

the word appears in the two languages in two different forms, /baddāl-/ at Ebla, /bidāl-/ at Ugarit.

— pp. 182-85 (§§33.242-243), pp. 278-80 (§52.2). T. presents the nominal feminine morphemes /-at/ and /-t/ as variants of the former, with the latter arising by vowel syncope; on p. 278, he says that the appearance of one or the other of the two forms is conditioned by the syllabic structure of a given word (“Die Wahl der Varianten ist silbenstrukturell bedingt.”). Whether or not the only proto-Semitic form was /-at/, the occurrence of the /-t/ form is not fixed by rule any more precisely in Ugaritic than in Hebrew, as T. recognizes implicitly by his inconsistent reconstruction of certain feminine noun types (see below, in particular remarks to pp. 183-84 [§33.243.12-15] and to p. 258 [§51.42a]) and as he recognizes explicitly on p. 280, in comparing Ugaritic, Phoenician, and Hebrew. That Ugaritic does not tend to follow the model known from Arabic, where /-at/ predominates, but the Hebrew mixed model is clear from the brief list of forms in §52.213 (p. 279)—though there are many individual divergences between the two languages. Certain nominal base types could only take /-at/ because of constraints on syllable-types (e.g., /qatl/qitl/qutl/qall/qill/qull), but this cannot be said of other base types: as proto-Hebrew had /qātilat/ and /qātilt/ side-by-side so Ugaritic may have done. It is possible, therefore, to say that {ylt}, for example, must have been /*yālidt/ because the absence of {d} in the orthography shows that this consonant has assimilated to the /t/, but it is not possible to determine whether a form like {yldt} is a participle (/yālidat-/ or a /QTLa/ form (/yaladat/). That being the case, there is no probative value in remarks to the effect that the absence of assimilation of the third radical to the feminine morpheme proves a particular nominal stem (e.g., p. 264 [§51.44e], *šwdt*, ‘huntress’, must be /qattāl-/ else /-at/ would have become /-t/ and the /t/ would have assimilated to the /d/; p. 272 [§51.46b], *ālmnt*, ‘widow’, must be /ʾalmānat-/ because if the second syllable had a short vowel the form would have become /ʾalmatt-/).

— p. 182 (§33.242a), p. 183 (§33.243.11a), p. 185 (§33.243.2), p. 253 (§51.41a), p. 294 (§53.312c), p. 295 (§53.322.1a,b). When the same scribe in the same text, at two lines distance (RS 19.042:11, 13 [PRU VI 79]), writes the very same form in conflicting ways, viz., {ḥ[a-a]m-ru-ú} and {ḥa-ma-ru-ú}, while other scribes write plural forms of the same word with {AM} and {MA} (p. 182), one begins wondering about the validity of drawing conclusions about vowel syncope on the basis of such writings (the first form is cited as evidence for “Posttonische Vokalsynkope in viersilbigen Formen” [§33.243.1]). {ma-ās-wa-tu} in RS 19.026:2 (PRU VI 113) and {ma-sa-wa-tu} in line 5 of the same text, parsed as identical forms (p. 253, 294-95), provide no relief. Then one encounters {ša-lan^l-tu₄}, ‘year’, where the proto-Ugaritic form cannot have been /šantu/ (see above, note to p. 146 [§33.115.44.5], etc.), but the writing is cited as evidence for “Posttonische Vokalsynkope in dreisilbigen Formen” (p. 185), followed by another case of different writings of a selfsame word {iš-tu₄} and {i-ši-t[u₄]}, ‘fire’ (in RS 20.201G+:8' and RS 20.189:37', respectively).³⁷⁴

³⁷⁴Texts published by Nougayrol, *Ugaritica* V, texts 131 and 132 (the second republished with an additional fragment by Laroche, *UF* 11 [1979] 477-80; the reading of {iš-tu₄} is credited to van Soldt, *BiOr* 37 [1990] 732, 733). T. obviously does not know what to do with {i-ši-t[u₄]}, since he notes the proto-Ugaritic form as “*iši/atu” and proposes with a question mark that /ʾišitu/ may have developed from /ʾišatu/ through vowel harmony (cf. p. 249 [§51.3b]); but since the vowel of the feminine morpheme usually does not

The case of {ša-ʿanl-tu₄} shows that the result of the “syncope” in question cannot have been /Ø/, that it must not have gone beyond vowel reduction of the type that produced “vocal shewa” in Hebrew (i.e., one that was still vocalic enough in pre-Massoretic-Hebrew to have maintained the aspirated quality of a following *b^gadk^{phat}* letter, e.g., /*malakay/ → /*mal^əkê/ → /malkê/ in the Massoretic tradition). Another example is provided by the plural of /qatl/qitl/qutl/ nouns (see below, remark to p. 251 [§51.41], pp. 295-96 [§53.322.1]). Conflicting writings by the same scribe make one wonder, however, if, when writing Ugaritic words, they were not capable of using {VC} and {CV} signs interchangeably, e.g., {MA} and {AM}, {AN} and {NA}, {IŠ} and {ŠI},³⁷⁵ and CVC signs for CV + CV. If so, ‘apprentices’, was pronounced /ǵamarūma/ in Ugaritic, ‘fire’ was /ʾištu/ (or even /ʾišatu/), and ‘year’ was /šanatu/. A study needs to be undertaken of proper names in syllabic script and conclusions drawn from orthographic variation apropos the validity of drawing narrow conclusions regarding Ugaritic phonology from such variant forms.³⁷⁶

— p. 182 (§33.242a), p. 273 (§51.46h-k). T. twice mentions the syllabic attestations of the gentilic ‘Ugaritian’, which is {u-ga-ar-ti-PI} (RS 19.042:15 [PRU VI 79]; the same writing but without the final {PI} sign for /ya/yi/ya/wa/wi/wu/ follows in line 16, apparently a simple scribal error), and in the first paragraph he vocalizes it as “*ʾugartīyu*.” Neither here nor in his previous brief study of this word³⁷⁷ did he broach the topic of *why* the name of the city of Ugarit would end in /-it/, a morpheme unknown in Northwest Semitic. Moreover, neither he nor van Soldt, who first observed the implications of the syllabic writing for the pronunciation of the gentilic and of the toponym,³⁷⁸ go seriously into the question of etymology. Van Soldt does, however, qualify the writing {A.GÀR-ít} in RS 16.162:23³⁷⁹ as “popular etymology.”³⁸⁰ Popular etymology it may well be, for it is well known that a place name is often more archaic than a given ethnic/linguistic group that inhabits it. But whatever the origin of the name may have been, it is not implausible to believe that the etymology underlying the writing in RS 16.162 reflects the truly *popular* etymology, i.e., that of the people of Ugarit, and hence that the pronunciation was derived from *ʾugāru*, ‘field’. The existence of the noun in Ugaritic is attested by the double divine name *Gapnu-wa-ʾUgāru*, ‘Vine-and-Field’; the association of two agricultural entities in this name shows also that the

harmonize with a preceding vowel, this word is not included above in the sections devoted to vowel harmony.

³⁷⁵Is {ri-gi-mu}, ‘word’, in RS 20.189:8’ (Laroche, *UF* 11 [1979] 479) another example of a CV sign used for VC? On p. 169 (§33.183), T. proposes this explanation of the word: /rigmu/ → /rigmu/ = [rigⁱmu], whereas on p. 260 (§51.42h) he considers the possibility that it is /qittīl/ ← /qattīl/.

³⁷⁶The data base of personal names in all Ugaritic sources, including the most recent Ugaritic and Akkadian texts, put together by C. Roche in Paris for her dissertation on Ugaritic prosopography (2001), will when published make such a study feasible.

³⁷⁷*UF* 29 (1997) 670.

³⁷⁸*UF* 28 (1996) 657, n. 21; cf. idem, *UF* 31 (1999) 775.

³⁷⁹Nougayrol, *PRU* III (1955) 126; cf. S. Lackenbacher, *Textes akkadiens d’Ugarit. Textes provenant des vingt-cinq premières campagnes* (Littératures Anciennes du Proche-Orient 20; Paris: Cerf, 2002) 228.

³⁸⁰*UF* 28 (1996) 657, n. 21.

‘meaning’ of the second was known despite the fact that the standard word for ‘field’ in Ugaritic was *šadû*. The form of the gentilic leads to the conclusion that the ending of the name was /-it-/ while the etymological consideration leads to the conclusion that it should have been /-īt-/. Is there a way to harmonize the two conclusions? Yes. The data gathered by van Soldt for the syllabic orthography of the name indicate that in the vast majority of occurrences the name is registered without a case vowel; van Soldt lists only five exceptions.³⁸¹ Working from the etymology and these writings with a case vowel, I have in the past assumed that the basic form was /ʔugārītu/ and that the noun was, because of this long vowel in the penultimate syllable, diptotic,³⁸² hence, for example, /baʕlu ʔugārīta/, ‘Baal of Ugarit’.³⁸³ The gentilic may be taken to demonstrate, however, that it was the ‘absolute’ case that was standard and that the syllabic writings represent the standard pronunciation at Ugarit, viz., the last syllable was closed and the vowel of the historic morpheme /-īt-/ was hence secondarily shortened. The absolute form would, therefore, have been pronounced /ʔugārit/, as van Soldt proposed in footnote 21 to his list of attestations, though his main entry is “ʔUgarit.”³⁸⁴ And, as T. proposed and van Soldt accepted in principle, the /ā/ would have shortened in the gentilic form because its syllable became closed by syncope of the /i/.³⁸⁵ The historic form of the name would, therefore, have been /ʔugārītu/, the form actually used in the late 14th and into the early 12th c. B.C. would have been /ʔugārit/, whereas the gentilic would have been /ʔugartiyyu/. What I add here to the proposals of van Soldt and T. is an explanation for the origin of the uncharacteristic /-it/ ending.³⁸⁶ As we have seen and will often see below, T. tends to overlook the principle of vowel shortening in closed syllables; this appears to be another instance thereof.

— p. 182 (§32.242b), p. 183 (§33.243.11b), p. 258 (§51.42a). One must query whether *rbt* is simply a secondary form of *rbbt*; since Hebrew has the same two forms, they would appear at least to go back to proto-West Semitic.

— p. 183 (§33.242b), p. 184 (§33.243.15), p. 189 (§33.311.3a, b), p. 204 (§33.444), p. 259 (§51.42c). Given that Hebrew has two basic forms, *qéret* (← /qart/) and *qiryā^h*, meaning ‘city’, and that {qrt} /qart/ is well attested in Phoenician, how likely is it that Ugaritic {qrt}

³⁸¹Ibid., p. 657.

³⁸²For this rule in Ugaritic, see M. Liverani, “Antecedenti del diptotismo arabo nei testi accadici di Ugarit,” *RSO* 38 (1963) 131-60.

³⁸³E.g. *Les textes rituels* (2000) 150 *et passim*.

³⁸⁴*UF* 28 (1996) 657.

³⁸⁵More recently, T. has objected to the view that a given word could have a long vowel in one form and a short vowel in another and has cut the Gordian knot by proposing that the common noun on which the place name was based was /ʔugaru/ (*UF* 33 [2001] 624-26). Whether he is correct or not about the reconstruction of the common noun is a question for Assyriologists to decide, but there is certainly no problem with forms of a given word varying as to the length of one of the vowels, for it happens all the time in verbs, viz., /yaqūmu/ ‘he will arise’ vs. /yaqum/ ‘may he arise’; /qum/ ‘arise!’, /qūma/ ‘arise!’ (both m.s. impv.—for the similar case of the proto-Hebrew Hiphil, see above, note 18). On the case of *epattu* and *epādātu* in Akkadian (singular and plural forms of a single noun), see below, remark to p. 183 (§33.242b).

