer. Yardeni herself notes correctly that “*waw* and *yod* are identical in structure” in KhQ1.⁵⁶ Analysis bears this out. On the basis of context (known words or syntactic expectations) seven *yods* can be identified in KhQ1 (1.7; 2.2; 2.11; 6.9; 7.6; 8.6; 12.3). Meanwhile, in the same way eight *waws* can be identified in KhQ1 (2.5; 4.6; 6.1; 7.1; 8.1; 8.3; 11.1; 12.1). (Uncertain *waw/yods* in KhQ1 remain at 5.4; 5.9; 6.6; 6.11; 9.1; 10.2; 12.4.) No systematic features or traits can be described that are specific to the confirmed *yod* cluster and not found in the confirmed *waw* cluster, and vice versa. For example, the *waw* of **בירהו** of line 2 and the second *waw* of **וכול** of line 8 are short in length like the present line 4, #6. In the present study every *waw* and *yod* reading in KhQ1 and KhQ2 is considered in principle palaeographically ambiguous between *waw* and *yod*, without marking this particular uncertainty in transcription.

4.5. For *mem* or *samekh* at line 4, #4, since there are no known cases of *samekh* in KhQ1 or KhQ2 for comparison this is difficult to evaluate. (Yardeni does not give reasons for reading *samekh* rather than *mem*.) But if palaeographic criteria are ambiguous, Cryer’s **המקום** is the expectation. **המקום** is repeatedly attested in Dead Sea economic texts as a general term for land in deeds of sale.⁵⁷ In the Aramaic deeds of sale published in DJD 27, which are closely related to deeds of gift in being conveyances of property, a standard word used is **אתרה**, “place”, the equivalent of Hebrew **המקום**.⁵⁸ These deeds of sale typically refer to houses and types of trees and fruit borne by the land, corresponding to identical elements recognizable in KhQ1. “Sacks” is unattested in any known Dead Sea economic text, whereas **המקום** is routine and expected at exactly this point in KhQ1. The property being conveyed is neither a *slave* (Cross/Eshel) nor *sacks* (Yardeni), but rather *land*, a *place* (correctly, Cryer). The central line 4 word is **המקומ**.⁵⁹ This in turn resolves the palaeographic ambiguities: the second letter of the word is *mem* (not *samekh*) and the fourth letter of the word is *waw* (not *yod*). Ironically, both Cross and Yardeni agree that an estate of land is the

⁵⁵ On *sin/samekh* interchange in Dead Sea economic texts generally see DJD 27, 12-13. On **סקים/שקים** specifically, M. Jastrow, *A Dictionary of the Targumim, the Talmud Babli and Yerushalmi, and the Midrashic Literature* (New York: Jastrow Publishers, 1967), 1019.

⁵⁶ Yardeni 1997 (*IEJ*), 234.

⁵⁷ E.g. readings of Milik in DJD 2: Mur 22.2 (Deed of Sale, heb.), **את מקום של חזקא**, “Ce terrain-ci qui appartient à Hizqa”; Mur 22.11, **ת[חומי המ]קום**, “the b[oundaries of the pl]ace”. **המקום** appears also at Mur 44.4-5 in a letter of Bar Kochba and in fragmentary contexts at Mur 49 1.1 and Mur 58 4.1.

⁵⁸ For example XHevSe 9.2-5 as read by Yardeni in DJD 27:

אנה מרעותי יומה דנה זבנ[ת] לך לאתרה דלי ... [ת]חומי אתרה דך ... אתרה דך בתחול[מה], “I, of my own will, on this day have sol[d] to you the place of mine ... [The bou]ndaries of that place ... That place—within its bounda[ries] ...”, etc. Also see notes 100 and 101 below.

⁵⁹ See below on medial and final *mem*.

thing being given in this text, despite their readings of words other than the natural word for an estate of land at the point where the thing given is named directly.

Cross:

“‘Sacks’ or ‘sackcloths’ as Yardeni translates here is most awkward in parallel with the following house boundaries and orchards. An estate is being deeded; one does not initiate such a deed with ‘sackcloths’ or ‘sacks’.”⁶⁰

Yardeni:

“This is simply a deed of gift of an estate and all the trees on it ... Despite our differences, we agree that one Ḥoni (ḤNY) ‘gave’ some property with fig trees and another kind of tree ... to ‘El‘azar.”⁶¹

4.6. The next letter (#8) following **המקומ** is a *he* as read by Cryer and Yardeni. Cross/Eshel wrongly read #8 as *het*, then interpret the mistaken *het* as belonging to a place name, **מחולן**, “from Ḥolôn”. But the letter’s crossbar extends left beyond the left vertical, and only the right vertical (not the left vertical) is above the crossbar. The next letter (#9), *lamed*, is unproblematic. Cross/Eshel transcribe an uncertain *waw* between the *he* and *lamed*; Cryer and Yardeni no letter. From study of the ostracon in Jerusalem and the color photos no letter seems to be in this position nor is the space between the *he* and the *lamed* greater than expected between *lamed* and a next letter. The correct reading therefore is **הל**. At the end of the line at the edge (#10) there is a vertical stroke with uninscribed space to the left, except close to or at the bottom a beginning of a left extension or foot may be discernible. The lack of a distinct flag at the top of the stroke seems to eliminate *yod/waw*. This final #10 letter could be *nun*, *zayin*, *aleph*, or *tet*.

4.7. A comment on medial/final *mem* is prompted by the reading **המקומ**. According to Cross/Eshel, “unlike in the Copper Document, medial and final *mem* are distinguished [in KhQ1]”.⁶² Cryer judged the opposite: “all the Mems on the ostracon have the same shape, with no distinction between initial and final Mem”.⁶³ Yardeni did not comment on this issue but transcribed final *mem* readings and medial *mem* possibilities conventionally in transcription. (There are no certain medial *mem* readings in Yardeni’s transcription of KhQ1.) The analysis here is that there is no distinction between medial and final *mem* in KhQ1. There are minor differences in the writing of other letters such as *aleph*, *he*, *het*, *lamed*, *shin*, *taw*, and *waw/yod*.⁶⁴ A basic structure is the same for these letters with variations in writing the basic structure. Similarly a single basic form is used for all

⁶⁰ Cross, DJD 36, 506.

⁶¹ Yardeni, “Breaking the Missing Link”, 44-46.

⁶² Cross/Eshel, DJD 36, 498.

⁶³ Cryer 1997 (*SJOT*), 234 n 9.

⁶⁴ *Aleph*: different meeting points of the strokes (4.1; 7.2; 7.4). *He*: extra horizontal stroke (7.8 compared to 4.3). *het*: difference in height of the crossbar (2.4; 2.9; 6.4; 15.1). *Lamed*: rounded hook (5.8) versus angled hook (3.3; 8.4). *Shin*: half-circle corrected to curved base stroke (1.5 compared to 1.2). *Taw*: shape of sloping vertical to the right (1.4; 2.7; 4.2; 6.3; 6.10; 10.4; 11.3) and in one case addition of a partial slanting foot to the left (7.3). *Waw/yods*: differences in heights and sizes of heads. Some letter differences could be explicable as corrections in which the writer changed one letter into another rather than actual differences in automatic writing of letters.

mems in KhQ1, normally open (line 1, #8;⁶⁵ line 4, #7; line 6, #5; line 7, #7), but twice closed (line 4, #4; line 5, #5). In each case a three-sided box is drawn and then a left vertical starts from above and cuts downward through the top horizontal. The two cases in which the left vertical closes the gap with the lower horizontal (4.4 and 5.5) appear to be non-significant. This is the same structure of the single exemplar of *mem* of the Qumran Practice Alphabet (KhQ3), found by de Vaux outside the buildings' walls.⁶⁶ Compare Cross's description of the semicursive hand of 4QXII^a: it "preserves a medial form of *mem* which resembles superficially final *mem* ... The medial form is usually open at the bottom left. The left downstroke always cuts sharply through the crossbar."⁶⁷ Similarly in analysis of a cursive final *mem* on an ostrakon from c. early 1st century CE, Yardeni reconstructs it as "an independent development from a semiformal middle *mem*" which Yardeni draws, and it (the reconstructed prior stage) looks like the KhQ1 *mem*.⁶⁸ Readings by Cross/Eshel of medial *mems* at KhQ1, line 5, #1; line 8, #3; and KhQ2, line 4, #4, are in each case mistaken. The removal of erroneous readings leaves the *mem* of KhQ1, line 4, #4 (the first *mem* of **המקור**) as the only certain medial *mem* in KhQ1/KhQ2. The lack of distinction between medial and final *mem* in KhQ1/KhQ2 makes these ostraca *like* the Copper Scroll (and like the Qumran Practice Alphabet) on this point, not unlike it. In this study all *mems* of KhQ1 (there are none in KhQ2) are transcribed by the single notation **מ**, even though this semicursive *mem* of the ancient writer looks closer to the final-form *mem* in the formal hands.

4.8. In fact no medial/final distinction for any letter is attested in either KhQ1/KhQ2 or the Qumran Practice Alphabet (KhQ3), with the single exception of one final-form *nun* of **נ** at KhQ1, line 3, #8 (but that is a special case in having a ligature; compare **ננ** of KhQ2, line 3).⁶⁹ Cross/Eshel mistakenly transcribe straight final *nuns* in lines 2, 13, and 15 of KhQ1, and in lines 3 and 4 of KhQ2. The final *nun* of **נתנ** of KhQ1, line 2, has a foot to the left in the photographs, drawn and transcribed correctly by both Yardeni in 1997 and drawn correctly by Cross in DJD 36 (but transcribed incorrectly by Cross/Eshel in DJD 36 as **נתן**). Yardeni does not draw or transcribe final-form *nuns* in either line 13 or 15. What Cross/Eshel see as a final *nun* in line 15 may be the right vertical of a *dalet*. Neither of the final-form *nuns* drawn and transcribed by Cross/Eshel in KhQ2 exist (see discussion of KhQ2 below).

⁶⁵ This particular letter, line 1, #8, which must be *mem* because the identification of **שתים** in the date formula is certain, seems to be missing part of the left downstroke in the black-and-white DJD 36 photo. However in the color photo in Roitman, *A Day at Qumran*, the left downstroke goes below the horizontal; at the bottom of the right downstroke a stroke to the left is visible; and the letter is readable as a routine *mem* structure. See the drawing of this letter of Yardeni 1997 (*IEJ*).

⁶⁶ DJD 36, Fig. 3.

⁶⁷ Cross, "The Development of the Jewish Scripts", in G.E. Wright (ed.), *The Bible and the Ancient Near East, Essays in honor of William Foxwell Albright* (Garden City, N.Y.: Doubleday, 1961), 133-202 at 186. The letters of 4QXII^a are at Fig. 4, line 1.

⁶⁸ Yardeni, "New Jewish Aramaic Ostraca", *Israel Exploration Journal* 40 (1990), 130-52, at p. 150.

⁶⁹ The single examples of *pe*, *tsade*, and *kaph* in the Practice Alphabet are all written in medial forms. The letter in the Practice Alphabet's *nun* position (between *mem* and *samekh*) has a vertical line with a prominent left horizontal oddly coming from the top, rather than the bottom, of the stroke; it is puzzling what that was intended to be.

5. Line 5: “all of” something.

5.1. The first letter of line 5 can be neither *mem* read certainly by Cross/Eshel and considered possible by Yardeni, nor *het* read by Cryer. Yardeni’s other possibility, *taw*, could be possible palaeographically but the correct reading seems to be rather *aleph*. The letter agrees with the *aleph* of line 6, #2 except for the right vertical. It seems that the right vertical of the *aleph* at line 5, #1 was drawn separately from the short oblique stroke, with either the top of the right vertical defaced or the stroke written shorter than usual. But the structure is that of a three-stroke *aleph*, and only illusorily looks like a two-stroke *taw*. Line 5, #2 is wrongly read by Cross/Eshel as an uncertain *he*. The start of the left downstroke above the crossbar, whereas the right downstroke does not, excludes reading this letter as *he*. Yardeni reads line 5, #2 as either *taw*, *mem*, or unknown; Cryer as *taw*. Indeed the letter is *taw*; compare with line 4, #2. The *taw* is corroborated by the word that emerges: **את**. Line 4 above starts with **את**. Lines 6 and 7 below start with **ואת**. It now becomes clear that line 5 too starts with the same word: **את**.

