Dušek, Jan

Les manuscrits araméens du Wadi Daliyeh et la Samarie vers 450–332 av. J.-C.

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To paraphrase the Zohar, even ancient texts need luck. Indeed, some are far luckier than others; there are cuneiform texts that have been sitting unedited for decades, but the relatively dry Samaria Papyri have now been blessed with two excellent editions. After a series of studies by F. M. Cross and his student D. M. Gropp, which culminated in Gropp’s excellent edition of the best-preserved of these texts in the DJD series in 2001, the texts have now been reedited with a rich, full commentary and extensive discussion by Jan Dušek.

The book consists of a history of research regarding the texts from Wādī Dāliyeh, a reconstruction of the legal formulary found in the texts, a complete edition of all of the papyri found there, and a synthetic discussion of the history of Samaria and Judea in the Persian period, with particular attention paid to some of the thornier issues in this history.

I will say up front that this book is indispensable. (This, of course, makes the list price particularly unfortunate.) The work has clearly emerged from sustained and intensive first-hand work on the papyri themselves. The analyses presented are thorough and clear,
and all major problems facing interpreters of these difficult and fragmentary texts are subjected to fresh investigation.

This volume will be indispensable for a number of reasons. The first and most obvious reason is that this is the first full publication of many of the texts. Dušek provides a full commentary on thirty-seven texts, whereas Gropp’s DJD edition provided photographs only for numbers 11r, 11v, 12, 13r, 13v, 14, 16, 17, 19, 20, 21, 22, 23r, 23v, 24, 25, 26, 27, 28r, 28v, 29r, 29v, 30r, 30v, 31, 32, 33r, 33v, 34, 35r, 35v, 36, 37, 38. Gropp’s DJD edition is accompanied by a helpful concordance (compiled by Yehudit Shemesh), but Dušek goes one better and provides a very helpful glossary that also lists all the occurrences of the words defined, as well as a concordance of proper names, toponyms, and month names.

Second, Dušek’s primary focus is actually the historical reconstructions made possible by the papyri, not the philological or epigraphic study of the texts themselves. This is not to slight his treatment of those issues: the philological commentary is thorough and sensible, and Dušek provides a typological discussion of the orthography of every letter (468–73). As will be seen, however, the book clearly conceives of these matters as preliminaries to the ultimate goal of the historical work.

Third, all previous editions available were by either Cross or his student. Certainly the texts could not have been in better hands: Cross clearly does not need my recommendation as an epigrapher and a philologist, and Gropp’s twenty years of work yield mature reflection and creative insights visible on every page of his DJD volume. Yet Dušek’s edition shows once again the value of collaborative research. In reading the new edition, it becomes clear to what degree the initial views of Cross have become accepted wisdom, despite the fact that others have not had the opportunity to investigate the texts on the same first-hand basis as Cross did. Gropp certainly did improve on his teacher’s readings and interpretations at a number of points, but the fundamentals of the interpretations remained the same. It is therefore quite valuable to have a very different set of eyes now trained so intensively on these texts. Those views that have been seconded here now stand on firmer ground, and those that have been questioned may have been in need of reevaluation.

The book begins with interesting histories of research regarding both the papyri and the bullae found at Wādi Dāliyeh, followed by a chapter on the numismatics. All three of these sections are useful, not providing a full catalogue of everything ever said regarding these texts and artifacts, but certainly describing and discussing the milestones in their publication and study. Before turning to the texts themselves, Dušek devotes nearly fifty
pages to a reconstruction of the structure of the contracts. His view of the standard structure differs in some important respects from those presented by Cross and Gropp.\(^1\)

Turning now to the texts, the reader will find that some of the readings proposed by Dušek are particularly worthy of attention. For example, the phrase אטיר, which was thought to be attested a few times in the papyri, was deemed significant both for legal history (since it was apparently borrowed from mahir etir in the Neo-Babylonian legal formulary) and phonological history (since the “transcription” of Akkadian /h/ by <כ> is said to show that the latter has a fricative pronunciation in postvocalic position within Aramaic).\(^2\) The phrase no longer exists at all in the corpus according to Dušek’s readings. In 3:3 and again in 25:2, Dušek reads מניד (157–59 and 369–70), and in 3:8 (where Gropp [63] had noted that “the presence of מניד here is odd”) he reads either מנין “amount” or מניד again (163–66). (It should be noted that Gropp had repeatedly observed that “the range of √mkr in Aramaic does not fit the context in the Samaria papyri.”)