³⁸⁶This explanation is now included in Bordreuil and Pardee, *Manuel* (2004) I 94-95.

and {qryt} reflect an identical base form? T. hypothesizes the base form to be /qariyat-/, i.e., {qryt} would represent a form in which the original triphthong /qariyat-/ has not undergone syncope and monophthongization because it is in “Pausalstellung” (p. 184), is a “Pausal-Var.” (p. 189) or is “in der Pausa” (p. 204). Because there is virtually no hard evidence for the existence of ‘Pausalformen,’ it appears more plausible to posit that {qryt} would represent the base form /qaryat-/, while {qrt}, /qarītu/, would come from /qariyt-/.³⁸⁷ The writing {qrt} could, of course, also represent a separate form /qartu/ equivalent to the Hebrew and Phoenician forms, a direct descendant of the biconsonantal root QR.

— p. 183 (§33.242b). Comparison of the Ugaritic, Hebrew, and Akkadian forms permits a reconstruction of the base form of {īpd}, ‘(a type of garment)’, as /’ipād-/ rather than “{qital} oder {qitāl}” (with reference to Akkadian). Hebrew /’ēpōd/ can only have come from /qitul/ or /qitāl/, while Akkadian *epattu* and *epādātu* can only have come from /qitāl/ (the singular form shows proto-Akkadian shortening of a long vowel in a closed syllable, i.e., /’ipādātu/ > /’ipādu/ > /’ipattu/). The base form may, therefore, be reconstructed as /qitāl/, and the Ugaritic plural {iptt} may be reconstructed as representing a secondary plural built off of a feminine singular form like the Akkadian singular: /’ipattu/ → /’ipattātu/ (unless, as the plural form is attested only once, the second {t} be simply dittographic in origin). If such be the case, then {iptt} does not represent devoicing of the /d/ after syncope (T. posits /’ipadāt-/ → /’ipdāt-/ → /’iptāt-/) and this example could have been included below in the section where possible feminine plurals with double feminine morpheme are listed.

— p. 183 (§33.243.11c), p. 519 (§74.232.1), pp. 524-25 (§74.232.21), p. 619 (§75.228b). Huehnergard’s proposal to see in {yštāl} the tD-stem of √Š’L, with metathesis because of the first radical being a sibilant (/yišta’al-/ ← /yitša’al-/), and in {yštīl} the Gt-stem of the same root (yišta’il-),³⁸⁸ makes more sense to me than T.’s idea that {yštāl} would show the Gt-stem without syncope (/yišta’al-/), {yštīl} the same form with syncope (/yišta’il-/). The existence side by side of the Gt and tD forms in Arabic (and in Aramaic, with Gt having become tG), each, originally at least, with its own meaning derived from the principal stem, G or D,³⁸⁹ leads me to believe that the Ugaritic situation was similar. On the other hand, it must be observed that the interpretation of {tštīl} in RS 15.098:6 (*KTU* 2.17:15), the only occurrence of the putative form with {i}, is uncertain, for the tablet breaks off immediately after the {l}; this, linked with the fact that the scribe of this tablet made a very parsimonious use of the word-divider (a total of seven in sixteen lines), renders the status of the five signs as a single word uncertain.³⁹⁰

³⁸⁷The reconstruction is based on the syllabic spelling {qa-ri-t[u₄]} in the polyglot vocabulary RS 20.149 iii 18’ (Nougayrol, *Ugaritica* V text 130).

³⁸⁸*UF* 17 (1986) 402.

³⁸⁹T.’s claim, p. 525, that the tD forms in the later languages are often secondary, derived from the Gt, stands in need of proving and, in any case, may not be used to deny the existence of any given tD form in Ugaritic.

³⁹⁰Cf. Pardee, *UF* 16 (1984) 252, n. 7, pointing out the division of the signs into two words in *KTU* ({tštīl}), a division abandoned in *CAT*.

— p. 183 (§33.243.11c), p. 528-29 (§74.234.2), p. 589 (§74.622.3), p. 619 (§75.228b). The restoration of the verb Š'R in RS 16.401:10' (*KTU* 2.32) as {[iš]tīr}³⁹¹ is not possible, for the right point of the sign preceding the {t} is preserved and is part of a horizontal wedge, not of the right wedge of {š} which in this text has a pronounced upwards slant (i.e., is inscribed as an oblique wedge rather than as a horizontal wedge). Though the restoration of the same verb in RS 34.124:42' is certainly plausible,³⁹² the absence of the preformative consonant is crucial, making it impossible to know whether the word was here {ištīr}, as in all other attestations.

— pp. 183-84 (§33.243.12-15). Instead of lumping all substantives ending with /-iy/ and followed by the feminine morpheme {-t} into a single group wherein /-iyat-/ has gone to /-iyt-/ then to /-î/-, it might be more prudent to observe the Hebrew example, where proto-Hebrew /-at-/ forms often exist side by side with proto-Hebrew /-t-/ forms. Such a situation leads to the conclusion that, for proto-Hebrew, there was not automatic syncope of the form /-at-/ but that the feminine morpheme could be either /-at-/ or /-t-/ (which does not mean, of course, that there is not a tendency for the /-t-/ form to have been generated from the longer form in the genitive construction, i.e., in construct). With few exceptions, the III-y forms have developed on the /-at-/ pattern in Hebrew (i.e., both /bōkā/ and /bōkiyyā/ are from /bākiyat/, not from /bākiyt/), while the opposite appears to be the case in Ugaritic (this is indicated by forms from roots both III-y, e.g., {ydt} = /yādîtu/ ← /yādiytu/, and III-ʾ, e.g., {qrit} = /qāri'tu/). The questions regarding Ugaritic are: (1) whether /qāri'tu/ was, in Ugaritic (as opposed to proto-West Semitic), a secondary form of /qāri'atu/, (2) whether a form like {qnyt} represents /qāniyatu/ or is from an entirely different base (see below, remark to p. 184 [§33.243.13]), and (3) whether a noun like {tmnt}, 'eight', which consists of a base form /tamāniy/ + feminine morpheme, developed as did (most of) the participial forms from the form /-t-/ (/tamāniyt-/ → /tamānî/-) or along the same lines as Hebrew (where the corresponding form fits the pattern of most III-y feminine nouns and ends with /-ā/ — see further below, second following remark and remark to pp. 247-77 [§51], etc.).

— p. 184 (§33.243.12), p. 551 (§74.412.26), p. 618 (§75.226), p. 665 (§75.535). Ugaritic and comparative data indicate that {rḥpt} in RS 24.252:8 should not be a participle, for that evidence indicates the D-stem for RHP, 'to soar', of which the participle would be {mrḥpt}. On p. 551, T. suggests that {rḥpt} is either a G-stem participle or to be emended to {mrḥpt}, neither a particularly appealing solution. If {rḥpt} is not a participle, {dit} earlier in the same line should not be either (in the other paragraphs cited, T. analyzes {dit} as a G-stem participle). {dit} is easily analyzable as a noun, perhaps /da'tu/, 'the bird of prey', while {rḥpt} may also be nominal or adjectival, perhaps /raḥḥāpatu/, 'the (feminine) soarer'.³⁹³

— p. 184 (§33.243.13-14), pp. 195-96 (§33.322.2). T. reconstructs III-y/w feminine substantives with /i/ in the penultimate syllable, e.g., /qatil/ or /qātil/ verbal adjectives or the number noun 'eight', in terms of vowel syncope, viz., the proto-Ugaritic /-at-/ morpheme

³⁹¹ Which was adopted from Dijkstra, *UF* 19 (1987) 40.

³⁹² Bordreuil and Pardee, in *Une bibliothèque* (1991) 144, 145, 149.

³⁹³ Pardee, *Les textes para-mythologiques* (1988) 81, 104-5.

would have lost the /a/ depending on the structure of the stem preceding this morpheme. He goes so far as to write a special rule for the feminine form of the number noun ‘eight’ (p. 184: in five-syllable words with a long vowel in the second syllable, the /a/ of /-at-/ drops). This obviously was not the case, however, in proto-Hebrew, for the Hebrew form is *šʾmōnāh*, and one wonders how T. knows, in the absence of any explicit data, exactly what the Ugaritic form was. Indeed, Hebrew shows three clear patterns, one with /īt/, apparently derived from proto-forms with the /-t/ form of the feminine morpheme, e.g., *rē(ʾ)šīyt* ← /riʾšīyt-/, ‘beginning’, and two others derived from proto-forms with the /-at/ form of the feminine morpheme: (a) *bōniyyāh*, ‘she who builds’, with secondary lengthening of the /y/ in the Massoretic tradition, and (b) *yāpāh* ← /yapiyat-/, ‘beautiful’, or *šʾmōnāh* ← /tamāniyat-/, ‘eight’. What attracts attention in the last type is that these forms show no trace of contraction, viz., /-iyat-/ has not gone to /-ât-/ in proto-Hebrew, for the result would have been /-ōt/ in Hebrew, but to /-at-/. T. indicates both /-iyt-/ and /-iyat-/ as becoming /-ît-/ in Ugaritic,³⁹⁴ and indicates others as showing no contraction (e.g. {qryt} = /qariyat-/) but his criteria for syncope of the /a/ of the feminine morpheme are inconsistent, as we have seen (remark to pp. 182-85 [§§33.242-243], etc.). Such reconstructions must be judged unlikely, both from the theoretical perspective and from the comparative one.

— p. 184 (§33.243.13), p. 264 (§51.44i), p. 665 (§75.535). I fail to see why *bkyt*, ‘mourning women’, should be vocalized with Akkadian *bakkītu* (the suggested base is either /qattīl/ or /qittīl/). Either the G-participle or the *nomen professionalis* form /qattāl/ appears more likely for West Semitic. Unfortunately, the three paragraphs cited reveal the author’s indecision which has been described above: the analysis as a participle is rejected on p. 184, mentioned as an alternative on p. 264, indicated as the only analysis, with no cross-reference to the preceding analyses, on p. 665. Just below the entry for *bkyt*, the suggestion is offered that the element *qnyt* of *ʾAtīratu*’s title *qnyt ilm*, ‘progenitress of the gods’, may also be a /qattīl/ or /qittīl/ noun, though the example is not cited in §51.44i alongside *bkyt*. The problem here is the retention of the /y/, normally not the case of the singular feminine active participle of III-y roots (*bkyt* did not present this problem because plural). Perhaps *qnyt* is also a /qattāl/ form. One feminine /qattāl/ is listed by T. (p. 264 [§51.44e]) and several are known in Hebrew (e.g., /ṭabbāḥot/, ‘cooks’, /raqqāḥōt/, ‘perfume-makers’). There is no way, however, of proving that *qnyt* is not simply an archaic /qātiyat/ form, perhaps with secondary lengthening of the /y/ as in the Massoretic tradition of Biblical Hebrew.