5.2. The marks at #3 were read as *yod* by Cross/Eshel (certainly) and Yardeni (uncertainly) but the marks are too small to be a *yod* and do not have the head of a *yod*. The color photos make it clear that non-letter defacing has intruded at #3 (two dark specks close together are of a darker hue from an abrasion); however that does not account for all of the traces. In the Zuckerman/Lundberg color photo the ink at #3 could agree with the top of a *taw* (with the lower part of the letter missing). The letter at #4 is either a *waw* or a *yod*. The next letter (#5) is either *mem* (Cross/Eshel, Cryer, Yardeni) or *samekh*, followed by *he* (#6), read by all. The extent of the gap before the *he* of #6 suggests that this *he* starts a new word. If the #3 mark is not a letter, the reading would be **את { } ומה**, that is, **אתהמה**, a short way of writing **את תומה**, “the whole of the ...”. In this case the *taws* ending **את** and starting **תומ** were written only once. Compare in line 6 the reading **ואתחמו** for **ואתחמו** and in line 7 **ואתאנימ** for **ואתאנימ**. On the other hand, if the mark at line 5, #3 is a letter it would be a defaced *taw* giving the same outcome: **את תומה**.

5.3. For the marks at #7 a horizontal line could be from the writer striking out letters, or it could be unrelated to the letters like the extraneous horizontal line below and to the right of the start of line 4. A horizontal mark through the *lamed* to the left at #8 could be an extension of the same anomalous horizontal line. In any case the remains at #7 seem inconsistent with either Cross/Eshel’s *zayin-he* or Cryer’s *gimel*; they suggest rather the top of a *mem* (compare at line 4, #4, and line 3, #12). Following the *lamed* at #8 Cross/Eshel, Yardeni, and Cryer all read a *waw* or *yod* (#9). While this is possible, the lack of a normal-looking head on the letter and the possibility of defacing suggests caution concerning this reading. Finally, there are marks (#10) at the end of the line in the photographs which seem to defy rational solution. Cross/Eshel read an uncertain *lamed*, Cryer a *lamed*, and Yardeni undeciphered. The problem with the *lamed* reading is there is no clear *lamed* hook below the ascender(s). The marks are simply puzzling.

5.4. Cross/Eshel correctly note that a “from this day” expression is anticipated or expected in a deed of gift.⁷⁰ But their reading of this expression in line 5 is palaeographically incorrect. Comparative parallels from other Hebrew and Aramaic Dead Sea documents suggest such an expression would occur before the naming of what is given, in texts written in first-person voices speaking in the

⁷⁰ Cross/Eshel, DJD 36, 504.

future tense. The corresponding position in KhQ1 would be not line 5 but rather the lacuna at the end of line 3.⁷¹ However the voice of KhQ1 is not first-person, future but instead third-person, past tense, as in P. Yadin 19, a deed of gift in Greek. In that text the “from today” expression comes at the end of the full listing of the property and its boundaries and description.⁷² In KhQ1 the corresponding position would be after line 8. In any case line 5, which follows the naming of the property in line 4, most likely has something to do with describing or further elaborating this property.

5.5. Might the line 5 reading be **ואת(ת) ומ המלאכתו**, “all of [(its/his)] earning[s...]”?⁷³ There would be a parallel with the same word and twofold distinction of wealth at 1QS 6.19, **גם את הונו**, “his property and his income will be given over to the Examiner in charge of the business of the Many”. Or might the reading be **ואת(ת) ומ המלוא(ו)**, “all of the fullness [of the ...]” or “all of [its] fullness”? But each of these conjectures are palaeographically problematic at letters #9-#10. In the end there is no claim here to identify the final visible word of line 5.

6. Line 6: boundaries of the property.

6.1. In line 6 Cross/Eshel read **וא>ת< תחומי הבית**, “the boundaries of the house”. Cryer and Yardeni each suggest other readings of the word before **בית**, but here the reading of Cross/Eshel is in principle clearly correct. As Cross/Eshel note, the word **תחום**, “boundaries” (or “territory”), is basic to texts dealing with transfer of land and frequently is in close association with **בית**. For example:⁷⁴

Mur 30, lines 16-18 (Heb. [Milik, DJD 2])

תחומי[י] המכר הזה ... המכר הזה בתחומו בית ו[] הֶתְאֲנִים הזיתים העץ
 “boundaries of this sale ... this sale, in its boundaries: a house and [_____] figs, olives, trees ...”

⁷¹ E.g. XHev/Se 8 (ar and heb [Yardeni, DJD 27]), lines 1-2, “[I] sold to you *today* the house [that I own] and the courtyard of...”; XHev/Se 9 (ar [Yardeni, DJD 27]), line 2, “I, of my own will, *on this day* have sol[d] to you the place of mine that is called...”; XHev/Se 64 (gr [Cotton, DJD 27]), lines 6-7, “I acknowledge that I have given you as a gift *from this day and for ever* my property in Maḥoza, which items are listed as follows...”, etc.

⁷² P. Yadin 19, lines 21-23, at N. Lewis, *The Documents from the Bar-Kochba Period in the Cave of Letters: Greek Papyri* (Jerusalem: Israel Exploration Society, 1989), 83-87.

⁷³ Note also variant spellings cited by Cross/Eshel (DJD 36, 503 n. 33) of forms of **מלא** (references here are updated): **מולאת** at 1QS 6.21 (twice) and 4Q256 7-8 xi 13; **מלואת** at 1QS 7.22, 24 and 4Q365 9a-b i 2; **מולות** at 4Q511 63 iii 2.

⁷⁴ More: Mur 22 (heb [Milik, DJD 2]), line 2, **בתחון[מין]**, “in its boundaries”; line 11, **ת[חומי]** **אתרא דך**, “boundaries of the place”; XHev/Se 50 (ar [Yardeni, DJD 27]), lines 8-9, **המ[קום]** **בתחומה ובמצרה תאניא**, “this place: within its boundaries and in its borders are figs”, etc.

XHev/Se 8a, line 8 (Aram. [Yardeni, DJD 27])

תחמא בתה דךךן דרומא

“The boundaries of that house (are):[(To) the south ...”

6.2. In the present line of KhQ1 (line 6) there are letters resembling *het* at #4 and *mem* at #5 following a *taw* at #3. This is too coincidental not to anticipate a form of the expected תחום preceding בית. As correctly read by all, the first three letters of line 6 are ואת. The ת is interpreted here as serving “double duty” as the second letter of the particle את and at the same time as the first letter of the next word, on analogy with the start of line 7 where the same phenomenon is clearly the case. It is not necessary to suppose an omitted letter by haplography as Cross/Eshel suggest. The phenomenon can be understood phonologically: the את was considered by the ancient hearer/writer as prefixed to the following word, תח(ו)מי, and as such, the *taws* ending את and starting תח(ו)מי were pronounced as a single sharpened consonant and written as a single *taw*.⁷⁵ The “doubled *taw*” phenomenon is seen in line 7 where the reading is ואתאנימו representing ואת תאנימו, and perhaps in line 5 where אתתומו may represent את תומו. It is less likely that the same scribal mistake occurred two or three times in brief succession than that these two or three instances are a glimpse of a rule-based phonological phenomenon.

6.3. Line 6, #4 is ה, *het*, as read correctly by Cross/Eshel. Cryer read ת and Yardeni reads ת/ח. In a reading of #4 as *taw* a darkened area in the photographs to the left of a striation is interpreted as the leftward extension of the *taw*. Yet there is no actual continuity of the ink, even faintly, between this mark and the ink of #4. This is clear in the color photos, unlike the *aleph* immediately below in line 7 where there is continuity of ink in the color photos. #4 has the structure of *het*, with two verticals of equal height and a crossbar at the top. For the *het* crossbar at the top compare KhQ1, line 2, #9; line 14, #1; and line 15, #1 (and, if it is a *het*, line 3, #10).⁷⁶ The #5 letter is *mem* as read correctly by Cross/Eshel.⁷⁷ In other *mems* the top horizontal is not as sharply slanted as the present letter, although there are slants upward in the *mems* of line 4, #4 and line 5, #5. Possibly this *mem* (line 6, #5) was written over an originally-written *waw* which is now the right side of the *mem*. That might account for the “peaked” appearance at the top right of this *mem*. (That is, the writer may have written the first three letters of the word תחו- spelled *plene*, then corrected by writing a *mem* over the *waw* giving the defective spelling, תחמ.) Cross/Eshel read the darkened area between #4 and #5 as *waw*, but this is doubtful. If it is a *waw* it could be an ancient attempt to correct a

⁷⁵ Compare Cross/Eshel’s comment: “Line 6 has been transcribed here as if one of the *taws* in the sequence ואת <ת> תחומי had dropped out due to haplography. Actually, however, there are a number of instances in West Semitic epigraphy where a doubled letter is written only once; for example ... *wyšbh* for *wyšb bh*, and *mlkty* for *mlk kty*” (DJD 36, 498).

⁷⁶ At KhQ1, line 2, #4, and KhQ2, line 2, #2, the *het* crossbar is below the tops of the verticals, but that seems to be part of the variation in writing the structure of *het*. See discussion at note 51.

⁷⁷ The line 6, #5 letter is not *pe* if the mark above its roof is ink, which it seems to be. An argument against this letter being *kaph* (another possibility to be considered) is there is no distinct right vertical going above the roof on the right, as in the *kaph* of line 8, #2. Therefore line 6, #5 seems to be *mem* since no other letter corresponds and since *mem* is expected. Compare the similar-appearing last letter of line 3 which may also be a *mem*.

defective תחומי to *plene* תחומי. More likely, the darkened area is unrelated to the current text but is related to a similar-appearing defacing at #5 a little lower to the left. Compare the observation of Cross/Eshel that “there are several traces of ink [on KhQ1], some smeared, suggesting the sherd was reused after being scrubbed”.⁷⁸

6.4. Line 6, #6 is a *waw* or *yod* with defacing. The color photos show the defacing to be unrelated to the writing of the letter. The downstroke goes down to the base level, but as discussed earlier *waw* and *yod* in KhQ1 cannot be distinguished on palaeographic grounds. *Yods* whose downstrokes go down to base level can be compared in שתים of line 1 and תאנימ of line 7. Letters #8-10 of the present line 6 are בית, “house”, followed by a word-separation space. Letter #7 preceding בית is a problem. Cross/Eshel read a certain *he*, Cryer a “scaled-down” *he*, and Yardeni an uncertain *taw*. If viewed in isolation #7 suggests either a *taw*, Yardeni’s reading, or *het*. However *he* is expected. There is visible defacing of ink immediately preceding #7; did that defacing include a removal of the top of the right vertical of a *he*? Or is #7 indeed *taw*, with the word before בית having an otherwise-unknown ות- feminine plural ending on the “boundaries” word, i.e. תחמות בית, “boundaries of [the] house”? Qimron notes that some nouns which are only masculine in biblical Hebrew have feminine byforms in Qumran texts, and vice versa.⁷⁹ Against this, תחומי is not known to have such an ending elsewhere. Or is #7 a case of *he* of an exemplar miscopied as *taw* or *het*?⁸⁰ #7 is considered here most likely to be *he* in agreement with Cross/Eshel, even though no right vertical above the crossbar is visible, presumably through defacing or a mistake (not because a different way of writing *he* was used). This is the sole case in the present study in which a letter is considered possible on the basis of context despite conflict with its apparent reading.