Those looking for Babylonian influence in this phrase need not be disappointed, however, since Dušek sees מניד as the reflex of Akkadian ina qâtê (159). Another well-known Babylonianism in the papyri is שחרץ, taken by Cross and Gropp to reflect Neo-Babylonian šīm ħaris “the exact price” or “the stipulated price.”\(^3\) After citing a dissenting voice, Dušek concludes with a summary of J. C. Greenfield’s view, which agrees in its essence with Cross’s, and adds that the Aramaic גמירן דמין is appended to the originally


2. See especially Douglas M. Gropp, “The Language of the Samaria Papyri: A Preliminary Study,” *Maarav* 5–6 (1990): 184. It is likely that this is not compelling; even if מ was not spirantized, /k/ and /x/ share a place of articulation, and if Aramaic מ was a pharyngeal fricative, this was not shared by מ and Akkadian /h/. Exactly the same fallacy—of assuming that the use of מ (in Aramaic) to transcribe a velar fricative (from Luwian) presupposes spirantization—is discussed by Richard C. Steiner, “Variation, Simplifying Assumptions, and the History of Spirantization in Aramaic and Hebrew,” in *ешע למחר: מחקרים בלשון העברית ביבשת ארמית والعربية, ועליהן מחקרים בלשונים נוספים של 학 in Phoenician proves spirantization in the latter language.\(^*\)

3. The first translation is Cross’s (“Samaria Papyrus 1,” 9*); the second is Gropp’s, DJD 27, 35; both references are in Dušek, 122. For further discussion of the details of the loan, see Gropp, “The Language of the Samaria Papyri,” 182–83; idem, “The Samaria Papyri and the Babylonio-Aramean Symbiosis,” 31–32.
Babylonian חשרץ because the latter was likely not understood by Aramaic speakers.

This can be contrasted with the view of Gropp, according to which גמירן דמין is itself a Babylonianism, “calqued on the Neo-Babylonian šīmī gamrūti.”

To give a sense of the many more readings and philological discussions worthy of note, I will mention a few here.

1. In place of the reading חלפא in 7:14 (גנַת לְ חַלָּפָא וּשְׂתָא möchte Lawyers), which goes materially unquestioned by Gropp, who takes it as a noun with the suffixed definite article, Dušek reads לחלף, explaining that חלף is the preposition (attested also in 10:3 and 16:6) meaning “in exchange for” (209).

2. Following a characteristically thorough review of the possibilities for reading, reconstructing, parsing, and interpreting, Dušek concludes that WDSP 14:6 contains the phrase אארך', in which א stands for אמה/אמן (297–99).

3. In the same text, in the previous line, Dušek takes נשכתא, which has served as the basis for speculation regarding the existence of a “Samaritan” temple prior to Alexander, as meaning simply “salles,” (300) undermining such speculation.

Finally, Dušek weighs in on the question of the morphology and syntax of the wordמקבל which appears in the contracts. Gropp (DJD 27, 23 n., 39, and 48–49) points out that a past transaction is being described, and suggests that “the perfect tense [sic] qabbil might have been expected.” To clarify the problem, consider the appearance of מקבל in the

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4 But even assuming that the Samarians could not have explained the etymology of חשרץ, is it likely that they did not understand its function? This raises an important question regarding technical legal terms which floated from one culture, and language, to others, but these types of questions are not the type that occupy Dušek in this book.