— p. 185 (§33.243.2). If {dl} /dallu/, ‘poor’, ever existed in trisyllabic form, it was in some previous stage of the language. Citing this and similar /qall/ forms of “Posttonische Vokalsynkope in dresilbigen Formen” does not, therefore, fall within the purview of a grammar of Ugaritic but belongs to a study of proto-Ugaritic or, more plausibly, of proto-West Semitic, since these forms show up as derivations from /qall/ bases in the other West-Semitic languages (cf. Hebrew *raḥ*, ‘numerous’, plural *rabbīm*); that monosyllabic and dissyllabic stems could exist side by side for millennia is proven by forms such as Hebrew *lēḥāḇ*, ‘heart’, (alongside *lēḇ*, pl. *libbōt*) or Aramaic *ʾamʾmayyā*, ‘peoples’ (alongside

³⁹⁴ Nowhere in the section on triphthongs, but at various points further on in the grammar: e.g., p. 259, §51.42f “*hrt* /*hirīt* ← **hiriyt* ‘Schwangerschaft’, p. 348, §62.181 “*tmnt* /*tamānīt*-, **tamāniyat*-. ”

Hebrew *‘ammīṽm*). T. recognizes that this phonetic change in geminate roots is not an inner-Ugaritic phenomenon, for he describes it as “schon vor-ug.”; that being the case, the proto-Ugaritic development should have been noted, if at all, under morphology (i.e., as /qall/ ← /qalVl/?), rather than under phonology. As for the rest of this paragraph, the ambiguous and conflicting data from syllabic writings (see above, remark to p. 182 [§33.242a], etc.) and the dubious nature of the two final entries ({ib} /êbu/, ‘enemy’, would be from /ʾayibu/ or even /ʾāyibu/ [cf. p. 189 (§33.311.2b), p. 252 (§51.41a)]); {‘šr}, ‘ten’, is vocalized /ʿašr-/ on the basis of comparative data alone, with that form being considered secondary to /ʿašar-/ mean that there is not a single certain case of post-tonic vowel syncope in three-syllable words in Ugaritic. If the last two cases be granted (and the existence of /qatl/ forms in Hebrew and Arabic certainly makes the last one plausible), they probably represent proto-Ugaritic, not inner-Ugaritic phenomena. The only form left that would represent an inner-Ugaritic development is {ša-ʿanl-tu₄}, but I remain dubious about how much can be deduced from that writing (see remarks above to p. 146 [§33.115.44.5], etc., and to p. 182 [§33.242a], etc.).

— pp. 187-200 (§33.3). It is worth remarking that all diphthongs of the types /ay/, /aw/ had monophthongized in proto-Ugaritic and in proto-Phoenician for there are no clear cases of their preservation (this is not a proto-West Semitic development, as is shown by their partial preservation in Hebrew, Arabic, and Aramaic); and that those of the types /iy/ and /uw/ had monophthongized in proto-West Semitic, as they are rarely preserved as such anywhere in any of the West-Semitic languages. On the other hand, most of T.’s cases of monophthongization of /yV/ and /wV/ in Ugaritic are very hypothetical, while there are good cases of their retention, and the behavior of triphthongs may not be described in simple terms on the basis of alphabetic writings. The evolution of triphthongs had not, therefore, reached a stable state at the time when Ugaritic was frozen in time, first by being written down on tablets, then by the disappearance of its civilization. As regards the diphthongs of the types /iy/ and /uw/, T. assumes that all /qūl/ and /qīl/ nouns which are semantically cognate to a verbal root of the hollow type result from monophthongization, viz., /qūl/ ← /quwl/ and /qīl/ ← /qiyl/. This approach overlooks the possibility of derivations from what appear to be biconsonantal roots with a pure long vowel between the two radicals, such as is clearly the case of /qāl-/ , ‘voice’ (see following remark), and /tāb-/ , ‘good’ (absent from the index of Ugaritic words—see remark below to p. 200 [§33.323.4b], etc.), in spite of the fact that both are associated with hollow verbal roots in later West-Semitic languages. It appears necessary to keep an open mind on the precise derivation of at least some such forms in Ugaritic.

— p. 188 (§33.311.1b), p. 252 (§51.41a). It is unclear what T. means by “Alph. Befund” for the contraction of /aw/ to /ô/ as compared with the “Syll. Befund,” where the phonology is indicated explicitly by writings with {u}-vowel signs combined with meaning/etymology, as well as by the meaning of the entries for other languages if the cited form is from a polyglot vocabulary. One of the syllabic example cited is, moreover, indecisive: T. transcribes {PI-

mu}³⁹⁵ as {yu-mu} in proof of a vocalization /yômu/, though it could be normalized as {ya-mu} and match the base of the Hebrew plural /yāmīm/.³⁹⁶ Moreover, of the five Ugaritic examples cited,³⁹⁷ only two contain {û}, and one of these is uncertain: {ûšn}, ‘gift’, could be /’ûšan-/ or /’ûšân-/ rather than /’ôšân-/. Of the three others, one must be judged uncertain: {ql}, ‘voice’, may be /qāl-/ , rather than /qôl-/. On p. 252, T. cites only Hebrew /qôl/ in favor of deriving the word for ‘voice’ from a /qatl/ base, but Aramaic /qāl/ shows that a base form with /ā/ is just as plausible for both Hebrew and Ugaritic. Indeed, the only comparative datum in favor of T.’s vocalization of which I am aware is Arabic /qawl-/, certainly a strong indicator, but indecisive in the absence of data from Ugarit.

— p. 188 (§33.311.2a), p. 252 (§51.41a). Because of the potential to list exhaustively those data that are reasonably clear in the long section where substantives are organized according to base form (§51 “Nominalbildung”), it is unclear why /ḥêbu/ ← /ḥaybu/, “Schuld, Scham (?),” an example of Ugaritic words attested only in syllabic script, is not listed as a /qatl/ form on p. 252.

— p. 189 (§33.311.2b), p. 252 (§51.41a). Again because of the possibility to make the lists in §51 exhaustive, it is unclear why two of the seven examples of nouns attested alphabetically in which /ay/ has become /ê/ (§33.311.2b) are absent from the list of /qatl/ forms from II-y roots ({îl}, ‘ram’, and {în}, ‘non-existence’). On {îb}, ‘enemy’, present in both sections, see above, remark to p. 185 (§33.243.2).

— p. 189 (§33.311.3b), p. 259 (§51.42f). The word *hrt*, “Schwangerschaft,” is a ghost word. The reading {ḥrt} in RS 2.[004] ii 41' (*KTU* 1.17), the only purported attestation of the word, was first proposed in *KTU* with a question mark after each of the three signs but was then taken over into *CAT* with only the standard indication of uncertain readings, viz., it was printed in Roman script. T. accepts the reading with no indication of its uncertainty in either section. It constitutes in fact one of the more spectacular misreadings in *KTU* because the proper reading is {ḥlḏ}: the {y} resembles a {h} because the surface level of the sign has disappeared leaving only the tops of the six wedges, which happen to be aligned nicely on the horizontal plane, giving the illusion of three long horizontal wedges; the {l} looks nothing like a {r} because it has retained its boxy outline in spite of being heavily damaged within the outline; the {d}, however, is certain, because it has retained its upper boxy outline while all three of the lower horizontals are partially visible.

— p. 189 (§33.311.3b), p. 199 (§33.323.3a), p. 258 (§51.42a), p. 259 (§51.42f), p. 667 (§75.536b). On pp. 189 and 259, {zḡt} in RS 2.[003]⁺ iii 18 and v 11 (*KTU* 1.14) is vocalized as a /qitil(a)t/ form, on pp. 199, 258, and 667 it is identified as /qatalat/. There is

³⁹⁵ So written in both attestations (RS 20.123⁺ IVa 15' [Nougayrol, *Ugaritica* V, text 137] and RS 20.426B:2' [ibid., text 138]).

³⁹⁶ He follows Huehnergard (*Ugaritic Vocabulary* [1987] 67, 133) in this respect, though Huehnergard indicated clearly the ambiguity of the syllabic entry.

³⁹⁷ No reason is indicated for limiting the examples to five; in the case of /ay/ → /ê/ (p. 189), eight examples are cited, in that of /iy/ → /î/ (ibid.) nine, including one proper name. Below, §33.312.12b (p. 190), a set of examples is explicitly termed a “Beispielauswahl”—the word could have been used more often.

no cross-reference to the two identifications of the base form. Like {zġt}, {gʿt} in the same passage of the *Kirta* text is classed with /qatalat/ nouns on p. 258 (see below, remark to §51.42a on that page).

— pp. 190-93 (§33.312). In the course of this section on diphthongs of the type /w/y+vowel/, various contractions are assumed on the basis of vocalizations based on comparative considerations alone, i.e., for which there is no evidence from Ugarit. All such vocalizations are, of course, purely hypothetical, for the proto-Ugaritic form need not have been identical to that of one or more of the other Semitic languages. Only a few of these are mentioned in the following remarks.

— p. 190 (§33.311.5), pp. 658-63 (§75.532), pp. 667-71 (§75.537). Without discussing the comparative data,³⁹⁸ T. proposes on p. 190 that the jussive/perfective of III-weak roots of the /yVqatl/ type had a final vowel that was long by contraction: “z.B. *ybn* /*yubnâ*/ ← **yubnaw*/y'es soll gebaut werden' ” (p. 190). In his vocalization of /yaqtiy/ forms on pp. 658-63 he assumes the analogical contraction (/yaqtî/);³⁹⁹ of /yaqtuw/ forms, the contraction (/yaqtû/). When, however, Arabic shows a short vowel in all such cases and proto-Hebrew certainly had a short vowel in the same position (both the jussive /yîben/ and the ‘wāw-consecutive’ /wayyîben/ can only have descended from /yibna/, while /way(y)ʾkal/ can only have descended from /yakalli/ [D-stem]),⁴⁰⁰ I see no reason to posit a different situation in Ugaritic.⁴⁰¹ (see the more detailed remark below to pp. 653-71 [§75.53]).

³⁹⁸ Below, pp. 655-56 (§75.531e), it is admitted that the proto-Hebrew form “**yigl*” (cf. the continuation of this remark) is said to come from “**yigliy*” but, without stating why, T. chooses to ignore this datum and those from “einer Reihe anderer sem. Sprachen” without citing a single Semitic language where the forms behave as he thinks they did in Ugaritic.

³⁹⁹ Cf. p. 193 (§33.312.32): “*yḥ* /*yaḥî*/ ← *yaḥyi* (Kürzung des Auslautvokals) ← **yaḥyî*.”

⁴⁰⁰ Because the imperfective forms of III-y roots have simplified along the lines of either /yiqtay/ (the majority) or of (yaqtiy), evidence for proto-Hebrew /yaqtuy/ and /yaqtuw/ have disappeared from Hebrew as we know it. T. assumes that all /yaqtul/ imperfectives of the III-weak types in Ugaritic followed the III-w pattern (cf., e.g., his vocalizations of the imperfectives of ‘LY, ‘to ascend’ [p. 661], none of which show {y} or {w} in the imperfective, though {y} is attested in other forms).

⁴⁰¹ Also without arguing the case, T. makes the same error in his treatment of the divine name YHWH (“Der Gottesname **Yahwa*,” VT 51 [2001] 81-106, esp. pp. 91-92), claiming that a /yaqtîl/ verbal form of a root HWY should have been /yahwî/. Such is not at all the case in proto-Hebrew: *yʾhîy*, ‘may it be’, and *yʾhîy*, ‘may he live’, can only have come from /yVhyV/ and /yVhyV/, where the final vowel was short (e.g., /yihyay/ → /yihya/ → /yihy/ → /yihî/ → /yʾhî/, exactly like /piryu/ → /piry/ → /pirî/ → /pʾrî/, ‘fruit’—on the formal identity of these types of nominal and verbal developments, see also below, remark to pp. 653-71 [§75.53]). Thus, M. Weippert’s reconstruction of the basic proto-Hebrew form as having a short final vowel (*Reallexikon der Assyriologie* V [1977] 252-53), which T. criticizes on p. 92, must be preferred. Whether the divine name YHWH was originally nominal /qatl/ or verbal /yiqatl/ or /yaqtîl/ cannot, therefore, be decided by the orthography and presumed morphology of the name that is deduced therefrom; this is all the more true since a proper name can be perceived as belonging to the nominal category and be ascribed nominal inflection (e.g., Amorite {ia-wi-um}, cited by T. on p. 91, must be /yaḥwiyum/ or /yahwiyum/ with nominative case vowel and mimation; at Ugarit, West-Semitic names are usually, though not always, correctly inflected for case when written in syllabic cuneiform). Only the plausibility of the respective etymologies enables one to lean in one direction or the other and, as T. recognizes, there is no good etymology from a hypothetical root YHW. It appears necessary, therefore, to retain the explanation of YHWH as based on a verbal form of the root HWY. It is perhaps not totally irrelevant to observe that if

— p. 190 (§33.312.12b). This paragraph is devoted to the retention of /y/ and a following vowel at the beginning of a word irrespective of the quality or the quantity of the vowel (for example, /yôm-/ ‘day’, and /yad-/ ‘hand’). T. refers to no modification of such syllables elsewhere in the West-Semitic languages; since, as far as I can remember, such syllables are normally retained in the West-Semitic languages, one wonders why a paragraph was devoted to the topic.