6.5. For #11 after בית, Cross/Eshel read *waw*, Cryer *waw*, Yardeni *waw* or *yod*. This letter is likely *waw* on syntactic grounds (introducing a noun or a verb). Although unnoticed in previous editions, a trace of a #12 letter is visible at the edge in the DJD 36 photo and in the Zuckerman/Lundberg color photo. It is a vertical downstroke starting above the height of the top of the preceding *waw/yod* going down to slightly above the “base level” of the *waw/yod*. Possibly this letter is a *nun* or a *shin*.

⁷⁸ Cross/Eshel, DJD 36, 502.

⁷⁹ Qimron, *Hebrew of the Dead Sea Scrolls*, 68-69.

⁸⁰ Cross/Eshel suggest KhQ1 could be a copy. “The text of KhQ1 is a draft or copy of a deed of gift” (Cross/Eshel, DJD 36, 503). Line 6, #7 is hardly *het* giving תפוח, “fruit”, e.g. תפוח בית יהוה, “fruit of the house of Yah[weh]”. Based on Dead Sea documentary parallels the house likely belongs to the original owner, “Honi”, as part of the property being given over to Eleazar. That is more likely than a theory of sacks of grain or fruit destined for or obtained from the temple in Jerusalem, etc. in a text which otherwise reads as a transfer of title of immovable property from one person to another. Also almost certainly to be excluded is reading #7 as *aleph* with part of the right arm defaced, giving אבית, attested at Mur 42.4 and 1QpHab 11.6 as a contraction for אל בית. In this case the sense would be “and its boundary (area) to the house of X”, with the “house of X” describing a limit of Honi’s property (X would be some name other than Honi). But phrases introduced by אל following תחום do not appear to be attested in Dead Sea texts; אבית naturally follows a verb, not a noun; and houses in Dead Sea deeds usually are part of the property being conveyed.

6.6. The reading of line 6 is therefore either, with Cross/Eshel, ואתחמי הבית, “and the boundaries of the house”, or else ואתחמו הבית, “and its boundary: the house ...” The second alternative, with *waw* pronominal suffix, would introduce constituents of the place’s boundaries or territory, starting with the house. The *waw* suffix would refer back to המקומו of line 4 as its antecedent, i.e. *its* territory, *its* boundary.⁸¹ The house would begin a list of items included in the place’s boundary or territory, rather than the house itself possessing the boundaries. But houses can have boundaries or territories, e.g. Elephantine Papyrus 25, line 8, ביתא זי תחומיה כתיבן, “This house, whose boundaries are described...”⁸² The question is which reading of the word gives a more natural fit in KhQ1 in the apparently sequential ואת ... ואת ... את ... את structure of lines 4-8.

Structure #1 with *yod* suffix (ואתחמי) (or with *waw*, ואתחמות בית):

(את) the *place* ... (את) *all of the* ...
and (ואת) the *boundaries of the house* and ...
and (ואת) the *fig trees*, etc.

Structure #2 with *waw* suffix (ואתחמו):

(את) the *place* ... (את) *all of the* ...
and (ואת) *its boundaries*: the house and ...
and (ואת) the *fig trees*, etc.

The judgment here is that Structure #1 in agreement with Cross/Eshel gives a slightly more natural reading. The defective spelling of תחמי, meanwhile, is routine. Compare תחמי אתריה at XHev/Se 8, line 4 (one line after the same word spelled *plene* in line 3 of that same text, בְּתַחֲמוֹהָ); and תחמא בתה at XHev/Se 8a, line 8. In this last example (XHev/Se 8a) there is a *plene* spelling of כול one line earlier in that text’s line 7, just as KhQ1 has both defectively-spelled תחמו (line 6) and *plene* וכול (line 8).

6.7. An implication of this analysis of line 6 is that it gives backward reinforcement to the correctness of reading המקומו, “the place”, rather than הסקימו, “sacks”, in line 4, as the nature of the property conveyed and described in this text. The text is concerned with immovable property, i.e. a parcel of land.

⁸¹ Compare Mur 30, line 18, which Milik in DJD 2 read as בְּתַחֲמוֹ בֵּית, “in its boundaries: a house and ...”; XHev/Se 8, line 4, תחמי אתריה, “boundaries of the place”; XHev/Se 50, lines 8-9, בה, אתרא דך בתחומה ובמצרה תאניא וכל די בה, “this place—within its boundaries and within its borders: fig-trees, and everything which is in it”, etc.

⁸² A. Cowley, *Aramaic Papyri of the Fifth Century B.C.* (reprint of 1923 edn; Osnabrück, Otto Zeller, 1967), 85-86.

7. Line 7: of figs and palm trees.

7.1. Cross/Eshel read **והתאנים הוי[תים]**, “and the figs, the ol[ives (?),]”. The first letter of line 7 is *waw* as read correctly by all. Cross/Eshel incorrectly read the second letter, a routine *aleph*, as an “N-shaped *he*” (and then cited this mistaken reading as supporting the plausibility of reading a routine *aleph* in line 8 as an “N-shaped *Het*”⁸³). But *alephs* are always N-shaped in KhQ1 whereas *he*’s never are; #2 is an *aleph*. The third letter is *taw* as read correctly by all. The first three letters are therefore the familiar letter string **ואת** which starts line 6 above and line 11 below. However at the same time Cross/Eshel’s **תאנים**, “figs” or “fig trees” is a good word in the context and must be correct. The phenomenon proposed by Cross/Eshel for line 6 in which two *taws* in a **ואת ת-** sequence were written only once, explains the reading in line 7 as well.⁸⁴ That is, the writer seems to have heard or understood the *taw* sound of **ואת** as also beginning **תאנימו**, such that a single *taw* was written instead of two.

7.2. Following the “fig trees” another type of tree or fruit is expected. At #8 a *he* is visible, read by all.⁸⁵ For #9 Cross/Eshel read *zayin*, Cryer *waw*, and Yardeni an uncertain *dalet* or *yod*. In fact #9 seems to be a *taw*; compare the *taw* at line 5, #2. Possibly the original right stroke of the line 7, #9 *taw* was overwritten by a stroke slightly to its left in order to put space between it and the preceding *he*, whose overwritten crossbar touched the right side of the originally-written *taw*. At the end of this line (line 7) there may be a trace of a #10 letter compatible with the right side of a *taw* or *mem* (compare the *mems* at line 7, #7 and line 6, #5), but not compatible with *yod*. The word therefore is readable as **התמו[רימו]**, “palms”. A less likely alternative would be **הזת[ימו]**, “olives”, spelled defectively. In this case #9 would be interpreted as a *zayin* written over a *taw* as a correction of a mistake by the same writer. In any case neither of Yardeni’s suggestions for #9, *dalet* or *yod*, seem possible. The wording “and fig trees, *pal[ms]*,” of line 7 followed by **וכול אילנ אד[מה]**, “and all trees of the ea[rth]”, of line 8 parallels Naḥal Ḥever 2.6, **ותמו[רין] ושקמיין ואילן**, “and *palms* and sycamores and trees”, and Naḥal Ḥever 7.48, **וכות כל תמרין ואילן**, “and, in like manner, all *palms* and (all) trees”.

8. Remaining lines of KhQ1: lines 9-15.

8.1. *Line 9*. #1 is a *waw* (compare above at line 8, #1). For #2 Cross/Eshel suggest *het* whereas Yardeni reads *nun*. The color photo in Roitman 1997 and the Zuckerman/Lundberg photos show both upper and lower pieces of the KhQ1 joined together (unlike the DJD 36 photo which has the pieces slightly apart). In these photos with the pieces joined no lower part of a right vertical is visible on the lower piece of the ostrakon. A trace of a left foot of a *nun* or left downstroke of a *taw*

⁸³ Cross/Eshel, DJD 36, 503 n. 36.

⁸⁴ See note 75.

⁸⁵ There is a slight hint of a vertical *waw*-like stroke before the *he* in the DJD 36 black-and-white photograph, but in the Zuckerman/Lundberg photographs the *waw*-like stroke is less distinct than the preceding *mem* or other letters in the vicinity and is not likely to be ink. Also, it is unusually close to the *he* to be a naturally-spaced letter.

could be visible on the lower piece. A Henderson photograph suggests #2 is *taw*.⁸⁶ For #3, Cross/Eshel's *nun* suggestion is excluded because no left foot is visible on the lower piece of the ostrakon. Yardeni reads #3 as *gimel*. The color photo in Roitman 1997 shows a chip or dark spot on the edge of the ostrakon crack, removing an apparent positive indication in the black-and-white photographs of a *gimel*. It is unclear what #3 could be (leading possibilities: *zayin* or *gimel*). Following these letters a cluster of marks in the DJD 36 black-and-white photo turn out in the color photo to be secondary damage marks from abrasions. (Notably, what looks like a left foot of a *nun* in the DJD 36 photo at #4 is illusory in the color photo.) After #4 there appears to be a word-separation space and then the ostrakon breaks off. Marks at the left edge of the line in the lower piece in the DJD 36 photo also appear to be secondary from abrasions in the color photo. The resulting reading וַיְהִי or וַיִּנְחַל starting line 9 could be either a noun or a *waw*-consecutive verb form.

8.2. *Line 10.* לִי or לְי, “to him” or “to me”, is followed by a direct object marker הָא and then a word starting with *he*. This suggests some transitive verb preceded in line 9, very possibly the word וַיְהִי or וַיִּנְחַל which starts line 9. Does this verb tell some additional action of the giver? Cross/Eshel and Yardeni each incorrectly read #5 as an uncertain *het*. In the color photos there is an extraneous mark in the expected position of a *he* crossbar extension to the left (compare in both black-and-white and color photos an extraneous non-letter spot of identical hue and appearance just above it). A similar non-letter spot seems to be in the position where the top of a right vertical is expected if the letter is *he*. Nevertheless, against Cross/Eshel and Yardeni the letter seems to be *he* because ink from a left extension of the crossbar, distinct from the extraneous spots, seems visible in the color photo of Roitman 1997, and this seems confirmed in the Zuckerman/Lundberg images as well. The letter spacing associated with this letter may add an element of further weight in corroboration of *he* as opposed to *het*.

8.3. *Line 11.* The readable letters וְאֵת appear to introduce some second object of the verbal clause which began with the verb hypothesized in line 9. Traces of two illegible letters follow in most photographs, then either a word-separation space or totally defaced letter, and finally a last letter of the line which is probably *tsade* as Yardeni reads. In the black-and-white photographs there seems to be a faint suggestion of a reading, וְאֵת הָאָרֶץ , “and the land [...]”.⁸⁷ The medial form of *tsade* in final position is consistent with uses of medial-form *mem*, *nun*, and *pe* in final position in KhQ1 and KhQ2. On another matter, Cross/Eshel write: “some of the *'aleps* of KhQ1 are quite formal, with rudimentary *kerai*. For example, see the *'alep* in line 11.”⁸⁸ In the black-and-white photographs, the *aleph* of line 10, #3 appears to have a *kerai* at the bottom of its left foot, but the color photographs make clear that that is non-letter defacing; that line 10 *aleph* has simply a routine unembellished downstroke like any other. For the *aleph* of line 11, #2, what appears in the black-and-white photographs to be a *kerai* at the top of the right arm is also revealed in the color photographs to be illusory. Only an apparent *kerai* at the bottom of the left leg of the *aleph* of line 11, #2 remains not falsified by the color photographs. While this last item could not be confirmed to be illusory, the fact that no other arm or leg of an *aleph* in either KhQ1 or KhQ2 has a *kerai* suggests some accidental thickening at the bottom of this particular *aleph* downstroke, rather than an intentional *kerai* on the part of the ancient writer.

⁸⁶ Fig. 11 of Henderson, “A New Image Enhancement Procedure”.

⁸⁷ See in particular Fig. 11 of Henderson, “A New Image Enhancement Process”.

⁸⁸ Cross/Eshel, DJD 36, 498.