5 Gropp, DJD 27, 22.

6 Cf. Hanan Eshel, “תעודה על 14 מאיוור דיילו המקדש בקיר שמורת,” Zion 61 (1996), 359-365. This view of Eshel’s goes back to an earlier article (Eshel, " TreeMap במקורות המדרש השומרוני ובחר " Zôn 56 [1991], 125-136) which drew criticism from Menachem Mor (" תמוק לו ממדרש השומרוני בר זריז זי הממקדש והמדרש, " BM 38 [1993], 313–327; see also Eshel, " TreeMap במקורות בחר בר זריז זי הממקדש והמדרש, " BM 39 [1994], 141-155, and Mor, " TreeMap במקורות שבטי: יישפ במדרש השומרוני: " BM 40 [1995], 43–64). The picture has now been fundamentally changed by Magen’s excavations at Mt. Gerizim, which found a temple Magen dates to the fifth century; cf. also Ingrid Hjelm, “What Do Samaritans and Jews have in Common? Recent Trends in Samaritan Studies,” CBR 3 (2004), 20; Menachem Mor, "(Tree Map במקורות בחר בר זריז זי הממקדש והמדרש, " ביבליות אברכים, מביאים את המקורים ממקרא בבראשית עד חזית פרה תורי ה" תרי הכרוס, מונולוג בלארוסית, דניצ’יא，则נק, ed. Moshe Bar-Asher and Moshe Florentin; Jerusalem: Bialik, 2005), 41-58, esp. the “additional note” (apparently written in 2000 or soon thereafter) on pp. 57-58.

hypothetical denial contemplated in 1.7–8. There the text stipulates what is to happen if the seller says to the buyer that he never sold his slave and never received the money: מַכְבֵּל אֵאָה, יָנָה מַכְבֵּל אֵאָה מַכְבֵּל אֵאָה, rendered by Dušek as “la somme de 35 sheqels, je ne las reçois pas de toi” (118).\(^8\) It seems that here the participle is out of place, and it is the perfect that is expected (comparable to מַכְבֵּל).\(^9\) According to Gropp, Cross therefore suggested that the participle is used as a “narrative past tense,” as in the Aramaic of Daniel; Gropp himself opts instead to analyze מַכְבֵּל as a passive participle rather than the active form.

Dušek (136) rejects Cross’s proposal on the grounds that the syntax of the participle within the Aramaic of Daniel cannot be separated from the larger question of the verbal system within the language of that book; since the verbal system of the SP is fundamentally different from that found in Daniel, according to Dušek, the comparison is invalid.\(^10\) This is an important methodological point: verbal forms do not exist in vacuums but rather within cohesive systems, and the existence of a syntactic structure in one system is not itself sufficient grounds for assuming it could exist elsewhere.

Turning then to Gropp’s own suggestion, Dušek rejects the analysis of מַכְבֵּל as passive on the grounds that “la function de מַכְבֵּל dans les contrats du Wadi Daliyeh est clairement celle d’un verbe transitif” (138). According to Dušek, this is conclusive, since in the language of the SP, as in Egyptian Aramaic, “un verbe à la voix passive ne peut pas avoir de complément d’objet direct, et par consequent être transitif” (138). Gropp does not, in fact, deal with this problem, nor indicate how he understands the syntax in a line such as מַכְבֵּל... מַכְבֵּל בַּל אֵאָה (3:3) if מַכְבֵּל is passive.

In this case, then, Dušek does a good job of clearing the field of proposals that will not work, although he does not offer a new analysis of the line that resolves the original problem (namely, why the participle, not the perfect, appears). It may be that the answer is simple: the line quoted (דָּלָא מַכְבֵּל... מַכְבֵּל) is simply the denial of the normal affirmation of sale, which can be retroverted as מַכְבֵּל... מַכְבֵּל. An apparent parallel may be found in a Hebrew

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\(^8\) This interpretation, which differs from that offered originally by Cross, was suggested by Yochanan Muffs and adopted by Gropp in DJD 27, 41–42 (and note there n. 55).