— p. 190 (§33.312.12b). The word {yšr} in RS 2.[003]⁺ i 13 (*KTU* 1.14) is glossed as meaning “Redlichkeit.” In the passage in question, however, the quality attributed to the wife in question is not ‘honesty’, but ‘(social) propriety’, i.e., her origin was the one best suited to a wife, probably the first cousin on the father’s side.⁴⁰²

— pp. 190-91 (§33.312.21b). This paragraph, on /wV/ within a word, requires three remarks: (1) The word {bnwt} in the title of *ʿIlū* {bny bnwt}, lit., ‘builder of progeny’, is not necessarily /bunwat-/ or /bunwāt-/ , as is shown by Arabic /bunuwwat-/ ‘sonship’;⁴⁰³ (2) two nouns are cited, {ḥẓt}, ‘fate’, and {kšt}, ‘(a type of) garment’, that are supposed to be derived from /qitl/- or /qutl/-base nouns with /w/ as the third root consonant, but the Ugaritic words need not be derived from such a base; (3) the parenthetical reference to /yaqwum-/ becoming “/yaqûm-/” has no place here, for the absence of the hypothetical first form in any of the West- or East-Semitic languages shows that the development would have pre-dated Ugaritic as we know it.

— p. 191 (§33.312.22a). What the editor read as {iš-ia-ti-mi} in RS 20.235:10⁴⁰⁴ is taken here as a dual noun of uncertain meaning. Van Soldt has read {IŠ} as {URU} and interpreted it as a town name of uncertain localization.⁴⁰⁵

— pp. 191-92 (§33.312.22b-c). Similarly to what was said with regard to §33.312.21b, there is no proof that Ugaritic {lḥt}, ‘cheek’, and {ḡlt}, ‘evil’, were from /qatl/ or /qitl/ bases with /y/ as the third root consonant and it is inappropriate to compare the hypothetical development of /yašyit-/ to “/yašît-/” for the same reason as was indicated in that remark.

— p. 191 (§33.312.22b). T. indicates the vocalization “/ku/ilyāt-/,” described as a plural, for the word meaning ‘kidneys’ and opines below (p. 296 [§53.322.1]) that III-y nouns of the /qatl/qitl/qutl/ types had a monosyllabic stem in the plural, rather than the dissyllabic stem shown for most forms from strong roots. Hebrew, however, has a regular ‘segholate’ type plural for this noun (viz., with a dissyllabic stem, /k³lāyōt/). Given the many irregularities in Hebrew of III-y nouns of this type, it would perhaps be best to remain agnostic on any given Ugaritic form for which internal evidence is not extant.

this divine name preceded the Barth-Ginsberg shift from /yaqtal/ to /yiqtal/, or came from a language or dialect where the shift did not take place, there would have been a stage of the development of the verbal form behind the name in which that form was /yahwa/, which is precisely the proto-form posited by T.

⁴⁰²Pardee, *Context I* (1997) 333 n. 7.

⁴⁰³The vocalizations /bunwat-/ or /bunwāt-/ are indicated again below, p. 198 (§33.322.43) and p. 474 (§73.423), against the analysis and vocalization as a G-stem passive participle (/banūwat-/). The Arabic form shows that the possibilities go beyond the /qutl/ and /qatūl/ bases.

⁴⁰⁴Nougayrol, *Ugaritica V* (1968) text 84.

⁴⁰⁵*UF* 28 (1996) 674 with n. 160; *UF* 30 (1998) 741.

— p. 192 (§33.312.31b), p. 265 (§51.45a). In the first section cited, *ʾrw*, ‘lion’, is said to be from a quadrilateral root ʾRWY, in the second, to be an /ʾ/-preformative; in the second, Hebrew /ʾaryē/ is cited as a parallel. The precise relationship between the Ugaritic and Hebrew forms is impossible to determine without more data, for Hebrew maintained certain *qatw* forms (e.g., /ʾāḥū/, ‘swamp’, /tōhū/, ‘wasteland’), and it is not clear why /ʾarwu/ or /ʾarwū/ would in proto-Hebrew have become /ʾary-/ or /ʾaryî-/. The two Hebrew forms /ʾarī/ and /ʾaryē/ could either be by-forms, viz., /qatl/ and /qatlal/, or the first could be a contraction of the second, viz., the proto-Hebrew form would have been /ʾaryî/ (that is, the genitive form retained from the inflection /ʾaryû/ʾaryâ/ʾaryî/); /ʾarī/ would be a secondarily contracted form of /ʾaryî/. Because the contraction of ‘regular’ III-y nouns from proto-Hebrew to Hebrew normally resulted in a final syllable with *seghol*, the peculiarity of the Hebrew long form /ʾaryē/, i.e., the spelling with *šere*, favors the first solution, viz., proto-Hebrew by-forms /ʾary-/ and /ʾaryay-/. I have, however, found no simple set of developments that would allow the derivation of the Hebrew and Ugaritic forms from an identical parent form. That III-y and III-w forms could develop side by side, however, is clear from the coexistence in Hebrew of forms such as /ʿerwā/ and /ʿeryā/, both meaning ‘nakedness’; so the relationship of Ugaritic {ʾrw} and Hebrew /ʾarī/ and /ʾaryē/ may become clearer with time.

— p. 192 (§33.312.31b), p. 486 (§73.523bα), p. 667 (§75.536b). In the first section cited, *ǧz* in RS 3.325+ vi 43 (*KTU* 1.16) is said to be derived from “*ǧazw/yi*,” in the second from “*ǧVzw/yi*,” and in the third from “*ǧVzwi*.”

— p. 192 (§33.312.32b), p. 196 (§33.322.3b), p. 254 (§51.41b). Given the retention of the {y} in *ʾny*, ‘(group of) ships’, and comparing Hebrew /ʾoniyyā/ (a different base but with /-yy-/), it appears likely that the Ugaritic form was either /ʾanayy-/ or /ʾanāy-/, not “/ʾany-/ (← *ʾanay-)” (p. 192). The /qatl/ form is assumed on the basis of a hypothetical proto-Ugaritic stem with two short vowels of which one would have disappeared by syncope; no Ugaritic or comparative evidence of which I am aware supports it.

— p. 192 (§33.312.32b), p. 245 (§45.21d). T. is certainly correct in reading *gdt* for *gdm* in RS 24.255:7 (*KTU* 1.111). I would, however, indicate the reading as {gdʿtʿ} rather than as {gdtʿ}, for what has been taken as a vertical wedge after the horizontal is neither clean, deep, nor clearly vertical. It may well be an erasure mark; if so, perhaps of an extra word-divider.⁴⁰⁶

— p. 193 (§33.312.32b), p. 216 (§41.221.12b), p. 252 (§51.41a), p. 301 (§53.34). On p. 193 “*zby-*” in RS 3.343+ iv 07' (*KTU* 1.15) is analyzed as a singular or plural noun in the pronominal state; on p. 216, the form is given as “*zby-y*” and said to be more probably singular than plural because modified by *tmnym*, ‘80’; on p. 251, “*zby-*” is cited as a plural in the pronominal state; and on p. 301, “*zby[m]*” is cited as a possible plural form.

— p. 193 (§33.312.32b), p. 255 (§51.41c), p. 325 (§54.323a). As T. has already recognized elsewhere,⁴⁰⁷ not only is the context of the reading {šlyh} in RS 1.064+:6 (*KTU* 1.27)

⁴⁰⁶ Pardee, *Les textes rituels* (2000) 620, 626.

⁴⁰⁷ *AfO* 42-43 (1995-96) 270.

unclear, but the reading itself is more than questionable. Indeed, I consider the reading {l'lyh}, 'in the upper room', to be far more plausible.⁴⁰⁸ This is a prime example of T.'s vacillation as regards his data, described above in the introduction: in the first two sections cited, he bases his analysis on the reading {šlyh}; in the third, he works with "(?)lyh," citing my article in *BSOAS* 58 (reference note 408).

— p. 193 (§33.312.32b). If {pgy} is indeed the masculine form of *pgt*, 'girl', then the Hebrew personal name *pūw'āh* should in all probability no longer be cited as a cognate of the Ugaritic word, for the *plene* spelling in Hebrew is normally the indication of a hollow root, not of a root that is III-y (according to normal Hebrew rules, /pu'yatu/ might have given something like /po'oyā/ or /pō'ā/ in Hebrew, but not /pū'ā/). Nevertheless, T. bases his vocalization of the Ugaritic form on that of the Hebrew name: "/pu'gy-/" As there is no particular reason why a word for 'boy/girl' should have a *qutl* base, for this base commonly expresses abstracts, the Ugaritic/Hebrew connection must, until new data appear, be considered questionable.

— p. 193 (§33.312.32b), p. 256 (§51.41e), p. 303 (§54.111). {rī} in RS 2.[014]⁺ i 12 (*KTU* 1.3) is translated "Aussehen" and/or "Erscheinung" in all three paragraphs cited but is identified as a "n[eue] L[esung]" only on p. 303. One must object that the reading is far from new, since it was proposed by de Moor in 1971.⁴⁰⁹

— p. 193 (§33.312.32b). T. cites Hebrew /'anā/, 'furrow', in support of his vocalization /'Vnât-/ for the corresponding Ugaritic word. If, however, proto-Hebrew had had a contracted vowel in the feminine morpheme, i.e., the equivalent of /ā/, the Hebrew form would not have had *qameṣ* but *holem* (cf. /'āḥōt/, 'sister'). In Hebrew, a good many nouns which seem etymologically to have developed from III-weak roots show no sign of proto-Hebrew contraction.

— p. 193 (§33.312.32b), p. 486 (§73.523b). In the first section cited, it is averred that {bd} in RS 3.325⁺ i 5 (*KTU* 1.16) does not derive from the root BDY but from BDD; in the second, just the opposite. However that may be, the signs in question may just as well represent the prepositional phrase *b + yd* meaning 'in the hand(s) of', i.e., 'into the control of'.⁴¹⁰

— pp. 194-200 (§33.32). T. pretty much throws in the towel on writing rules for the behavior of triphthongs in Ugaritic, apparently because "der betreffende Befund ist schwierig zu interpretieren," something with which I would be the last to quarrel. He contents himself for the most part with listing all the possible combinations of vowels and /y/w/; the forms corresponding to the principal rule, viz., with or without contraction, are listed first, the exceptions and/or possible exceptions thereafter. Because the examples for a given rule are always few, usually based on comparative rather than internal data, and

⁴⁰⁸ *BSOAS* 58 (1995) 232; *Les textes rituels* (2000) 360, 1270.