8.4. *Line 12.* A clear reading וביד, “and into the hand of—”, or “and by the hand of—”, is followed by a heretofore unrecognized proper name: יהוה יהוה, “Judah”. This reading, though the letters are faint and difficult to decipher, is based on the Zuckerman/Lundberg photos and one photograph of Henderson.⁸⁹ The proper name is of interest since it is not the name of either of the two parties to the conveyance (Honi or Eleazar). Who is this Judah? Is he a scribe, perhaps the writer of KhQ1?

8.5. *Line 13.* The first letter is *lamed* and the second also looks like a *lamed* which would give לל, followed by what seems to be a space and then either a medial *nun* or some other defaced letter. It is difficult to interpret this sensibly; is #2 other than a *lamed*? The vertical stroke at #4 which Cross/Eshel read as a final-form *nun* is blurry and may be from previous writing on the ostrakon.

8.6. *Line 14.* #1 is probably *het* as read by Cross/Eshel, not *he* read by Yardeni. What appears to be a right vertical intentionally above the crossbar seems to be part of a non-letter descending line just above it. The hairline extension of the crossbar to the left is too inconsequential to confirm that a *he* was intended. At #2 there is what looks like a *lamed* ascender but the letter is uncertain. #3 is undecipherable in all photographs except one: oddly, one photograph of Henderson shows a clear *nun* in the line 14, #2 or #3 position. This *nun* is not confirmed, even faintly, in any other photograph, including the Henderson photograph published by Cross/Eshel in *IEJ* and DJD 36.⁹⁰ #4 read by both Cross/Eshel and Yardeni as a certain *ayin* is probably in error; the letter appears instead to be *tsade*.⁹¹ #5 does not seem to be *bet* read certainly by Cross/Eshel and uncertainly by Yardeni.⁹² Is #5 a *resh*? The letters after this are unreadable. There is no reading of “Hisday the servant” of Cross/Eshel here or any other place on the ostrakon.

8.7. *Line 15.* The first letter is a *het*, in agreement with Cross/Eshel, against *he* read by Yardeni. The color photos show no extension of the crossbar to the left as illusorily appears in the black-and-white photos and as drawn by Yardeni (but, correctly, not drawn by Cross in DJD 36). In the color photos the supposed left extension is an extraneous non-letter mark below the height of the crossbar. The crossbar is at the top of the verticals. The mark which Yardeni draws rising upward from the middle of the roof of the letter also seems to be non-letter in the color photos; compare the black-and-white photo in Cross/Eshel’s article in *Biblical Archaeology Review*. With these two clarifications #1 can only be *het*. #2 is a *lamed* and after that all is indecipherable. *Line 16.* There is a trace of an undecipherable letter.

⁸⁹ Zuckerman/Lundberg photos 151 and 152, and the photograph of Henderson at Fig. 11 of “A New Image Enhancement Procedure”.

⁹⁰ See Fig. 11 of Henderson, “A New Image Enhancement Procedure”.

⁹¹ Although *ayin* and *tsade* resemble each other the structure is different. With *ayin* a left arm is added to the main slanting stroke which is straight (see at KhQ2, line 4, #5); with *tsade* a right arm is added to the main stroke which is curved (KhQ1, line 11, last letter). In the present case (KhQ1, line 14, #4) a right arm appears to have been added to the main stroke, the structure of *tsade*.

⁹² The apparent tick at the upper left of line 14, #5 is non-letter defacing in the color photo of Roitman 1997 (part of a horizontal line of defacing dots). The color photo shows another intrusive mark to the left of the left arm of the previous letter (#4) which renders illusory the impression in the black-and-white photo of a right ear on #5.

9. Reconstruction of KhQ1.

9.1. The left ends of all lines are missing in KhQ1. No line-length for KhQ1 can be determined from a secure “wraparound” line restoration. Cross/Eshel assume a line length not much longer than the visible letters of lines 1-8. They write in DJD 36: “It is possible that ‘Jericho’ [in line 2] refers to the district or toparchy, and that a specific place-name appeared at the end of line 1, possibly the ancient name of Khirbet Qumrân; however there is not much room left for such a reconstruction.”⁹³ But Cross/Eshel give no explanation for their line-length assumption. (Elsewhere Cross/Eshel assume the opposite, that there is enough room in line 1 for such a word, e.g. in Roitman 1997: “It may be presumed that the ancient name of Qumran appeared [at the end of line 1].”⁹⁴) There is no material limitation to significantly longer line lengths in KhQ1.⁹⁵ It may be that lines 1-8 of KhQ1 contain only a few more letters at the end of each line. But unless this is established it cannot simply be assumed.

9.2. It is possible that the line 2 lacuna continued with no more than **ן]ב** and a proper name of Ḥoni’s father, and that line 8 continues after the completion of the word partly visible at the end of line 7 (with or without one more word), in agreement in spacing with a minimal reconstruction of line 2. But neither of these possibilities just named, at lines 2 and 7, are certain. In line 2 the name of Ḥoni’s father’s father, or a gentilic or town of origin of Ḥoni, could well have followed.⁹⁶ In line 7 there could be unknown additional elements in a list prior to the closing clause of line 8, although the parallels cited earlier to “palm trees” followed by “<trees in general>” are perhaps an argument for a short column width. Most importantly, the successive lines beginning with **תא** and **תא** at lines 4, 5, 6, and 7 raise the question of whether the lines were the same lengths. The issue of column width of KhQ1 is unresolved. The reading of KhQ1 of this study is as follows.

⁹³ Cross/Eshel, DJD 36, 501. Similarly earlier, “To be sure, there is not a great deal of room left in the line for such a reconstruction” (Cross/Eshel 1997 [IEJ], 20).

⁹⁴ Cross/Eshel, in Roitman, *A Day at Qumran* (1997), 39. Also in *Biblical Archaeology Review*: “The end of line 1 may have included a more specific local place name, possibly the ancient name of Qumran, which remains a mystery” (Cross/Eshel, “The Missing Link”, 52).

⁹⁵ The surviving KhQ1 ostrakon measures 6.3 cm in width (Cross/Eshel, DJD 36, 497), and 15 letter spaces per line are visible in KhQ1 at its widest. Compare, for example, the Maresha ostrakon from 176 BCE published by Eshel and Kloner which measures 14 cm in width with a visible line of 33 letter spaces, over twice that which has survived in KhQ1 (E. Eshel and A. Kloner, “An Aramaic Ostrakon of an Edomite Marriage Contract from Maresha, Dated 176 B.C.E.”, *Israel Exploration Journal* 46 [1996], 1-22 at 2).

⁹⁶ Yardeni commenting on the Dead Sea documentary texts in Aramaic and Hebrew: “The parties appear in their names and the names of their fathers, and sometimes their grandfathers’ or family names. In addition to the name, or in its place, an appellative may appear. The parties are further identified by their place of origin ... **מן**, or their place of residence ... **בתב ב**, or by both of these” (Yardeni, DJD 27, 14-15).

10. Transcription of KhQ1 (Ostracon No. 1).⁹⁷

Translation

1	In the second year of [...]° בשנת שתימ ל°	1
2	in Jericho, gave Honi s[on of <PN> ... (?)	בירחו נתנ חני ב]נ	2
3	to Eleazar son of N< >, [...]לאלעזר בן נ°(מ/כ)	3
4	the place, the l[...]]אתהמקומהל°	4
5	all of the ml[...]]את(ת°)ומ המל°	5
6	and the boundaries of the house and [...]ואתחמ(י/ו)(ה°/ת)בית ו(נ°/ש°)	6
7	and the fig trees, the pal[m trees ...]ואתאנימ התמ°ימ	7
8	and all trees of the ea[rth ...]וכולאילנאד°מה	8
9	and it/you shall be <verb>(?) [...] ו(ת°/נ°) °	9
10	to him/me the [...]ל(ו/י)את ה°	10
11	and the land (?) [...]ואתהאר°צ	11
12	and by (or, into) the hand of Ju[d]ah [...]וביד יהו°ד[ר]ה	12
13	l ... [...] ל ° °	13
14	h.... [...] ח(נ/א/°)צ° °	14
15	hl... [...] חל °	15
16	... [...] °	16

11. Interpretation of KhQ1.

11.1. A number of Dead Sea economic or documentary texts have been published which deal with transfers of title of land. Of particular interest are the documentary texts in Hebrew and Aramaic published by Yardeni in DJD 27 in 1997. As brought out in Yardeni's introduction, although the documents are individually variant they use the same language and basic forms. The descriptive summary is worth quoting:

Prominent in the legal documents from the Judaean Desert is the phenomenon of common phrases and frozen expressions characteristic of conservative legal language. Nevertheless, there is a wide variety in the orthography and morphology of the words themselves, and so too in the use of synonymous expressions ... In addition to these, there are differences in formulation, such as change in person (subjective or objective formulation), expansion or contraction of the clauses, changes in the order of the boundaries of the property, etc. The differences in formula and linguistic variety reflected in the deeds from the Judaean Desert witness to the fact that their writers formulated the deeds independently according to the differing circumstances, and to the freedom allowed at that time in writing legal documents. Nevertheless, a picture emerges of a unified and well-established structure as to the order of

⁹⁷ This Hebrew transcription reflects only word-separation spacings that actually exist on the ostracon, whereas in discussion in this article word-separations have usually been normalized for ease of reading.

the clauses in the most common deeds (deeds of sale for immovable property, and promissory notes)—evidence of an accepted tradition.⁹⁸

11.2. In this introduction Yardeni goes on to outline thirteen features which characterize Dead Sea deeds of sale of immovable property.⁹⁹ These features provide the best context for understanding KhQ1. (1) *The date* is visible in line 1, but it is year only; it is missing the day and month (discussed below). The rest of line 1 will have been occupied with the remainder of the date formula. (2) *The place* is either “Jericho”, the first word of line 2, or as Cross/Eshel have suggested, some place within the legal district of Jericho, named at the end of line 1. (3) *The parties and the transaction* are visible in lines 2-3; the giver is Ḥoni and the receiver is Eleazar. בן plus the name of Ḥoni’s father will have followed in the lacuna at the end of line 2. Line 2 could end at that point or Ḥoni’s place of origin might also be named (-ג). The lacuna ending line 3 following Eleazar’s father allows for a further word or words. This could be a place of origin, a descriptive word such as a title, or a statement of purpose for the gift. If places of origin for Ḥoni and Eleazar were not named, the estimated length of line 2 is perhaps c. 5-8 spaces beyond the visible end of line 2. (4) *The property* is named in line 4, introduced by a direct object marker continuing syntactically from נתנ of line 2: את המקום, “the place”, or land.¹⁰⁰ A clause in line 5, introduced by a direct object marker,]° ומה המל, “all of the ml- ...”, might further describe the property, although this is unclear. The lack of a waw preceding את starting line 5 could suggest that another verb, parallel to נתנ in sense, occurred at the end of the lacuna of line 4. Alternatively, את starting line 5 could be naming the second item in a series of four direct objects of נתנ, each introduced with את. (That two את’s in lines 4 and 5 are followed by two ואת’s in lines 6 and 7, instead of one את followed by three ואת’s, could be simple accident.) The borders of the house of line 6 and the fruit and trees of lines 7-8 appear to be further naming of the property being conveyed.

11.3. Expected next is a (5) *boundary clause* in which the extent of the property is named. ואתחמי הבית is visible in line 6, “and the boundaries of the house ...”; however actual definitions of the boundaries do not seem identified in KhQ1. Yardeni notes in DJD 27 that first mentions of “the place” and “boundaries” can be separated from fuller descriptions of these items in this kind of document.¹⁰¹ (6) *Description of the property* may occur in lines 7-8 in terms of the figs, palms, and other trees produced by the land. (7) *The sum* would be skipped in a deed of gift. (8) *Receipt*, (9),

⁹⁸ Yardeni, DJD 27, 13.

⁹⁹ Yardeni, DJD 27, 13-17.