\(^9\) Compare such formulae in the Elephantine papyri as 'אֲמֹר אָתִי אֶל אֱלֹהִים אָתִי אֲמֹר אָתִי אָתִי אֲמֹר... יָמֹר אָתִי אֶל אֱלֹהִים אָתִי אֲמֹר... ' אֲמֹר אָתִי אֶל אֱלֹהִים אָתִי אֲמֹר... ' אֲמֹר אָתִי אֶל אֱלֹהִים אָתִי אֲמֹר... ' אֲמֹר אָתִי אֶל אֱלֹהִים אָתִי אֲמֹר... ' אֲמֹר אָתִי אֶל אֱלֹהִים אָתִי אֲמֹר... ' אֲמֹר אָתִי אֶל אֱלֹהִים אָתִי אֲמֹר... ' אֲמֹר אָתִי אֶל אֱלֹהִים אָתִי אֲמֹר... ' אֲמֹר אָתִי אֶל אֱלֹהִים אָתִי אֲמֹר... ' אֲמֹר אָתִי אֶל

\(^10\) It should be noted that Dušek apparently wrote his thesis in the École Pratique des Hautes Études in Paris on Système verbal de l’araméen du livre de Daniel à la lumière de l’époque achéménide et du Qoumrân in 2002.
document from the time of Bar Koseba’s revolt (five hundred years later than the SP), which reads, "this *sela’ I receive from you..." Here the prefixed -ת(<תאתת) makes it unambiguous that the *סלוּ is the direct object; the participle, therefore, is most naturally taken as active.11

However, all these readings aside, the focus of the book is, as mentioned, on the historical information to be gleaned from the texts. Dušek synthesizes this information, together with biblical, numismatic, and archaeological evidence, to produce lucid and comprehensive discussions. “Lucid and comprehensive” should not necessarily be taken to mean “correct in all details,” of course, and some of Dušek’s more interesting conclusions will be discussed briefly here.

In his discussion of the governors of Samaria, Dušek clearly would agree with the equally recent verdict of Hanan Eshel that Cross’s reconstructions are in need of revision in light of recent epigraphic and numismatic publications.12 Dušek goes further than Eshel in his revisionist reconstruction, however. Where Cross has reconstructed three governors Sanballat, and Eshel two, Dušek concludes (see esp. 548–49) that there was only one Sanballat, who governed for essentially the entire second half of the fifth century (from before 445 until about 410–407).

11 Magen Broshi and Elisha Qimron, “A Hebrew I.O.U. Note from the Second Year of the Bar Kokhba Revolt,” JJS 45 (1994): 286–94; on 290 they write: “מקבל: i.e., מְקֻבָּל, מַקְבָּל, or מַקַּבֵּל.” I am not sure why they raise the possibility that the form is passive (and why they list only the Aramaic passive, but not active, form as a possibility). The passive form מְקֻבָּל is tempting at first, since in MH it appears with the recipient as the subject ("I have received"). However, in MH there is never a direct object in these constructions. In order to specify what was received, a relative clause is needed (e.g., מוקבל אננה ממקבל). This is also never found in MH with regard to the physical transfer of objects, though it is not clear that such a construction would not in fact be possible.

One difference between the syntaxes of the Samarian Aramaic text and the much later Hebrew one is the word order: in standard Imperial Aramaic, the participle is followed by the independent pronoun which is its logical subject (here, מַקְבָּל, מַקְבָּל; cf. Takamitsu Muraoka and Bezalel Porten, *A Grammar of Egyptian Aramaic* [Handbook of Oriental Studies, Near and Middle East 32; Leiden: Brill, 1998], 292–93), whereas the Hebrew text places the pronoun first. (This syntactic question within Hebrew [contrast את ממקבל with ממקבל את] deserves further study; an important start is to be found in Takamitsu Muraoka, *Meḥqarim ba-Lašon* 4 [1990]: 219–52, esp. 221–28.)

This hinges on the identity of the Sanballat apparently named in the bulla (WD 22) found around WDSP 16, which reads [יהו בן[;elyn פחת שמה[. The importance of this bulla to historical reconstruction is signaled by the eleven pages Dušek devotes to it (321–31). Eshel relies on numismatic evidence as interpreted by Meshorer and Qedar, who identify the סנאבלט named on coins as Sanballat II, on the grounds that the coins are certainly dated to the mid-fourth century.13 Dušek, however, points out that WD 22 is actually not Sanballat’s at all, but his son. The identity of the son has been the subject of many conjectures. Dušek restores the name [דליהו, the known son of Sanballat [I]. Since this Sanballat governed until the late fifth century, his son could well have governed into the mid-fourth century.