⁴⁰⁹ *The Seasonal Pattern in the Ugaritic Myth of Ba'lu according to the Version of Ilmilku* (AOAT 16; Kevelaer: Butzon & Bercker; Neukirchen-Vluyn: Neukirchener Verlag, 1971) 67, 75; cf. Pardee, *Ugaritic and Hebrew Poetic Parallelism: A Trial Cut* (*'nt I and Proverbs 2*) (VTS 39; Leiden: Brill, 1988) 2 *et passim*.

⁴¹⁰ Cf. Pardee, *UF* 5 (1973) 230; idem, *Context I* (1997) 339; Wyatt, *Religious Texts* (1998) 220.

usually accompanied by apparently conflicting examples, one understands T.'s reticence to formulate overarching rules. This being the case, one may question T.'s decision to indicate a given form under a totally hypothetical base (e.g., {yr}, "Frühregen," /yarû/ ← /warayu/, against Hebrew /qātil/ [p. 198]⁴¹¹) on the basis of rules as poorly formulatable as are these (on pp. 195-96, four possible exceptions to the general retention of the /y/ in the sequence /āCiyV/ are indicated; why was this form not added to that list?). On the other hand, I missed even so simple a rule as: III-y/w nouns show a triptotic case system with contracted second vowel (e.g., /šadû/šadâ/šadî/, 'field') whether the historical consonant be /y/ or /w/; the rule appears easily extractable from the various forms listed, whether attested by data from Ugarit, from comparative Semitics, or purely hypothetical. Because the rule is based on monophthongization, there is, of course, never any direct data from Ugarit on whether the proto-Ugaritic third consonant was /y/ or /w/. T. indicates one or the other (e.g., /šadû/, 'field', ← /šaduwu/ but /qanû/, 'reed', /qanayu/) without indicating the comparative basis for his decision. The reader should note that T. for the most part limits his choice of examples in this section to nominal forms, leaving the even more problematic verbal forms for treatment under verbal morphology (pp. 653-71 [§75.53]).

— p. 195 (§33.322.2a). T. vocalizes the month name {h̄yr} as /h̄iyar-/, but, in spite of the phonetic and calendric differences, the month name /ʿiyyār/ used in both Hebrew and Aramaic shows that the Ugaritic form may have had /yy/ and /ā/.

— p. 195 (§33.322.2b), p. 569 (§74.423), p. 669 (§75.537d). Though T. may be correct in identifying 'ryt in RS 18.031:25 (*KTU* 2.38) as a form of the D-stem, it neither means "entleert" nor is the interpretation as a D-active, that T. presents as an alternative (on p. 669, it is the preferred interpretation), plausible. The full sentence is {w . ānyk . tt (25) by . 'ky . 'ryt}, 'your ships have found haven in Akko, (they are) stripped (of their sails)' (on {tt}, see above, remark to p. 110 [§32.144.12b], etc.). That the use of 'RY reflects a nuance of 'nakedness', rather than one of 'emptiness', appears clearly indicated by RS 18.025:16 (*KTU* 4.338), where LBŠ, 'to clothe', is used for outfitting a ship—the content of that text makes it clear that the reference is not to filling the holds of the ships, but of equipping them. Because the ships to which reference is made in RS 18.031 have gone through a storm, it appears very likely that 'RY expressed their loss of 'clothing', i.e., their sails (and associated rigging). It remains uncertain, however, whether { 'ryt} is a simple adjective, a G-stem stative or passive participle (cf. the G-passive /'uriya/ in Arabic), i.e., 'have become naked', or a D-passive finite form in an asyndetic construction, i.e., 'have been made to be naked'.

— p. 195 (§33.322.2c), p. 202 (§33.423b), p. 315 (§54.214b), p. 317 (§54.221a), p. 426 (§73.122), p. 540 (§74.342), p. 657 (§75.531e), p. 663 (§75.533), p. 668 (§75.537c). How can T. seriously propose as his preferred analysis of {ibky} (RS 34.126:13 [*CAT* 1.161]⁴¹²)

⁴¹¹Note that another explanation of the Hebrew form may be in order, as /qātil/ is usually used only for animate entities (*fā'il qahwe* in modern Arabic is not a 'coffeemaker', but 'someone who makes coffee').

⁴¹²*KTU* here had an entirely different reading (see Bordreuil and Pardee, *Syria* 59 [1982] 125).

a G-stem imperative when not a single other G-imperative listed on pp. 663-64 shows prothetic {ʾ}?⁴¹³

— p. 195 (§33.322.2d), p. 655 (§75.531d), p. 664 (§75.534). Surely the historical reconstruction in the first section cited of *dw* in RS 3.325⁺ ii 20, 23 (*KTU* 1.16) as */*dawiy-/* is to be preferred over that on p. 655, where */*daway-/* is presented as an option before that of */*dawiy-/*, and both presentations are preferable to that on p. 664, where only */*daway-/* is mentioned, albeit with a question mark. If form corresponded to meaning in Ugaritic in any way corresponding to that of the later Northwest-Semitic patterns, then the meaning ‘to be sick’ dictates that the primary stative base form */qatila/* be preferred to the fientive base */qatala/*. Also involved is the question of precise morphological analysis: the form is simply translated “krank” on p. 195—as though it were a stative adjective—but specifically parsed as */QTLa/* in the other two sections cited. Though a significant number of */qatal-/* stative adjectives are attested in Hebrew,⁴¹⁴ */qatila/* and, to a lesser extent, */qatula/* forms predominate in the verbal paradigms; and this tendency is, of course, standardized in Arabic.

— p. 196 (§33.322.2d). Since {r^h} in the administrative texts is usually taken to mean ‘his companion’, not “sein Hirte,” it is surprising that only this latter interpretation is indicated. Judging from the Hebrew forms */rē^a/* and */rē^e/*, the Ugaritic word corresponding to the first meaning appears to represent a contraction from */riⁱiyuhu/*. The precise socio-economic function of the ‘companion’ remains uncertain.

— p. 196 (§33.322.3b), p. 214 (§41.21), p. 469 (§73.333.4). If the 1 c.pl. pronominal suffix was indeed */-nū/* (see below, remarks to p. 214 [§41.21], etc., and p. 467 [§73.332.5]), then the characteristic vowel of the 1 c.pl. and the 1 c.du. was probably not identical for, as T. points out on p. 469, it is unlikely that the consonantal glide */y/* in the 1 c.du. form {*-ny*} would have occurred after */ū/*. One may surmise that the 1 c.pl. form arose from the nominative pronoun (i.e., the one used on the */QTLa/* verb: *(/nu + ū/ → /nū/)* while the 1 c.du. arose from the oblique form *(/nu + ā + a/ → /nāyā/* by vocalic harmony).⁴¹⁵

— p. 196 (§33.322.3c), p. 272 (§51.46e), p. 835 (§89.35). An old reading of RS 18.147:11 (*KTU* 2.46), known since 1968 but not present in the transliterations which I sent to T. (see above, introduction), gives a totally different understanding of the passage. Instead of {*midy . w [.] ḡbny*}, where *ḡbny* is taken to mean “Dicke, Fülle,” the reading should be {*midy . [r]ḡbny*},⁴¹⁶ and the passage, instead of expressing a claim that the author of the statement is fat and full (lit. ‘with me is muchness and thickness’), is stating the exact opposite. In context, it says: ‘Concerning the fact that my son has sent a tablet regarding

⁴¹³It is unclear why he does not list on pp. 663-64 the other forms with an *ʾalif*-sign in first position that he interprets elsewhere as G-imperatives ({*išhn*}, see remark to p. 170 [§33.211.2], etc., and {*ūqrb*}, see remark to p. 202 [§33.423a], etc.).

⁴¹⁴J. Fox, *Semitic Noun Patterns* (Harvard Semitic Studies 52; Winona Lake: Eisenbrauns, 2003) 162.

⁴¹⁵On */a/* as an oblique case vowel on pronouns, see Gelb, *Sequential Reconstruction* (1969) 69-72.

⁴¹⁶The reading was first reported by Schaeffer in *Ugaritica* V (1968) 725 n. 1 as having been proposed by Herdner. Then, Herdner herself expressed her preference for the reading in *Ugaritica* VII (1978) 59. I did not become convinced of the correctness of Herdner’s proposal until I was preparing my final copy of the tablet in June of 1996.

food (in which he [= you] said): “(Here) with me, plenty (has become) absence of food,” let my son assign this: sea-faring boats. Let him [...] and food [...]. In other words, the author of the present letter cites a previous letter in which his ‘son’, the king of Ugarit, has pleaded that his food supplies have dwindled to the point that famine is threatening; the father-figure (of unknown identification) is apparently suggesting that the ‘son’ send ships to Egypt for food, the standard procedure in the late 13th-early 12th centuries.⁴¹⁷

— p. 196 (§33.322.3c), p. 326 (§54.412), p. 334 (§54.423g), p. 485 (§73.514b), p. 493 (§73.533), p. 622 (§75.236), p. 649 (§75.526), cf. p. 805 (§84.22). The interpretation of the first {ù} of {ùbù} in RIH 78/20:18 (CAT 1.169) as an “Ausrufpartikel” (p. 805) meaning “Wehe!” (pp. 334, 485—this text is not, however, cited in §84.22 on p. 805 where the putative “Ausrufpartikel” is treated) is in my estimation out of the question, for two reasons, one lexical the other literary. (1) There is no particle in Ugaritic such as the one T. sees here (in all of the examples cited in §84.22, the particle in question is a conjunction, either /’ô/, ‘or’, or else /’û/, ‘and’).⁴¹⁸ (2) Though the beginning of the line in the RIH 78/20 is lost, {ùbù} is more plausibly linked with the two preceding signs and the phrase is to be read {’błt . ùbù . àl . tbi}, ‘the house I enter you (or: she) must not enter’, as is shown by the parallel in Arslan Tash I 5-8 *bt ’b’ bl tb’n w ḥṣr ’drk bl tdrkn*, ‘The house I enter you must not enter, the court I tread you must not tread’.⁴¹⁹ I fail to see why T. rejects this interpretation of RIH 78/20:18, which appears far more plausible than one based on a particle of dubious existence.⁴²⁰ On the basis of the considerations offered above (remark to p. 37 [§21.322.5a], etc.) on {ùbà} in RS 24.244:72 (KTU 1.100) and {tbi} in the phrase under discussion in RIH 78/20, I take this occasion to correct my vocalization of {ùbù} with stem-vowel /ā/:⁴²¹ it is more plausibly /û/, that is /’ubû’u/. The final {ù} reflects, therefore, the indicative mood vowel /-u/, which is expected in a formulation such as this, while the first

⁴¹⁷For a recent discussion, see J. Freu, “La fin d’Ugarit et l’Empire hittite. Données nouvelles et chronologie,” *Semitica* 48 (1998) 17-39.

⁴¹⁸Aside from the fact that no text requires the interpretation of {ù} as an interjection, the etymological side of the question makes the existence of such a particle at least questionable. T. hesitantly suggests that the particle would be vocalized /’ô/ and be derived from an original /’aw/, but that is the exact derivation of the particle meaning ‘or’. Should the existence of such an interjectional particle ever be proven for Ugaritic, it may well have a vocalization different from the disjunctive /’ô/ (cf. Hebrew, where the two are ’ô^w and ’ôy—on Ugaritic *ây* as the particle corresponding to Hebrew ’ôy, see above, remark to p. 172 [§33.213.2]).