¹⁰⁰ “The property is either indicated explicitly, or with the word אתרהא or אתריא [Aramaic equivalent of המקום]” (Yardeni, DJD 27, 15).

¹⁰¹ Yardeni comments at XHev/Se 9, line 2, that אתרהא indicates “the property, which is not described here” (Yardeni, DJD 27, 44). Compare XHev/Se 50, lines 8-10, אתרהא דך בתחומה, “That place—within its boundaries and within its borders: fig-trees, and everything which is in it and which is fitting to it, the entrance and the exit, as it is fitting”. The definition of the boundaries occurred earlier, in lines 6-8 of that text. In XHev/Se 21 “places within their boundaries” are detailed in lines 3-5, said to be sold in line 5, and then referred to collectively as simply “places” (the meaning is understood) in line 7.

ownership, (10), responsibility and ‘cleansing’, (11) guarantee, and (12) exchange of the deed, may or may not have correspondences in lines 9-16 which are largely undecipherable. (13) Signatures of the parties and witnesses are missing in the lower lines of KhQ1, although it is possible that a scribe is named in line 12, “and by the hand of Judah ...”.

11.4. The structure **ואת ... ואת ... את ... את** seems to follow from the verb **נתן** of line 2, creating either one long sentence from lines 1-8, or else two consecutive sentences (lines 1-4 and lines 5-8, with a second verb parallel to “give” at the end of line 4). Either way the 3rd person, past-tense voice of **נתן** is likely to have remained consistent throughout lines 1-8 (and did not, for instance, change to a 1st person future-promise voice by a verb introducing Honi as speaking at the end of line 3). However the 3rd person, past-tense form of KhQ1 compares to none of the Hebrew and Aramaic documents published in DJD 2 and DJD 27.

11.5. The 3rd person, past tense of KhQ1 does compare, however, to the Greek document Papyrus Yadin 19 from 128 CE, one of the three known Jewish deeds of gift from the Roman period found in the Judean desert according to Cross/Eshel.¹⁰² In P. Yadin 19, a Judah of Ein Gedi wills property to his daughter Shelamzion. The text is written in a 3rd person voice defining a transaction accomplished by the document, with intent for benefit to the recipient to run into the future starting from the completed event of the transfer (“so that the aforesaid Shelamzious shall have the half ... from today, and the other half after the death of the said Judah”). If KhQ1 paralleled P. Yadin 19 on this point the corresponding expression in KhQ1 would follow the description of the property (that is, about line 9 a new verb would introduce a statement of promise or intent in the future). But KhQ1 is too broken to confirm or disconfirm this. As in P. Yadin 19, the voice of KhQ1 is not that of one of the parties to the transaction but rather that of a third party, perhaps a scribe or other official. But KhQ1 differs from P. Yadin 19 in other key points—KhQ1 has no signatures, the date is only a year date, and it seems generally more terse. In P. Yadin 19, after the statement of the gift a summary follows in the giver’s hand, in Aramaic: “Yehudah son of Eleazar Khthouision: I have given the courtyard and the house therein to Shelamzion my daughter according to what is written above ... Yehudah wrote it.” Below that the scribe who drafted the deed states in Greek that he wrote it. On the back of the deed are signatures of seven witnesses (six in Aramaic, one in Greek). All of this seems missing in KhQ1.

11.6. Notably, although boundaries are mentioned in KhQ1, the definitions of those boundaries are missing. Yardeni notes:

The boundaries clause is found in all deeds dealing with the transfer of immovable property, including Aramaic deeds from Elephantine, and mediaeval deed formularies. The boundaries are indicated according to the points of the compass. The order is generally: east, west, south, north ... though sometimes this changes.¹⁰³

Yet in KhQ1 there is no sign of definition of the boundaries. How is this terseness and brevity of KhQ1 compared to known deeds to be understood?

¹⁰² Papyrus Yadin 19, “Deed of Gift” is found at Lewis, *Documents from the Bar-Kokhba Period*, 83-87. It is identified by Cross/Eshel as one of three deeds of gift known from the Roman period found in the Judean desert (DJD 36, 501 n. 17).

¹⁰³ Yardeni, DJD 27, 15.

11.7. Cross/Eshel suggest that KhQ1 was a rough draft written in advance of the deed; that is why the month and day of the date are missing in line 1 of KhQ1. The full date would be written when the real deed was made in fuller form.¹⁰⁴ Cross/Eshel also note the missing signatures and the material of writing of KhQ1 (a potsherd) as calling into doubt an identification of KhQ1 as the deed itself.¹⁰⁵ But would not a draft be a draft of the intended deed (instead of an abbreviated form to be expanded later)?

11.8. These considerations caused Callaway to question whether KhQ1 is accurately characterized as a deed of gift (or a draft of one). Callaway suggested that KhQ1 is instead “a quasi-legal document attempting to make a particular claim about the current ownership of Ḥonî's property in Jericho ... It is a past tense account, a sort of after-the-fact claim about a property exchange between Ḥonî and Eleazar”.¹⁰⁶

11.9. Actual deeds typically contained a promise to certify ownership at later times upon request. In basic agreement with the suggestion of Callaway, KhQ1 could be not a deed of gift but rather a statement or certificate of title, referring back to what had been documented with a full deed of gift, but not itself that deed. If this conjecture is correct, certain implications would follow. First, “Year 2 of—” of line 1 would be the complete date, similar to year-only dates on coins or weights. (The actual deed with month and day and fuller details would exist somewhere else.) Second, “Year 2 of—” (whenever that was) would be some time before the writing of KhQ1, not necessarily the date of writing of KhQ1. And third, the lack of a visible future tense in KhQ1 would be accounted for. By this hypothesis nothing would be missing in KhQ1; the text would be complete in terms of its genre and purpose.

12. A 1QS connection?

12.1. With the line 8 *yahad* reading nonexistent, is there contact between KhQ1 and the literary texts found in the caves near Qumran on the basis of an accurate reading of the text? The Community Rule mandates that a new member's property be given over to an officer of the community described in that text at the start of the new member's second year, and a written record made of the gift.

1QS 6.17-20

ובמילאת לו שנה בתוך היחד ... יקריבו גם את הונו ואת מלאכתו אל יד האיש
המבקר על מלאכת הרבים וכתבו בחשבון בידו ועל הרבים לוא יוציאנו

“... and when he has completed one year within the community ... his property and his earnings shall be given into the hand of the Examiner in charge of the business of the Many

¹⁰⁴ *Cross/Eshel*: “Since we must suppose the ostracon is not the legal document itself, but a draft of the deed of gift, the scribe may have left open the precise date” (DJD 36, 500).

¹⁰⁵ *Cross/Eshel*: “There appear to be no signatures of witnesses [in KhQ1] (though admittedly the lower part of the ostracon is virtually illegible). Moreover the date in the first line is incomplete. That the document is written on an ostracon also seems to lead to the same conclusion that this is not the original deed” (DJD 36, 505).

¹⁰⁶ Callaway, “A Second Look at Ostracon No. 1”, 161-62, 164.

and they shall write it into the account-record in his (the Examiner's) own hand and they shall not spend it for the Many ...”

12.2. According to the Community Rule, at the end of the novitiate's second year, if he was approved for full membership, his property (which up to then had been held in trust separately) would then be mingled with the community's funds (1QS 6.21-23). Cross/Eshel suggest a parallel in Acts 4.34: “for as many as were possessors of lands or houses sold them, and brought the proceeds of what was sold”.¹⁰⁷ However as Cross/Eshel note, in Acts 4.34 the property is first converted to money before it is given over to the group. In KhQ1 the property is given with no conversion to money.

12.3. Is it possible the “second year” of line 1 of KhQ1 refers to the second year of the novitiate of 1QS? In 1997 Cross/Eshel considered this possibility but rejected it on the grounds that since other economic texts begin with date formula and location, that is how the visible “in the second year of—” and “in Jericho” of lines 1-2 of KhQ1 should be interpreted.¹⁰⁸ Cross/Eshel were correct that the overwhelming expectation is that KhQ1, line 1 is a date formula. The year-alone form does not affect this. As Cross/Eshel note, a marriage contract ostrakon in Hebrew from Maresha from 176 BCE has only month and year, but no day.¹⁰⁹ An ostrakon in Aramaic published by Yardeni from c. 1st century CE—a record of grain deliveries—has a date formula with only a numbered year, without day or month, exactly like KhQ1.¹¹⁰ Although the date formulas in these ostraca vary in specifics, they are still date formulas. Therefore there is no likelihood that the “second year” of KhQ1 without a month or year means it is the 1QS second year, despite the coincidence.¹¹¹ To keep the “year two” coincidence in perspective: in DJD 2 there are eight Dead Sea documentary texts in Hebrew and Aramaic in which a numbered year in a line 1 date formula is readable, and three of

¹⁰⁷ Cross/Eshel, DJD 36, 504.

¹⁰⁸ *Cross/Eshel*: “Prof. S. Goranson has suggested that we should consider the possibility that year two refers to the second year of Ḥonî as a neophyte. However, since Line 2 starts with the word Jericho, it seems that we are dealing with a regular formula of deeds, which begins with a date and the place where the deed was written” (Cross/Eshel 1997 [IEJ], 20 n. 8).

¹⁰⁹ Eshel and Kloner, “An Aramaic Ostrakon”, 3-5. “[The ostrakon from Maresha] is the first marriage contract written on pottery to be discovered in Palestine ... [it] appears to be a copy or a draft, made in order to set out the marriage terms” (“An Aramaic Ostrakon”, 19-20).

¹¹⁰ Yardeni, “New Jewish Aramaic Ostraca”, 132-33. Of this group, Ostrakon No. 1, line 1, reads:](ל/ב/ע)<number marks>שנת, תליתיתא, “The third, year 12 (or 13) to/of ... [...]”. As read by Yardeni, תליתיתא names a third delivery, followed by the date formula which consists of year only.

¹¹¹ To make a case for a 1QS/KhQ1 “second year” identification, it might be proposed to restore lines 1-2 something like: נתנ הנו ב[יד מבקר] לאלעזר, “In the second year of the [<novitiate> of <PN>, <in the presence of the assembly>] at Jericho he gave his wealth (נתנ הנו) into [<the community's treasury/the hand of the treasurer>], to Eleazar son of N___, Ov[er]seer of the Community]...”, etc. This would read הנו instead of הני after נתנ in line 2 (for the spelling, compare נוֹיֵבָה at 1QS 8.23). But against this, a proper name (i.e. הני, “Honi”) is expected to follow נתנ in line 2 as the verb's subject; the restoration of line 1 just noted followed by a *lamed* prefixed to “Eleazar” beginning line 3 is questionable; and it would remove the date formula from line 1 which is expected on the basis of other documentary texts.

those eight are “in the second year of” (the others are years 1, 3, 4, 6, and 11). In DJD 27 there are an additional eight such texts of which one is “year two” (the others are years 3, 3, 3, 3, 8, 8, and 25). In this limited database 4 out of 16 total have “second year” date formulas, or a 25% incidence through random chance. Yet none of this is the relevant statistic. The relevant comparison is that *one hundred percent* of numbered years found in line 1’s of documentary texts are date formulas. Therefore this is the indicated interpretation of KhQ1, line 1.

12.4. Interestingly, the abbreviated, past-tense form of KhQ1 noted earlier is in agreement with the form which 1QS appears to indicate was to be made in recording gifts of property of new members to the *yahad*—presumably past tense, third person, and archived.¹¹² However 1QS 6.17-20 says that both the *receiving* of the novitiate’s earnings and property and *the writing of the record* of the gift were to be done by the same officer, the **הַאִישׁ הַמְּבַקֵּר**. But in KhQ1 these two functions are done by differently-named individuals (the recipient is “Eleazar”, line 3; the writer is “Judah”, line 12). Furthermore, as noted, the parallel between 1QS 6.17-20 and Acts 4.34 (to the extent Acts 4.34 can be invoked to assist in understanding 1QS) suggests that the property and earnings given to the officer of a community such as that of 1QS would have been liquid (money), whereas in KhQ1 the land itself is conveyed, not first converted to money.