Dušek questions the views of Cross and others, noting that their reliance on papponymy to reconstruct missing generations of high priests “runs up against the fact that papponymy is not attested in any of the historical sources concerning the genealogy of High Priests of the Jerusalem Temple in the Persian period” (585).14 Dušek essentially agrees with the reconstruction argued for by VanderKam, most fully and recently in his From Joshua to Caiaphas (2004), according to which there were only six high priests during the two centuries from the time of Darius until that of Alexander (Joshua, Yoyaqim, Elyashib, Yoyada, Yohanan, Yaddua).15

Regarding the date of Ezra’s mission (discussed on 591–93), Dušek concludes that the most likely date was 398, rather than 458. In this case, no new arguments are adduced; instead, Dušek provides an overview of the issues involved and the pros and cons of the various possibilities, along with an abbreviated history of scholarship. Surprisingly,

13 Yaakov Meshorer and Shraga Qedar, Samarian Coinage (Jerusalem: Israel Numismatic Society, 1999), 26–27.

14 In rereading Cross’s arguments, it is striking that the evidence for the practice of papponymy is given as: (a) “The practice in the ruling houses of the Persian period has long been recognized” (with no references); (b) Mazar’s reconstruction of the Tobiad dynasty depends on papponymy; (c) the Ammonite dynasty in the seventh century B.C.E. (as offered in F. M. Cross, “Notes on the Ammonite Inscription from Tell Sirān,” BASOR 212 [1973]: 12–15). The last is explicit over three generations (since the text begins, ממלכ תמנדר, but the third is not and could be easily disposed of. More importantly, even if Cross’s restoration of three Amminadabs is correct, there would be at most one name repeated thrice in the history of the Ammonite monarchy—certainly not enough to justify the presumption of papponymy required for Cross’s argument regarding the Persian period governors and high priests.

15 Contrast Maria Brutti, The Development of the High Priesthood during the Pre-Hasmonean Period: History, Ideology, Theology (JSI Sup 108; Leiden: Brill, 2006), 115–16, who accepts Cross’s view of pervasive papponymy with no discussion. It should be emphasized that at issue is the practice of papponymy in the Persian period; in the Hellenistic period (which is Brutti’s primary focus) the issue is separate, and the case for papponymy is much stronger.
however, he does not interact with the biblical evidence at all, not even mentioning the fact that placing Ezra in 398 runs contra to the biblical text. This is not to say that the biblical evidence must be taken as conclusive,\textsuperscript{16} but it is to say that it has to be reckoned with. He does a fine job of stepping through the options and demonstrating how they are intertwined with the other historical issues of the period.

The strengths of this book, to reiterate, are its careful discussions of manifold issues through analysis of earlier views, and a deep and sustained study of the texts themselves. From the discussions that result, the reader repeatedly emerges with a clear idea of the primary issues involved, the data available to solve the problems, and the major views regarding the optimal solutions. In many cases, Dušek’s own views are compelling; even when not, he has provided the reader with the information necessary to make an informed judgment to that effect.

The only subfield to which Dušek does not make an explicit contribution is the study of the personal names in the papyri; his comments on even the most tantalizing PNs (such as שֶׁכַּי) are brief and limited to the question of the bearers’ identity within the world reflected by the papyri. I found virtually no typographical errors of any consequence (although כֶּסֶף appears for כסף in the transcription of WDSP 28.6r on p. 387), and the book overall was really a pleasure to read. Dušek’s thorough analysis of many of the philological and historical issues raised by these very important—and very fortunate—texts will provide a fundamental basis for much future work.*

\textsuperscript{16} The actual meagerness of the biblical evidence was been remarked by many; see, e.g., Frank Moore Cross, “A Reconstruction of the Judean Restoration,” \textit{JBL} 94 (1975): 4–18; see also idem, \textit{From Epic to Canon: History and Literature in Ancient Israel} (Baltimore: Johns Hopkins University Press, 1998), 160.

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