⁴¹⁹See my translation of the Ugaritic text in *Verse in Ancient Near Eastern Prose* (1993) 211-13; the new edition of this text in *Les textes rituels* (2000) 875-93, with previous bibliography on the interpretation of line 18 on p. 292 n. 114; and my new edition of the Arslan Tash text in *Syria* 75 (1998) 15-54, esp. pp. 18 (translation), 36 (comment). Though Ford accepts the existence of the particle in question (see note 741), he correctly does not accept that analysis of the first {ù} of *ùbù* (UF 34 [2002] 156, 199-200).

⁴²⁰The editors divided the signs as does T. (Bordreuil and Caquot, *Syria* 57 [1980] 349: the {ù} was identified as a conjunction), but the explanation of the Ugaritic text on the basis of the Phoenician one was proposed soon after (Avishur, UF 13 [1981] 25) and was subsequently accepted by Caquot (*Or* 53 [1984] 176). If one wishes to leave the Arslan Tash parallel aside, one should at the very least follow the editors in taking the particle as a conjunction: ‘...also you/she must not enter ...’.

⁴²¹*Les textes rituels* (2000) 878.

/ū/ reflects vowel harmony with the long stem vowel (so T., p. 175 [§33.215.21b], and p. 186 [§33.26], on {ùbà} in RS 24.244:72 [KTU 1.100]).⁴²²

— p. 196 (§33.322.41b), p. 541 (§74.36), pp. 541-42 (§74.362), p. 843 (§91.313). On p. 196, T. translates *np̄y* in RS 1.002:18' *et passim* (KTU 1.40) by “Befriedigung”; on pp. 541-42 he makes a special point of rejecting my translation “well-being”⁴²³ (which he renders by “Wohlfahrt” in German) in favor of “Befriedigung,” with the added comment that “hier wird dagegen die Nuance ‘(volle) Befriedigung’ (bzw. ‘Entschädigung’) favorisiert”; finally, on p. 843, he favors “Wohlfahrt” by placing it before “Befriedigung” in his listing of the two as optional translations of *np̄y*.

— p. 196 (§33.322.41b), p. 336 (§54.52), pp. 344-45 (§62.112), p. 352 (§62.21). In none of these sections does T. explain why he vocalizes the word for ‘one’ that is written {‘št} and {‘šty} with a long vowel in the second syllable (/‘aštāy-/), in spite of the fact that on p. 344 he cites one author who has proposed /‘aštay-/.⁴²⁴

— p. 197 (§33.322.42a), p. 751 (§81.4e). T.’s glosses of the particle *hl̄ny* as “nun” and “siehe!” cover only one part of its functions. He bases those translations in part on the interchange with {hn̄ny} in the first part of what I have termed the ‘double formula of well-being’ characteristic of Ugaritic letters and of Akkadian letters from Ugarit, Emar, and Hatti (‘here with me, things are fine; there with you, whatever is well, return word [of that] to me’).⁴²⁵ On this equivalence, I have no quarrel, and it constitutes one side of the use of *hl̄ny*—though one could argue that the parallel with *tm̄ny* in that formula indicates a meaning ‘here’ for both *hl̄ny* and *hn̄ny*. I would not go that far, but it is clear from the fact that *hl̄ny* can appear as the first word in the body of a letter (the clearest cases are RS 15.174:7 [KTU 2.21], RS 16.379:12 [KTU 2.30], RS 29.093:11 [KTU 2.70], RS 94.2406:3, RS 94.2580:4), whereas *hn̄ny* never appears in this position, that the former has a nuance of ‘here’ that is absent from the latter (see further below on *hn̄*, etc., remark to pp. 737-38 [§81.11a-d]).

— p. 197 (§33.322.42a). Until one checks the text, T.’s notation “*ḥe-e-ia/ya*” for RS 15.119 verso 7', 9' (PRU III, p. 87) leaves the reader uncertain as to the exact reading of the cuneiform. The text reads {ḥe-e-ia} in line 7', {ḥe-e-PI} in line 9'. Nougayrol read {PI} as {ya}. T. does not indicate why he prefers {yu} over {ya}, but one may assume that it is because the word is syntactically accusative in line 7', nominative in line 9'.

⁴²²The line in question is now vocalized /bêta ʾubūʾu ʾal tubuʾ/tubūʾī/ in Bordreuil and Pardee, *Manuel* (2004) II 68 (the indication of alternatives for the vocalization of the second form are owing to the fact that the gender of the addressee is unknown).

⁴²³He cites my preliminary presentation in *Leslau* (1991) where “well-being” is used on pp. 1183, 1185, 1187, 1192-94 (strangely, he cites only p. 1191, where the literal meaning of the term, viz., “being made beautiful, whole,” is the point of the discussion). For the more detailed study on which this preliminary presentation was based, see now *Les textes rituels* (2000) 92-142, and for a specific remark on the difficulties of translating *np̄y* into English, *Ritual and Cult* (2002) 111 n. 112.

⁴²⁴M. Cecchini, “‘šty 1 in ugaritico,” *Or* 50 (1981) 106-9; cf. Bordreuil and Pardee, *Une bibliothèque* (1991) 154.

⁴²⁵“Une formule épistolaire en ougaritique et accadien,” *Semitic and Assyriological Studies Presented to Pelio Fronzaroli* (Wiesbaden: Harrassowitz, 2003) 446-75. On T.’s interpretation of the formula, see below, remark to p. 244 (§45.122b), etc.

— p. 197 (§33.322.42a). Given that the vocalization “/tōkīyi/” of the second element of the place name *glltky* is glossed as “inner(e[r/s]),” the /ō/ must be an error for /ô/, for below T. correctly takes the proto-Ugaritic form as /tawk-/ and indicates the Ugaritic form as /tôk-/ (p. 772 [§83.312], p. 775 [§82.412]).

— p. 197 (§33.322.42a). It is not clear why T. gives the alphabetic forms {mšry} and {mšrym} under “Syll. Befund,” rather than citing actual attestations of Ugaritic *nisbe* forms in syllabic script (either such forms exist and should be cited or they do not exist and the alphabetically written *nisbe* forms should have been cited under “Alph. Befund”).⁴²⁶

— p. 197 (§33.322.42b), p. 261 (§51.43c). I see no reason why {‘ly[...] } should be taken in RS 2.002:23 (*KTU* 1.23) as the epithet of *Ba‘lu* that T. vocalizes /‘alīy-/ (he indicates this text as a questionable attestation of the word, but nowhere does he suggest any other interpretation). *Ba‘lu* appears nowhere else in this text, of which the main interest is how ‘*Ilū*’s last offspring, ‘Dawn and Dusk’, came into being.⁴²⁷

— p. 197 (§33.322.42b), p. 273 (§51.46i), p. 280 (§52.222). Though the translation “Beginn” (pp. 197, 273) or “Anfang” (p. 280) is certainly not etymologically incorrect for Ugaritic *rīšyt*, in its one occurrence (RS 24.266:25’ [*KTU* 1.119]), the word plausibly serves as an adverbial modifying *šmn*, ‘oil’, at the beginning of the unit and thus has the specific meaning of ‘best’ (i.e., the first-pressed oil is the best).⁴²⁸

— p. 197 (§33.322.42c), p. 274 (§51.46j). The fact that the Hebrew word for ‘foreigner’ bears the *nisbe* ending (*nokrīʿ*) is an insufficient basis for taking Ugaritic {nkr} as the only example that would be attested of the Ugaritic *nisbe* ending on a singular noun not represented in the writing (“/nukrî/ < *nukrīyī”). Instead of consisting of the /qutl/ abstract base plus *nisbe*, as in Hebrew, the Ugaritic form probably showed one of the adjectival bases, as in Akkadian (*nakru*, *nakaru*, etc.). Furthermore, the fact that there are no certain cases of contraction of the *nisbe* ending, neither in the singular nor in the plural (see following remark), is best explained by the fact that the *nisbe* morpheme was /-Vyy-/ , rather than /-īy-/ , as indicated by T. (pp. 273-74 [§51.46h-k]). Though one may quibble on the basis of the fact that /i/, /ī/, and /y/ are homorganic in nature, the tenacity of the *nisbe* ending, not to mention the Massoretic vocalization with {iyy}, are indicators that the Hebrew, and probably Ugaritic and Phoenician, forms were phonetically /-iyy-/.

— p. 197 (§33.322.42c). Almost as unlikely is the analysis of {šdynm} and {šrm} in the *Kirta* text (RS 2.[003]⁺ iv 35, 36, 38, 39 [*KTU* 1.14]) as gentilic substantives with /-iyyīma/ having gone to /-īma/. Far more likely that the forms in question were intended to represent the names of the towns Tyre and Sidon (the final {-m} in each case would represent the plural or the dual morpheme, if the town name is expressed in terms of its principal quarters, or simply the ‘enclitic’ morpheme).⁴²⁹ In each case, the expression is ‘DN of GN’, and it is

⁴²⁶In the section on the *nisbe* ending (p. 273 [§51.46h]), T. cites only {u-ga-ar-ti-yu} (RS 19.042:15, 16¹ [*PRU* VI 79])—on this form, see above, remark to p. 182 [§33.242a], etc.).

⁴²⁷Pardee, *Context I* (1997) 274-83, in particular, p. 276 n. 7.

⁴²⁸Pardee, in *Verse in Ancient Near Eastern Prose* (1993) 216; idem, *Les textes rituels* (2000) 679-80.

⁴²⁹Pardee, *Context I* (1997) 336 n. 33.

just as plausible, if not more so, to take the expression as meaning ‘^ʿ*Aṭiratu* of Tyre, the goddess of Sidon’ as to see her as the goddess of the Tyrians/Sidonians. The other two examples of gentilic plurals claimed in this section to be written without the expected {y} ({gynm} in RS 9.453:28 [KTU 4.44] and {kpslnm} in RS 17.124:1, 6 [KTU 4.274]) have no basis in previous Ugaritology⁴³⁰ or in grammatical plausibility.

— p. 198 (§33.323.1b), p. 258 (§51.42a). In the first section cited, *pn(m)*, “Gesicht,” is identified as coming from /panay-/; in the second, the same word is glossed as “Vorderseite” and is identified as coming from /panaw-. That the same word is referenced is assured by the fact that the same text is cited each time (though, unfortunately, the text cited, RS 3.361 ii 14 [KTU 1.1], does not contain the word in question and, since the word is common, it is not possible to determine precisely to what text reference is intended).

— p. 198 (§33.323.1b), pp. 740-41 (§81.13a-b). On p. 198, the adverb ‘*I* is vocalized /‘alû/ and derived from “‘*alawû*”; on p. 740, the same vocalization is indicated but the derivation is given as “‘*alayu*.” A similar ambivalence is encountered in the case of the adverb ‘*In*: /‘alânû/ from “‘*alaw-ānû*” on p. 198, /‘alānu/ from ‘*alay-ānu*’ on p. 741. Such discrepancies are tied in with T.’s theory about III-w roots still being productive in Ugaritic (a theory that is fraught with difficulties: see above, seventh general remark, and below, remark to p. 457 [§73.131], etc., and cross-references there), but the user of the grammar is entitled at least to a unified presentation.