12.5. Cross/Eshel note that whereas all previously known Judean deeds of gift are from men to women (wives or daughters), KhQ1 is a deed of gift from a man to another man. Cross/Eshel interpreted this as unusual and in light of the find site, significant:

If these arguments are sound, we are dealing here with a deed of gift ... addressed to a man which is a rare occurrence in the Second Temple period. Its subject matter is of great importance. The deed is not a grant to a family member, but, we believe, to a member of the community living where the ostrakon was found, namely the bursar of the sectarian community.¹¹³

12.6. But is the male-to-male giving evidence of a 1QS connection? Below are a range of possibilities for a context that might underlie a man giving property to another man without receiving money in return. Each has allusions in ancient texts.¹¹⁴

- a ruler or patron gives a grant of land to a retainer
- a man with no heirs gives an inheritance to a servant
- a deed of inheritance to a son-in-law

¹¹² 1QS 6.17-20, quoted above.

¹¹³ Cross/Eshel, DJD 36, 505.

¹¹⁴ For a ruler or patron giving a grant of land to a retainer, compare Herod the Great giving land to Jacimus the Babylonian (*Ant.* 17.29-30). For an estate going to a servant, compare Abraham’s wealth expected to be inherited by his servant Eleazar (Gen. 15.3). For giving to a son-in-law, compare Tobit 8.21 and 10.10: “[Raguel] handed over to Tobit Sarah his wife and half of all his goods” (10.10). For jubilee year conveyance of land without payment compare Lev. 25.28. (For possible echoes of sectarian interest in implementing jubilee year provisions compare Luke 4.17-21 and Matt. 6.12.) For support for a revolutionary leader, charitable giving, gift made under threat, etc. compare parables in the Gospels addressed to wealthy persons alluding to life-threatening consequences if wealth, including land, is not voluntarily given to “the poor” (Matt. 19.27, 29; 21.21; Luke 12.16-21; 16.9-25; 19.8-9), etc.

- a transfer of land related to a jubilee year practice
- repayment of a debt
- a tax payment
- charitable giving
- support for a revolutionary leader
- a gift made under compulsion or threat (not necessarily distinguishable from the previous three)
- a variant of a practice in 1QS, but not 1QS
- a new member giving over property to a community officer as enjoined in 1QS

Is every alternative above, except the last, excluded or unreasonable as a possible background to KhQ1?

12.7. The fact is the visible text of KhQ1 says nothing about a community or about Eleazar representing anyone other than himself. Nevertheless, a lacuna at the end of line 3 allows for one more word following Eleazar's patronymic and possible place of origin, compared to the spacing of equivalent wording associated with Ḥoni of line 2. This extra word at the end of line 3 could name a title or adjective for Eleazar, or alternatively a purpose for the gift. However, it was previously noted that lines 4, 5, 6, and 7 start with **אָא**, **אָא**, **אָא**, and **אָא** respectively, and that this suggests that the lines may not have been the same lengths. If that is so, there is no actual reason to assume an additional word at the end of line 3.

12.8. Is the find site, Qumran, itself proof of a 1QS context or interpretation of KhQ1? The reasoning would be: Qumran was a site inhabited by persons practicing 1QS; the ostrakon was found at Qumran; therefore KhQ1 reflects 1QS. The problem with this line of reasoning is it has not been confirmed (as distinguished from argued to be plausible) that 1QS was practiced at Qumran. In the excavations of Qumran that have been conducted to date, no archives or records of a religious community (e.g. membership lists, financial records, dated documents) have been identified at Qumran, either at the site or in the caves. In 1997 Cross/Eshel suggested that KhQ1 itself was such a document from the archives of the hypothesized community at Qumran.¹¹⁵ But Cross/Eshel abandoned this claim in DJD 36 in 2000.¹¹⁶ The best case for such a text among the finds in the caves, the fragmentary 4Q477 "Register of Rebukes"—with its listing of infractions of three named individuals in a manner reminiscent of a procedure described in the Community Rule—is not confirmed to refer to persons located *at Qumran*, nor is the text's genre certain. If 4Q477 does refer to persons at *Qumran*—which cannot simply be assumed—a question necessarily arises: why has only one of this kind of text emerged from the caves at Qumran and not more, if the texts in the caves reflect c. 150-200 years of a 1QS-organized group's habitation at the site as many scholars suppose?

¹¹⁵ *Cross/Eshel*: "It is not impossible that in the second year of the Revolt (if we prefer this dating), papyrus was in short supply ... The donor is entering a communal sect which shared all of its possessions. This interpretation explains the gift of an estate ... recorded in an ostrakon which was once in the archives of the Qûmran community, and ended up in a dump outside the perimeter wall of the site" (1997 [*IEJ*], 26).

¹¹⁶ *Cross/Eshel*: "we must suppose that the ostrakon is not the legal document itself, but a draft of the deed of gift" (DJD 36, 500).

12.9. Furthermore, all archaeologists and scholars today suppose some inhabitants of Qumran of the era did not practice 1QS; for starters this is the conventional understanding of Qumran's Period III. Some have also argued that Qumran's periods Ia and Ib involved inhabitants unrelated to 1QS (Bar-Adon, Magen, Drori, Humbert, Garcia Martinez).¹¹⁷ Some have thought that the inhabitants of Qumran at the end of Period II were zealots who had replaced sectarians of the scrolls at Qumran.¹¹⁸ Cross himself elsewhere seems to express some uncertainty concerning the identity of those at Qumran at the time the Romans arrived in 68 CE.¹¹⁹ Is it possible that non-1QS-practicing inhabitants of Qumran might have left writing behind at Qumran? If so, can KhQ1/KhQ2 be excluded as belonging to them?

13. Palaeography and dating of KhQ1/KhQ2.

13.1. No writing found at the actual site of Qumran has yet been identified as matching any of the hundreds of scribes who produced the literary texts in the caves, nor has distinctive phrasing or wording associated with a text in the caves turned up in any writing found at the site. The present ostraca do nothing to change this situation. The shape of the *bet* of KhQ1 and KhQ2 is distinctive with an exaggerated "tick". In the huge quantity and variety of scribal hands represented in the literary texts found in the caves at Qumran no such *bet* has been identified. Based on this point alone it appears that the writer of KhQ1 was not a copyist of any of the texts found in the caves. Davies, Brooke, and Callaway correctly note that "the script [of KhQ1] bears no resemblance to the beautiful and usually skilled hands known from the manuscripts from the caves".¹²⁰ These ostraca only deepen the questions concerning the circumstances by which huge numbers of literary texts with their astonishingly diverse variety of professional scribal hands came to be deposited in the caves near Qumran.

13.2. On palaeographic grounds Cross/Eshel claim to know a 38-year maximum range of possibility for the date of writing of KhQ1/KhQ2. They write: "The script on the ostraca is Late Herodian. Cross has defined 'Late Herodian' as the period between 30-68 CE. The ostraca are penned in a

¹¹⁷ P. Bar-Adon, "The Hasmonean Fortresses and the Status of Khirbet Qumran", *Eretz Israel* 15 (1981), 349-52 (Heb.; Eng. summary p. 86); J.-B. Humbert, "L'espace sacré à Qumrân. Propositions pour l'archéologie", *Revue Biblique* 101-2 (1994), 164-214; F. Garcia Martinez, *The Dead Sea Scrolls Translated. The Qumran Texts in English* (2nd edn, Eng. trans.; Leiden: Brill, 1996), xli. According to a report in 1994, Y. Magen and A. Drori proposed that the Hasmoneans "settled residents [at Qumran], who were loyal to them ... perhaps demobilized soldiers, possibly under the direct ownership of the Hasmonean family itself ... [Later, 40 to 37 BCE] a lot of the fighting was in the Jordan valley, and we see the fire at Qumran dating from its capture by Herod" (A. Rabinovich, *Jerusalem Post*, May 8, 1994, quoted by Z.J. Kapera in Kapera [ed.], *Mogilany 1995. Papers on the Dead Sea Scrolls offered in memory of Aleksy Klawek* [Cracow: Enigma Press, 1998], 86).

¹¹⁸ Zealots at Qumran at the end of Period II was urged by e.g. Allegro in the 1950's (G. Vermes, *The Complete Dead Sea Scrolls in English* [Middlesex, England: Penguin, 1997], 584).

¹¹⁹ Cross: "There is some likelihood that the Essenes, at least in part, put up resistance [in 68 CE]. Certainly someone resisted the Romans, using Qumrân as a bastion" (F.M. Cross, *The Ancient Library of Qumran* [3rd edn; Minneapolis: Fortress Press, 1995], 62 n. 2).

¹²⁰ P.R. Davies, G.J. Brooke, and P.R. Callaway, *The Complete World of the Dead Sea Scrolls* (London: Thames & Hudson, 2002), 186.

vulgar, semi-formal style, with an occasional cursive lapse.”¹²¹ Cross/Eshel suggest “year two” of KhQ1, line 1 refers to Year 2 of the Jewish Revolt, that is, 67 CE.¹²² Cross/Eshel refer to “palaeography, which places the ostracon in the mid-first century CE”, as if this is a fact.¹²³

13.3. But there is a methodological problem in Cross’s palaeographic datings of texts in Vulgar semiformal. According to Cross, “the Vulgar semiformal is a crude, simplified derivative of the Herodian formal character.”¹²⁴ A premise in Cross’s system is that “Herodian formal” and its derivative, Vulgar semiformal, started c. 30 BCE.¹²⁵ No evidence was ever set forth that the scribal writing hands termed “Herodian formal” started that late (i.e. at the start of the Herodian period), but the assumption that this is so has influenced hundreds of palaeographic datings of Qumran texts in DJD editions. This belief, without positive evidence, has been impervious even to directly contradicting information. For example, in 1968 Naveh reported Vulgar semiformal Hebrew writing on Alexander Jannaeus coins of 83 and 78 BCE—before the Herodian period.¹²⁶ According to Cross, Vulgar semiformal is derivative from “Herodian formal”. But if “Herodian formal”/Vulgar semiformal were in routine use decades earlier than they are supposed to have begun to exist in Cross’s system—as the Alexander Jannaeus coins testify—then there is no basis for certainty that true dates of Qumran texts in “Herodian formal” are as late as their published palaeographic dates. Curiously, although Naveh’s 1968 report has never been contested or refuted, it seems never to have affected a Qumran text palaeographic dating in any DJD edition published in the decades since then.

13.4. Cross/Eshel also characterized the script of KhQ1/KhQ2 as sharing traits with the script of the Copper Scroll and as contemporary to that text on the basis of palaeography.¹²⁷ In an earlier study,

¹²¹ Cross/Eshel, DJD 36, 497.

¹²² Cross/Eshel, DJD 36, 500. Cross: “Particularly noteworthy is a recently discovered ostracon from Qumrân ... dated ‘in the second year ...,’ presumably the second year of the First Jewish Revolt against Rome (67 CE)” (Cross, “Palaeography and the Dead Sea Scrolls”, in P. Flint and J. VanderKam [eds], *The Dead Sea Scrolls after Fifty Years* [Vol. I; Leiden: Brill, 1998], 379-402 at 382).

¹²³ Cross/Eshel, DJD 36, 500.

¹²⁴ Cross, DJD 3, 217 n. 1. Similarly Cross, “Development of the Jewish Scripts”, 174.

¹²⁵ Cross: “The term *Herodian* is used here and throughout our paper to apply to the era 30 B.C. to A.D. 70 ... At the same time, it applies fittingly to a stage of the formal script, which, owing to the emergence of a complex of new characteristics at the end of the Hasmonaean era, has its own style and integrity...” (Cross, “Development of the Jewish Scripts”, 173).

¹²⁶ Naveh: “The [coins’] inscription is in the Jewish (so-called square Hebrew) script and in the Aramaic language. It consists of מלכא אלכסנדרוס, and the word שנה followed by the numerals כ and, more often, כה. These are dated coins of the 20th and 25th years of Alexander Jannaeus, corresponding to 83 and 78 B.C. respectively ... the legend is written in the ‘vulgar semiformal’ in Cross’s terminology ... The closest parallels to these letters are to be found on ossuaries. As the latter are generally attributed to the Herodian period and as the earliest known vulgar semiformal examples do not antedate this period, the palaeographical significance of this dated Hasmonaean inscription is quite obvious” (J. Naveh, “Dated Coins of Alexander Jannaeus”, *Israel Exploration Journal* 81 [1968], 20-25 at 21-23).

¹²⁷ Cross/Eshel: “The script of both ostraca [KhQ1/KhQ2] is Late Herodian, penned in a vulgar semi-formal style, and it shares many traits with the script of the Copper Document, a vulgar semi-

Cross concluded there was a 50-year maximum range of possibility on palaeographic grounds for the Vulgar semiformal writing of the Copper Scroll, 25-75 CE.¹²⁸ This was an argument cited by Cross/Eshel for dating KhQ1 to the 1st century CE. In fact nothing in actual data rules out a 1st century BCE date for the Copper Scroll on palaeographic grounds,¹²⁹ but that is not the important point. The important point is that, as noted by Callaway, the script of the Copper Scroll is not closely like that of KhQ1 except in very general ways.¹³⁰ Therefore it is doubtful that either of these two texts could be used to precisely date the other, even if a specific date were confirmed for one of them.

13.5. Nor are there archaeological grounds favoring a dating of KhQ1/KhQ2 to Qumran's Period II (1st century CE) over Qumran's Period Ib (1st century BCE). Pottery was found with KhQ1 and KhQ2, none yet published.¹³¹ In a recent communication, the excavator, James Strange, told the present author that none of this pottery found with the ostraca can be dated to Period II in a manner that excludes Period Ib.¹³²

13.6. There is, however, an archaeological argument against the separate suggestions of Cryer and Callaway for dating KhQ1/KhQ2 *later than* Qumran's Period II.¹³³ there are no known dumps of Period III or Bar Kokhba-era material outside the buildings' walls at Qumran.

formal hand of the same date" (Cross/Eshel 1997 [*IEJ*], 17-18). Similarly Cross/Eshel, "The Missing Link", 69 n. 6.

¹²⁸ Cross: "The Herodian semiformal hand cannot be dated with quite the precision with which the elegant, formal Herodian scripts can be analysed ... the script [of the Copper Scroll] is to be placed in the second half of the Herodian era, that is, within the broad limits A.D. 25-75" (Cross, *DJD* 3, 217).

¹²⁹ This statement is based on the author's study of the specifics cited by Cross in *DJD* 3.

Again, see note 126.

¹³⁰ Callaway noted correctly that "[t]his script [of the Copper Scroll] bears little detailed resemblance to the script on ostracon no. 1 [KhQ1]" ("A Second Look at Ostracon No. 1", 153 n. 6).

¹³¹ For description see note 3.

¹³² James Strange, personal communication, 19 September 2003.

¹³³ Cryer 1997 (*SJOT*); Callaway, "A Second Look at Ostracon No. 1". Cryer identified the *bet* in KhQ1/KhQ2 with "figure-2" cursive *bets* of early 2nd century CE documentary texts and argued that this dated KhQ1 no earlier than the first attested occurrence of the figure-2 *bet* elsewhere, namely c. 120 CE. Cryer: "Cross and Eshel admit that the Beths on both of the Qumran ostraca are atypical, as they have a 'tick' which 'descends to the rounded, clockwise stroke'. In actual fact, that 'tick' is reflected in the little elaboration at the top of some post-Herodian *cursive* Beths ... It might be added that the loop where the lower stroke of the Beth doubles back on itself and extends beyond the letter on the right is visible in the second ostracon published by Cross and Eshel [KhQ2]..." (p. 237). But *contra* Cryer, the base of the KhQ2 *bet* does not loop or double back on itself. The KhQ1/KhQ2 *bet* is not a figure-2 *bet* but is a predecessor of it. This removes Cryer's principal argument for the late dating. Callaway proposed that the names Ḥoni and Eleazar in KhQ1 are not only paralleled in Bar Kokhba-era texts but reflect identical persons. But the common occurrence of the names, the find site of KhQ1 (Qumran), and the lack of known post-Period II dumps outside the buildings' walls at Qumran make this proposal of Callaway unlikely.

13.7. A different palaeographical description of KhQ1 was given by Yardeni, who—it should be noted—has done the palaeography on nearly all of the other Dead Sea documentary texts in Hebrew and Aramaic. According to Yardeni the writing of KhQ1/KhQ2 is “early Herodian semi-cursive”.¹³⁴ Converted to absolute dating this suggests c. late 1st century BCE.¹³⁵ That is, if Yardeni had done the editions of KhQ1 and KhQ2 instead of Cross/Eshel, KhQ1/KhQ2 would be regarded by Qumran scholars as from either the end of Qumran’s Period Ib or early Period II (depending on the exact date of writing of the ostraca and for the end of Qumran Period Ib). And if KhQ1 and KhQ2 were dumped outside the buildings of Qumran from a clearing of debris from a destruction (as seems very reasonable) then it is more likely that KhQ1 and KhQ2 are from the *end* of a Qumran period than a *beginning* (since debris of archaeological destructions tends to be items in use at the time of the destruction). By this reasoning it is more likely that KhQ1/KhQ2 are from the end of Ib than early II.

13.8. In light of the names of the parties of KhQ1, Ḥoni the giver and Eleazar the recipient, it is interesting that the name “Eleazar” was found at Qumran by de Vaux on a bowl among hundreds of others at locus 86, all from a Period Ib context. Like KhQ1, the writing on this bowl from locus 86 was given a 1st century CE palaeographic date by Cross.¹³⁶ However the true date of the locus 86 bowl is known on archaeological grounds to be 1st century BCE, earlier than its palaeographic dating.¹³⁷ No other name or writing was reported found on these hundreds of locus 86 bowls. According to de Vaux the name had been scratched into the clay by the Period Ib potter prior to firing.¹³⁸ The meaning of “Eleazar” on the locus 86 bowl is obscure, especially since these bowls, stored in large numbers, may have been the ancient equivalent of disposable paper plates, used only

¹³⁴ Yardeni 1997 (*IEJ*), 233.

¹³⁵ Note that absolute datings of the cursive and semicursive sequences by Cross and Yardeni are arrived at by different and arguably sounder bases than the absolute datings of the formal and semiformal hands (because there is a database of independently dated writing in semicursive and cursive hands in the 1st century BCE and 1st century CE).

¹³⁶ Cross: “an inscribed bowl from 86/89 seems clearly to be dated palaeographically to the first century A.D.” (F.M. Cross, *The Ancient Library of Qumran* [3rd ed., 1995], 62 n. 3).

¹³⁷ R. de Vaux, *Archaeology and the Dead Sea Scrolls. The Schweich Lectures of the British Academy 1959* (Eng. trans.; London: Oxford University Press, 1973), 11-12; P. Lapp, *Palestinian Ceramic Technology 200 B.C.-A.D.* (New Haven, Conn.: American Schools of Oriental Research, 1961), 50-51; J.-B. Humbert and A. Chambon, *Fouilles de Khirbet Qumrân et de Aïn Feshka* (NTOA Series Archaeologica 1; Fribourg: Éditions Universitaires, 1994), 318-20; J. Taylor and T. Higham, “Problems of Qumran’s Chronology and the Radiocarbon Dating of Palm Log Samples in Locus 86”, *Qumran Chronicle* 8 (1998): 83-95; R. Bar-Nathan, *Hasmonean and Herodian Palaces at Jericho. Final Reports of the 1973-1987 Excavations. Volume III: The Pottery* (Jerusalem: Israel Exploration Society, 2002), 203-4; J. Magness, *The Archaeology of Qumran and the Dead Sea Scrolls* (Grand Rapids, Mich.: Eerdmans, 2002), 123.

¹³⁸ De Vaux: “The crockery [at locus 86/89] (especially the plates and beakers) clearly belongs to the pottery group of Period Ib and is different from that of Period II which has been found on the upper level of the large room ... The name אלעזר had been scratched on one of the bowls before it was baked and, so Milik argues, the lettering is typical of the writing of the first century A.D. [i.e. it is typical of Qumran texts which Milik and Cross thought were 1st century CE]. I am doubtful whether five letters (scratched, and not traced with a pen as are the manuscripts to which he compares these letters) can overthrow the sum total of the archaeological indications” (de Vaux, *Archaeology*, 12).

once after removal from the store-room. That is, the bowl with Eleazar's name on it might never have been used. Who was this Eleazar of Period Ib? Was he the potter? Or was this Eleazar a person at Qumran for whom the potter made the bowls (and the potter was marking a pile or allotment)? Was Eleazar of the locus 86 bowl Eleazar of KhQ1? In this case KhQ1 would be from Qumran Period Ib, just like the locus 86 bowl with its similarly erroneous 1st century CE palaeographic date. Or is this a coincidence of unrelated Eleazars? Since Eleazar is a relatively common name this is not known, but the coincidence is curious.

13.9. Additional arguments could be cited in support of a Period Ib dating of KhQ1/KhQ2.

- There seem to be more Ib dumps than II dumps outside the walls at Qumran.
- The lack of systematic medial/final distinctions for any letter in KhQ1/KhQ2 seems “early” (and in agreement with the Qumran practice alphabet from Period Ib).
- The Qumran practice alphabet (KhQ3), found in a Period Ib dump outside the north wall at Qumran, although from a different writer and with different forms of letters than KhQ1/KhQ2, is similar to KhQ1/KhQ2 in originating from an unskilled scribe and in being an ostrakon found in a dump outside the buildings' walls. Since the Qumran practice alphabet is from Period Ib, by analogy KhQ1/KhQ2 might be as well.
- By separate argument the compositions and copies of the Community Rule texts found in the caves near Qumran appear to be from the time of Qumran's Period Ib.¹³⁹ If the giving of KhQ1 *does* reflect 1QS, Period Ib might be suggested as the first context to consider for KhQ1, in the absence of evidence suggesting differently.

However none of these points are decisive. To be clear, the argument here is not that KhQ1 is from Period Ib but that there is no basis for Cross/Eshel's exclusion that it could be.

14. Summary and conclusion concerning KhQ1.

KhQ1 appears to reflect a deed of gift, but it could be some kind of record or statement of a transaction that has already occurred. KhQ1 and KhQ2 are dated to either Qumran's Period Ib or Period II. The person or persons who wrote KhQ1 and KhQ2, like the writers of all other known writing found in the buildings of Qumran, did not produce any of the scrolls known from the caves, since no match is identifiable in the handwritings. Nothing in KhQ1 or KhQ2 testifies to a connection with 1QS or any other of the texts found in the caves—not in genre, content, or identity of the scribe. In favor of a community-gift interpretation of KhQ1 are indirect arguments: the find site at Qumran raises the question of a possible relationship to the 1QS/4QS texts due to association with the same site, and the male-to-male giving. These are intriguing but insubstantial; there is no “smoking gun”. Important differences have been noted between the 1QS giving and KhQ1—although due an unclear understanding of how ancient texts related to reality, these negative arguments could be less strong than they seem. In the end it is neither confirmed nor disconfirmed that KhQ1 is concerned with turning over of property to a religious community. If this conclusion is

¹³⁹ G. Doudna, *4Q Pesher Nahum: A Critical Edition* (Sheffield: Sheffield Academic Press, 2001), 707-710.

frustratingly equivocal it is because the evidence is insufficient to say more, at least so far as is known to the present study.