— p. 198 (§33.323.2b), pp. 771-72 (§82.311), p. 817 (§87.3), p. 818 (§87.4). The vocalizations of the various forms derived from the root BLY, ‘disappear, not exist’, do not consistently reflect the disappearance of the /y/: the noun *bl*, “Nichtsein,” is vocalized /balû/ ← /balayu/ (p. 198); the preposition *bl* is vocalized /bali/ or /balu/ with no specific derivation indicated (p. 771); the substantive *bl* that has come to function as a negative particle is vocalized /balî/, also with no specific derivation indicated; finally, the noun *blt* that is also classed with the negative particles is vocalized /bal(V)ti/ and not even a root derivation is mentioned specifically (p. 818), though one may infer from the “(V),” for which there is no evidence in any of the forms cited for comparison from other languages, that T. considers the root here also to have been BLY. A topic that someone with T.’s expertise should treat in more detail is that of substantives and particles apparently related to roots *tertiaef infirmaef* that show a short vowel or Ø-vowel in the position of the weak consonant, e.g., Biblical Hebrew *b^ʾlî*, *bal*, and *biltî^y*, all apparently from this root BLY.⁴³¹ Such variety in the comparative material makes any vocalization of the Ugaritic particles hazardous, but either consistency or arguments in favor of non-consistency would make T.’s presentation appear less arbitrary.

⁴³⁰ See, for example, van Soldt’s two principal studies of the Ugaritic toponyms (*UF* 28 [1996] 653-92; *UF* 30 [1998] 703-44), where neither word is considered, or del Olmo Lete and Sanmartín, *Diccionario I* (1996) 154, 222, where, respectively, {gynm} is taken as a personal name, {kpsln} as a common noun.

⁴³¹ It may be remarked here that short vowels in the place of the weak consonant in roots *mediaef infirmaef* can often be explained by shortening of an historically long vowel in a secondarily closed syllable, e.g., /*būšatu/ → /*būštu/ → /buštu/ → /bušt/ → /bōšet/, ‘shame’, in Hebrew.

— p. 199 (§33.323.2b), p. 269 (§51.45t), p. 597 (§74.624), p. 713 (§76.525), p. 810 (§85.7). Though the formation is not completely regular, Ford’s suggestion to take *tp* in RS 22.225:2 (*KTU* 1.96) as the 3 f.s. /YQTL/ form of PHY, ‘to see’,⁴³² certainly makes better sense of this incantation against the Evil Eye than does taking *tp* as a *t*-preformative noun or a /YQTL/ form of YPY, ‘to be beautiful’, the two options presented on p. 269⁴³³—on pp. 199, 597, and 713, only the nominal option is considered; on p. 810, no translation is offered for *tp*. (On the relationship of this line to the preceding, where the verb *šnwt* is found, see remark below to pp. 596, 597 [§74.624]; on the form {ypn} in RS 3.322+ iii 14 [*KTU* 1.19], which is certainly from PHY, see above, remark to p. 60 [§21.354.1b], etc.; on the implausibility of emending *tp* here to *tpnn*, see remark below to that word in RS 22.225:5-6, p. 579 [§74.511b], etc.).

— p. 199 (§33.323.2b). Though T. may be correct in analyzing the form *yph* in three Ugaritic rituals⁴³⁴ as an indicative form with contraction (“/yiphû/ < *yiphayu”), it is also possible that the form is jussive (/yipha/ ← /yiphay/), for the form occurs only in prescriptive rituals with *mlk*, ‘the king’, as subject of the verb. In general, however, the ritual formulation prefers indicatives over jussives and his analysis is probably correct.

— p. 199 (§33.323.2c). Structurally speaking, in spite of the poor condition of the top left portion of the tablet, {šty} in RS 2.002:6 (*KTU* 1.23) should be dual (T.’s alternative analysis) rather than plural.⁴³⁵

— pp. 199-200 (§33.323.3-4). T. provides no proof for his assertion that the final syllable of stems ending in /-a/ from III-y/w roots followed by the feminine morpheme /-at/, hence /-ayat-/ and /-awat-/ , contracted to /-ât-/. The corresponding forms in Biblical Hebrew do not show proto-Hebrew /ā/, but these forms could, of course, have disappeared through paradigm leveling with strong-root forms. (See already above, remark to p. 184 [§33.243.13-14]).

— p. 200 (§33.323.4b), p. 258 (§51.42a). {ṭa-bu}, an entry in the polyglot vocabulary RS 20.189A+B⁴³⁶, is listed under “Alph. Befund” rather than under “Syll. Befund.” This entry provides, by the way, a prime example of the highly hypothetical nature of the reconstructed forms of hollow roots (see above, seventh general remark): “/ṭâbu/” is reconstructed as coming from /ṭayabu/ with no explanation as to why a root that rather characteristically shows /ō/ in Hebrew should be II-y rather than II-w or as to why a stative adjective would

⁴³²Ford, *UF* 30 (1998) 218-29, 255-56; I have accepted this interpretation in my English study of the ritual texts and proposed that the absence of the second radical would have been a result of assimilation: /tiphay/ → /tippay/ → /tippa/ (*Ritual and Cult* [2002] 161 with note 12 on p. 164). On the quantity and the quality of the final vowel, see remarks below to pp. 653-71 (§75.53) and to p. 656 (§75.531e).

⁴³³The verbal option is not taken up in the appropriate section, §75.532: one finds no entry for WPY/YPY on p. 660.

⁴³⁴He cites *KTU* 1.90:1 (RS 19.013) and 1.168:1, 8 (RIH 77/120B⁺), to which is to be added RIH 77/2B⁺:11 (*CAT* 1.164).

⁴³⁵Pardee, *Context I* (1997) 276 n. 4.

⁴³⁶Laroche, *UF* 11 (1979) 479 (line 25 in the editor’s numbering; line 26 in Huehnergard’s numbering: *Ugaritic Vocabulary* [1987] 29, 131).

have been /qatal/ rather than /qatil/ or /qatul/. Moreover, whatever produced the form /ṭābu/ in Ugaritic, Akkadian, Aramaic, and proto-Hebrew obviously happened a long time before Ugaritic as a language appeared, and it is hardly appropriate to list Ugaritic /ṭābu/ as a /qatal/ form (p. 258), as though the derivation from “*ṭayab” were certain and a feature of proto-Ugaritic. To these implausibilities are added the proposal (put forward twice on p. 257, once for masculine forms, again for feminine forms), with a plausibility rating of ‘possible’ (“möglw.”), of deriving the hollow-root participles from the /qatal/-base rather than from /qātil/—this proposal does not re-appear below in the section on hollow-root active participles (pp. 642–43 [§75.521d]).

— p. 200 (§33.323.6), p. 427 (§73.132), p. 617 (§75.223), p. 663 (§75.533). In the first two sections cited, the f.s. imperative *dī* (RS 3.325⁺ v 48 [KTU 1.16]) is vocalized /dīⁱī/, in the other two /d^uī/.

— p. 202 (§33.421). T. includes in his definition of prothetic {ʾ} that it “im Kontext nicht konsonantisch artikuliert wird.” This is true of only some such words in Arabic (e.g. /ʾibn-/) and cannot, to my knowledge, be shown to be operative at all in any of the old Northwest-Semitic languages. There is, in any case, no clear evidence from the Ugaritic writing system that the /ʾ/ was not pronounced. T. claims only one such case, {dmʿt} for the expected {ūdmʿt} in RS 3.322⁺ ii 33 (KTU 1.19); others take {dmʿt} there as the singular, {ūdmʿt} elsewhere as the plural.⁴³⁷

— p. 202 (§33.423a), p. 426 (§73.122). The chances of {ūqrb} in RIH 78/20:5 (CAT 1.169) being a G-stem imperative are, in my estimation, very close to nil. Nevertheless, T. presents this as the only analysis on p. 202, with a cross-reference to §73.122, where the same analysis is presented again,⁴³⁸ along with the alternative analysis of the {ū} as “Partikel *u*” (but without a statement as to which particle spelled {ū} he means; this text is not cited below in either of the relevant principal paragraphs, viz., §83.141 and §84.22). In my estimation, none of these three analyses adequately explains the poetic unit {ḥṭ . nqh . ū qrb . ḥṭ}, ‘The rod has recovered, Yea the rod has approached’.⁴³⁹ There is in Ugaritic no G-imperative with prothetic /ʾ/, no emphatic particle {ū},⁴⁴⁰ and disjunctive /ʾô/, ‘or’, is inappropriate here. Hence, it must be the conjunction /ʾū/, which T. does not recognize

⁴³⁷E.g., del Olmo Lete and Sanmartín, *Diccionario I* (1996) 133.

⁴³⁸In this section, T. explains the {ū} as showing vowel harmony with the stem vowel /ʾuqrb/, but there is no reason to expect this verb to have had /u/ as its stem vowel: Hebrew shows unambiguous /a/, while the evidence from Aramaic and Arabic, favored by T. for his Ugaritic vocalization, is ambiguous (Aramaic shows the imperfect /yiqrab/ alongside imperative forms with /o/; the appearance in Arabic of more than one imperfect/imperative form to express various nuances of activeness/stativity is well known). Furthermore, if this criterion is valid for {ūqrb}, it should apply to {ibky} and to {išḥn} as well, which is not the case of the latter form (see above, remark to p. 170 [§33.211.2], etc.).

⁴³⁹See my brief treatment in *Verse in Ancient Near Eastern Prose* (1993) 212, and the more detailed treatment in *Les textes rituels* (2000) 876, 877, 883.

⁴⁴⁰Ford has recently analyzed the {ū} of *ūqrb* as the negative emphatic particle (UF 34 [2002] 155 and 171: “woe”) but such an analysis is no more required here than in any of the other putative occurrences of the particle (see remarks to p. 196 [§33.322.3c], etc., and to p. 805 [§84.22]).

(there is no entry for it in this grammar), but of which a growing number of examples indicate the existence.⁴⁴¹

— p. 203 (§33.432a). To the examples of infinitives of roots beginning with /l/ before which the expected preposition *l* is absent should be added RS 18.025:16 {lbš} (*KTU* 4.338).⁴⁴²

— p. 203 (§33.432b). This is another wasted paragraph, as none of the three examples cited in the eleven lines of text is in any way convincing (T. himself repudiates the third and presents alternative interpretations for the other two). All three examples are presented as involving imperfective verbs with pronominal suffixes containing one {n} too few; but in none of the three examples is the pronominal suffix necessary.⁴⁴³

— p. 204 (§33.442). The {y} of {šmym} in RS 3.322⁺ iv 24, 30 (*KTU* 1.19), instead of being a *mater lectionis* as T. proposes, may be either an historical writing or, perhaps even more plausibly, a *nisbe* ending designating ‘those who inhabit the heavens’. Though I am dubious about the validity of consonantal writing revealing pausal forms (see above, remarks to p. 60 [§21.354.1a], etc., and to p. 183 [§33.242b], etc.), I may remark that it is not at all clear why the {y} of this form is identified as a *mater lectionis*, while examples of aberrant {w} and {y} are identified in the following two paragraphs as historical writings preserved in pause.

— p. 206 (§40.24). As far as I can see, T. never explains why /tiš^ˈ-/ belongs with the other nouns cited here that show “inkompatible Wurzelstrukturen” (the others listed are *tl̥t̥*, ‘three’, *td̥t̥*, ‘sixth’, *špš*, ‘sun’, and *šrš*, ‘root’).

— p. 207 (§41.1). The description of the 1st and 2^d person pronouns as consisting “aus zwei Bildungselementen, nämlich einem Grundelement *’an und einer spezifischen Endung” is potentially confusing. /’an/ may be considered basic to the pronoun only in the sense that it appears in the pronouns of these two persons—but not in those of the third person. It would have been more precise to identify the /’an/ element as a deictic particle, as did Loewenstamm long ago,⁴⁴⁴ that has been prefixed to the properly pronominal element.