15. A postscript: KhQ2 (Ostrakon No. 2).

15.1. Cross/Eshel reported that whereas KhQ1 is “a thick bodied sherd of a larger jar”,¹⁴⁰ “the smaller ostrakon [KhQ2] is inscribed on a relatively thin sherd”.¹⁴¹ Unlike KhQ1, no edition of KhQ2 has been published other than that of Cross/Eshel in 1997 in *Israel Exploration Journal*, repeated in DJD 36 in 2000. For KhQ2 only a single black-and-white photograph has been published, identically in *IEJ* in 1997 and DJD 36 in 2000. As was the case with KhQ1, some of the Cross/Eshel readings in KhQ2 call for correction. Here is Cross/Eshel’s reading of KhQ2 followed by my readings of the same lines.

KhQ2. Cross/Eshel transcription

Line		
1] (?) [שׁ ק [
2] (?) []ת הו[¹⁴²
3	Jehose]ph son of Nathan[]יהוס פ בנ נת[
4	his [s]ons from ‘En[Gedi (?)]בני מעי[

KhQ2. New transcription

Line		
1	...]q’[...]קא[
2	... the prie]st(?), Ho[ni ...]הנ חו[ני
3	... Jose]ph son of Natha[n(ael) ...]יהוס פ בנ נת[נ
4	...]ny and Reue[1 ...]ני ורעא[ל

15.2. In line 1, where Cross/Eshel read שׁ ק [the second letter should be read as *aleph*. There are no other examples of *sin/shin* in KhQ2, but both of two cases in KhQ1 (line 1, twice) disagree in form with the present letter of KhQ2 in not having a sharp angle or “point” at the bottom. Also, the word-separation space transcribed by Cross/Eshel appears mistaken; the letters קא appear to belong to the same word, likely a proper name from the context. One possibility for such a proper name would be חזקא read by Milik at Mur 22.1-9 ii 4; another is נתקא read by Milik at Mur 74.3.

15.3. In line 2, the Cross/Eshel reading of *he* for the letter starting the second word is mistaken; the letter is *het*. *Het* in KhQ1/KhQ2 is written with the left and right verticals of equal height (as in the

¹⁴⁰ Cross/Eshel, DJD 36, 497.

¹⁴¹ Cross/Eshel, 1997 (*IEJ*), 17.

¹⁴² Whereas the Cross/Eshel transcription in *IEJ* in 1997 has a bracket preceding the *taw* in line 2, DJD 36 has an incongruous *ayin* and no bracket in this position. It is clear this *ayin* in DJD 36 is a typographical mistake.

present letter), whereas *he* has a structure in which the left vertical never is above the crossbar but the right one always is, and the crossbar extends intentionally to the left beyond the left vertical. The crossbar in the present letter goes out to the left very slightly but this was not intentional (if it were intentional it would have been more emphasized). The 1997 drawing of Cross/Eshel is mistaken in having an “inked triangle” at the top left of the letter above the crossbar; only the straight-line crossbar is there. The final letter of line 2 is *taw*. From context this second word]חנ[is likely to be a proper name which could be חנ[ני, “Ḥōni”. Cross/Eshel read the first letter of line 2, a vertical stroke with a prominent left foot, as ת[. If *taw* were correct the word might be בת[, “daughter of <Proper Name>”, but the *taw* reading is in error. The one clear *taw* in KhQ2 in line 3 has no left foot. The blot at the base of the left vertical of the line 3 *taw* is the end of the lower left vertical curving slightly to the left but without a foot.¹⁴³ Nor do *taws* in KhQ1 anywhere have fully-developed left feet.¹⁴⁴ The shape and prominent left foot of KhQ2, line 2, #1 is in exact agreement, however, with the *nuns* in KhQ2 in lines 3 and 4. Line 2, #1 is therefore a *nun*, with a mark from a preceding letter touching at mid-height. (For medial *nun* form in final position compare בנ one line below in line 3 [and in KhQ1, נתנ of line 2 and אילנ of line 8].¹⁴⁵) By process of elimination the letter preceding this *nun* of line 2 must be *he*. No other letter matches.¹⁴⁶ A proper name might be expected based on context, but it is difficult to identify a proper name ending in -הנ. The word is suggested here to be כו[הנ (ה), “priest”. Though this restoration is conjectural, there seem to be no good alternatives. כו[הנ (ה) would not be out of place in a list of proper names; presumably it would modify a preceding proper name.

15.4. In line 3, as just noted, the *taw* (#5) does not have a foot to the left. The spot of ink at the base of the left vertical is the bottom of the vertical which has curved only slightly to the left, in the same form as the *taws* of KhQ1. There is no final-form *nun* at the end of line 3 as drawn and transcribed by Cross/Eshel. There could be a trace of ink consistent with a medial *nun* (i.e.]נתנ[) although this is ambiguous in the photograph. A medial *nun* is expected: compare בנ of line 3 and medial נ in final position in line 3.

¹⁴³ Cross/Eshel incorrectly interpreted the *taw* of KhQ2, line 3 as having a short, distinct foot to the left at a 90-degree angle; see the drawing in *IEJ* in 1997 and the *taw* in the Cross/Eshel script chart for Ostracon No. 2 in *IEJ*, Fig. 3, line 1. Close study of the DJD 36 photo of KhQ2 shows the left vertical of that *taw* slants slightly to the left at about a 45-degree angle (not a 90-degree angle). Underneath the slanting lower part of the vertical is a non-letter dark spot (in texture like the background of the ostracon and unlike the ink of the letter strokes) which has given the illusory appearance of a 90-degree-angle foot. The slant at the end of this KhQ2 *taw*'s left downstroke is in agreement with *taws* in KhQ1. The foot of the letter starting KhQ2, line 2, agrees only, and perfectly, with the *nuns* of KhQ2.

¹⁴⁴ *Taws* in KhQ1: line 1, #4 and #6; line 2, #7; line 4, #2; line 5, #2; line 6, #3 and #10; line 7, #3 and #9; line 10, #4; line 11, #3. KhQ2: line 3, last letter.

¹⁴⁵ The two final-form *nuns* drawn and transcribed by Cross/Eshel at the ends of lines 3 and 4 of KhQ2 are both non-existent. See discussion below.

¹⁴⁶ The letter cannot be from a preceding *bet* giving בנ, however much that word might be anticipated, since if the mark were from the top of a *bet* the lower part of the *bet* should also be visible, but it is not. Also and independently, the horizontal mark preceding line 2, #1 does not seem consistent with the top of a *bet* which would have an emphasized tick going upward to the right.

15.5. In line 4 Cross/Eshel have a word-division space following [נײ] , but a better placement of the graphically ambiguous word division seems to be [נײ-ו] . The ending $-ny$ is likely the end of another proper name (another Hōni?) followed by “and”. However, it could also be read [נײו] , “and his sons: <PN> and <PN>, etc.” The Cross/Eshel reading of #4 as *mem* is mistaken; that letter is a *resh*.¹⁴⁷ The Cross/Eshel reading of #6 as *yod* is also mistaken. That letter is *aleph*. The main downstrokes of *yod/waw* in KhQ1/ KhQ2 are vertical or nearly vertical,¹⁴⁸ but the right stroke of #6 is a slope, starting from the top of a vertical and going down at an angle to the right, indicating the N-shape of *aleph*.¹⁴⁹ Finally, there is no ink from a final-form *nun* at the end of line 4 as represented in the Cross/Eshel drawing and transcription. There are no final-form *nuns* at all in KhQ2, and based on [הנ] of line 2 and [בנ] of line 3 there is no expectation that there should be. The line 4 letters seem to give part of another proper name, [רעאנל] , “and Reu[el]”, corresponding to the biblical Hebrew רעואל (or, if the *waw* is attached to the preceding word: “and his sons: Reu[el...”). There is no reading of Ein Gedi or any other place name in KhQ2.

15.6. KhQ2 therefore appears to be almost entirely proper names: [קא] of line 1, [הו] of line 2, [פ] of line 3, [נת] of line 3, [נײ] of line 4, and [רעא] of line 4 are probably proper names. If the first word of line 2 is [הכוהנ] , “the priest”, that too can be consistent with a list of names if it is associated with a preceding proper name. The lack of writing below line 4 means these lines are at the end of a text. What these names mean or represent is unknown, though their position at the end of a text is consistent with Dead Sea documents which typically end with names of signatories and witnesses. However these names of KhQ2 are not actual signatures since they were written by the same writer.

15.7. KhQ1 and KhQ2, although reflecting identical structures of letters in most if not all cases, nevertheless show minor differences in the writing of the letters:

- A difference in writing the head of *qof*, if Yardeni’s description is correct. According to Yardeni the *qof* at KhQ1, line 4, #5 is semicursive in form, whereas the *qof* of KhQ2, line 1, #1 has a different, formal skeleton.

¹⁴⁷ The faint mark above the right shoulder of KhQ2, line 4, #4 is non-letter background. The tick on the left is drawn separately from the roof in agreement with the way *resh* is drawn in KhQ1, as opposed to *dalet* of KhQ1 which has the left ear and roof drawn in a single stroke.

¹⁴⁸ The *yod* of KhQ1, line 2, #2 is not an exception (see note 35), nor is the *yod* of KhQ1, line 8, #6 (in this letter, the slope down to the right, then angle to straight down, drawn by both Cross and Yardeni is probably illusory; the stroke seems to be a defaced single line straight down).

¹⁴⁹ *Yods* of KhQ1 (1.7; 2.11; 7.7) as well as at KhQ2 (4.2) illustrate the main downstrokes of *yods*, which are not like the angled stroke of line 4, #6. The ‘inverted-V’ shape of line 4, #6 is comparable to the *alephs* of KhQ1 (of lines 3, 5, 6, 7, and 8). The right vertical stroke of the present *aleph* in KhQ2 is faint but seems visible. Compare similar faint vertical letter strokes on KhQ2: the *qof* and *aleph* of line 1 and the vertical of *resh* of line 4. Compare also the angle at the bottom of the N-shape of line 4, #6 with the identical angle of the *aleph* above in line 1, and especially also the *aleph* of KhQ1, line 5, #1.

- In KhQ1 (line 3) נב is written with final-form *nun* and ligature. In KhQ2 (line 3) the same word is written נב with medial-form *nun* and no ligature.
- The *bets* of KhQ1 have a rounded, curved stroke down to the base stroke without a right-shoulder angle. But the one *bet* of KhQ2 (line 3, #2) appears to have an angle in its right shoulder.
- The verticals of the *het* in KhQ2 are higher above the crossbar than in any *het* in KhQ1 (including at KhQ1 line 2, #4, where the ears are less high than they appear in the DJD 36 photograph, as the color photos make clear). The left verticals of most *hets* in KhQ1 also tend to be slightly concave, whereas in KhQ2 the left vertical of the *het* is ramrod straight.
- In the *nuns* of KhQ2 there is a long left foot at a 90-degree angle. In KhQ1 the *nun* foots are shorter in length and written at a slant less than a 90-degree angle. Or to put it another way, visually the four *nuns* of KhQ2 have more pronounced foots than any of the numerous *nuns* of KhQ1.

Although these differences seem to suggest different writers with similar or identical scribal training, in another case Yardeni suggested that differences in letters in ostraca which reflect the same distinctive letter structures could indicate the same writer writing at different times and circumstances.¹⁵⁰

¹⁵⁰ Yardeni commenting on four ostraca written in Aramaic: “The script of the ostraca is generally the typical Jewish cursive script known from documents and ossuaries from the end of the Second Temple period to the end of the Bar Kokhba revolt. It is reminiscent of the cursive script in the finds from Masada and from Wadi Murabba‘at, though the shape of several of the ostraca letters is not identically attested in them. The occurrence of the same unique letter shapes in all four ostraca may imply they were written by one person. Differences in time and in writing conditions may explain the differences in size and execution of the letters in the four ostraca. The script was executed with a free hand, and the writer was apparently well trained in the common writing style of the period” (Yardeni, “New Jewish Aramaic Ostraca”, 147).