— p. 207 (§41.1). Though one may perhaps be generous enough to allow that it is not possible to determine with certainty whether the 2 m.s. independent pronoun in Ugaritic had final /a/ or /ā/, the fact that Hebrew shows *qameš* here makes it highly unlikely that proto-Ugaritic had /ā/ (T. indicates that “/’attā/” was derived from “/’antā/”). That being the case, a detailed argument should be mounted if one wishes to hold that final /a/ had become /ā/ in this word (and in like position elsewhere) in the Ugaritic language of the 13th century. T.’s openness to the possibility of /ā/ here, in the case of the 2 m.s. pronominal element of the /QTLa/-conjugation and elsewhere, and to other long vowels in similar positions (on the case of the 2 f.s. pronoun, see remark below to p. 465 [§73.331.3], etc.), would appear to reflect

⁴⁴¹ See Bordreuil and Pardee in *Une bibliothèque* (1991) 169; Pardee, “Index des mots ougaritiques” in *Les textes rituels* (2000) 1103; Bordreuil and Pardee, *RSO XIV* (2001) 411; idem, *Manuel* (2004) II 141.

⁴⁴² Pardee, *JAOS* 95 (1975) 616.

⁴⁴³ On the first example (RS 3.346:20 [*KTU* 1.2 iii]), see Pardee, *Context I* (1997) 248, with note 52; on the third (an identical form in RS 2.[014]⁺ v 28 [*KTU* 1.3] and in RS 2.[009]⁺ ii 14 [*KTU* 1.6]), *ibid.*, pp. 254 and 270.

⁴⁴⁴ *Lešonenu* 23 (1958-59) 77.

the expectation that, if the proto-Hebrew vowel had been short, it would have dropped by the time of Biblical Hebrew. Whatever the precise development to the Hebrew forms may have been, however, the evidence of Akkadian and Arabic indicates that the rule for final short vowels in passing from proto-West Semitic to Hebrew must be that such vowels were either dropped or retained and lengthened in Biblical Hebrew; in the case of /a/, such lengthened vowels in Biblical Hebrew, where length was still phonemic, were represented by the Massoretes as *qameṣ*. (The different developments of historical short vowels must, of course, be amenable to rule-ordered explanation, but the data may presently be insufficient for such an enterprise to be carried out in a fully convincing way.) Until evidence appears for /a/ → /ā/ or similar changes for the other vowels from proto-Ugaritic to Ugaritic, one must, in the light of the many archaisms undeniably present in the language (e.g., the fully functioning case system where the cases of singular nouns were expressed by short vowels), assume that the singular pronouns had final short vowels in Ugaritic (on the linking of short vowels and singular grammatical number, see further below, note 1146).

— p. 208 (§41.112.11). Though “Vokaldissimilation” was almost certainly a factor in the shift from /*ʾanōku/ to /ʾanōkī/ in Canaanite, analogy with other 1 c.s. pronominal forms consisting of or including /ī/ cannot have been uninvolved in the process.⁴⁴⁵

— p. 209 (§41.112.3), p. 212 (§41.131c), p. 228 (§41.32), p. 482 (§73.513.5b), p. 493 (§73.532), p. 615 (§75.212.5), p. 666 (§75.536), p. 784 (§83.112a), p. 827 (§89.231a), p. 863 (§93.33a). Given the implausibility of T.’s other examples of independent pronouns used datively in Ugaritic and Hebrew (see remark below to p. 212 [§41.131c] on {lm ank} in the *Kirta* text), his analysis of *āt* in the formula *āt(m) w ank ibgyh* (RS 3.361 iii 16 [KTU 1.1], RS 2.[014]⁺ iii 28' [KTU 1.3]) as meaning “Dir (allein) will ich es offenbaren” fails to convince. Either his stance that the imperative of the verb ʾTY/W, ‘to proceed (to a place)’, would have been spelled {ity} because pronounced /ʾitiyī/ (p. 482) is incorrect (and he cites no basis for that vocalization other than by a reference to §73.122, which we have already seen to be totally unconvincing⁴⁴⁶) and {āt} is in fact an imperative, or else his alternative interpretation of {āt} as an infinitival form used in place of an imperative (pp. 482, 493, 615, 666) is preferable. If {āt} in RS 18.140:19' (KTU 2.45:20), a passage that T. has not analyzed in this grammar, is a verbal form as has been proposed,⁴⁴⁷ it is more plausibly an imperative than an infinitive,⁴⁴⁸ for the use of the infinitive to replace a finite form is not particularly common in prose. The analysis of {āt} in this text as a verb, rather than as a pronoun, appears to be required from context, for the phrase *āt ng̃t* is isolated from the preceding and following phrases by the conjunction *w* and, for that reason, must be

⁴⁴⁵Blau, *Maarav* 2 (1980) 146-48.

⁴⁴⁶See remarks to p. 170 (§33.211.2), etc., p. 195 (§33.322.2c), etc., p. 202 (§33.423a), etc.

⁴⁴⁷J. Hoftijzer, “Quodlibet Ugariticum,” in *Zikir šumim. Assyriological Studies Presented to F. R. Kraus...* (Leiden: Brill, 1982) 121-27, esp. pp. 123-24.

⁴⁴⁸The corresponding verb in Arabic is of the /yaqtil/ type, plausibly the basis for T.’s assertion that the imperative in Ugaritic should have had an /i/-vowel in the first syllable (on pp. 425-26 [§73.121], T. reconstructs the base form as *qtil* which has in Ugaritic become /q̃til/).

translated ‘go to Nuḥašše’.⁴⁴⁹ Whether the verb ʾTY/W was of the /yiqtal/ type in Ugaritic, or the forms cited are infinitival, or, perhaps, the G-imperative of the /i/-type was /qatil/, must be decided by data yet to be discovered.

— p. 210 (§§41.112.8-9), p. 214 (§41.21), p. 227 (§§41.223.2-3), p. 289 (§53.211), p. 428 (§73.135), p. 429 (§73.15), p. 443 (§73.232), p. 446 (§§73.233.7-9), p. 468 (§§73.333.1-3). These are some of the principal sections where the dual morpheme which T. vocalizes /â/ is presented, viz., the dual nominative on nouns/pronouns and the dual morpheme on verbs. On pp. 289 and 468, the circumflex accent is explained as owing to the contraction from an original /ay/. Because in proto-Ugaritic /ay/ became /ê/, the derivation, if correct, must belong to an earlier stage of (West) Semitic (T. makes reference to Old South Arabian and to Egyptian). The Ugaritic morpheme should, therefore, be transliterated as /ā/ until data are provided that illustrate the plausibility of the proposed derivation.

— p. 211 (§41.12), p. 213 (§41.132e), p. 514 (§74.222.3). T. avoids taking {t}-preformative forms in the ritual texts as 2 m.s. (‘you will do X’), preferring to take them as 3 m.pl., active or passive (see also below, remarks to p. 285 [§52.5c], etc., p. 325 [§54.323a], etc., pp. 505-6 [§73.634a], p. 568 [§74.422], p. 582 [§74.515.1], p. 595 [§74.623], p. 601 [§74.623.3b], p. 602 [§74.623.3d], p. 633 [§75.512], etc., p. 735 [§77.51b], and p. 770 [§82.38]). The issue becomes crucial in RS 24.248:19 (KTU 1.104), for T. cites that text as a possible example of *hmt*, 3 m.pl. pronoun, used in the nominative case whereas in all other cases of *hwt*, ‘him’, *hyt*, ‘her’, and *hmt*, ‘them’, the pronoun is clearly in an oblique case. If one takes the verb forms as 2 m.s., however, the example disappears. The context is: {w tt ʾ. ʾ ḥdtn (18) tnm . w ḥdth (19) tdn . hmt (20) w tštn tnm}, which may be translated ‘You will again furnish (*nšp*-outfits), (18) two of each; then again (19) you will bring them near (20) and you will place (them) twice (i.e., the two of them, each as appropriate)’.⁴⁵⁰ T. translates “... und dann zweimal erneuert(?); am Neumondstag(?) werden sie (*sc.* in Z. 16 erwähnte *nšp*-Gegenstände/Kleider) niedergelegt/entfernt und zweimal hingestellt” (p. 213).⁴⁵¹ The form *tt* cannot have anything to do with the number ‘two’ and one wonders how *ḥdtn* could mean ‘be renewed’. *tt* may, therefore, be identified as an example of the imperfect of the weak root TWY discussed above (remark to p. 110 [§32.144.12b], etc.; cf. below, remark to p. 595 [§74.623]) and *ḥdtn* taken as a substantive used adverbially, literal meaning ‘anew’. Against T.’s translation, *ḥdth* in line 18 probably has a similar meaning (the previous paragraphs of the text had dealt with the eighth, ninth, and tenth days of the festival, and fixing the next event at the ‘new moon’ is out of place at such a juncture). Otherwise, our interpretations are similar, save for T.’s taking *hmt* as the subject of the two

⁴⁴⁹The interpretation of del Olmo Lete and Sanmartín, *Diccionario* II (2000) 321 “tú (estás en) LN,” is untenable in this prose text.

⁴⁵⁰On this interpretation, see *Les textes rituels* (2000) 567, 570-72; for the English rendition, *Ritual and Cult* (2002) 36.

⁴⁵¹T. maintains this interpretation in *UF* 33 (2001) 690, recognizing only that it is “sehr unsicher”; he does not address the issue of *hmt* as a nominative form. On T.’s treatment of line 17, see above, remark to p. 142 (§33.114.11), etc.

verbal forms.⁴⁵² Because of the uniformity of usage of the {-t} forms elsewhere as obliques, and because the {-t}-preformative forms are just as plausibly addressed to the cultic official in charge of the ceremony, it does not appear too strong to judge T.'s interpretation as straining the evidence. Whether or not the /-t/ morpheme began its life as an oblique morpheme, and though one may grant that {-t} has other functions in Ugaritic (see remark below to p. 230 [§42.3], etc.), its function when attached to 3^d person pronominal forms in Ugaritic and Akkadian is clearly that of an oblique marker (Akkadian examples cited by T. on p. 211). The Phoenician forms with {-t} in nominative forms cited on p. 213 are, of course, irrelevant for determining the function of the Ugaritic forms. If the Ugaritic forms can be shown to correspond to syntactic function, and such is the case up to the present, the Phoenician forms show either the remnants of such a system after the disappearance of case vowels and most case distinctions or else the remnants of another system.

— pp. 211-13 (§41.13), p. 231 (§42.4), p. 535 (§74.32). In the first section cited here, on the syntax of the independent pronouns, two elements are missing, one from a well-known text, the other from a new text. (1) In RS 1.002 (*KTU* 1.40), the 3 m.s. pronoun is employed as a second subject in a *casus pendens* (as what is sometimes incorrectly described as a copula) in the formulae *dbħn ndbħ hw t' nt'y hw*, which may be literally translated, 'The sacrifice, sacrificed is it; the t'-offering, offered is it'.⁴⁵³ (2) In RS 96.2039, the nominative form of the 3 m.s. independent pronoun is attested in a demonstrative function that is parallel to the use of the oblique form recognized by T.⁴⁵⁴ The expression *hn bnš hw* is attested three times in that text (lines 8, 10, and 15); the phrase means simply 'this/that servant', for in the third case, *hw* cannot be taken as the second subject of a cleft sentence ({w yūħd (15) hn bnš hw (16) w štnn[h] (17) bd . mlāk[ty]}, 'this servant must be seized, and deliver him over to my messenger-party').⁴⁵⁵

⁴⁵²T. does not even consider the option of taking *tdn* and *tštn* as 2 m.s., only that of taking them as 3 m.pl., impersonal subject (p. 213). Nor does he consider taking *tdn* as from a root other than YDY (see below, remark to p. 514 [§74.222.3], etc.). Finally, he has also cited this form in the process of claiming a similar usage in Phoenician for a 3 m.s. oblique form *h't* (*UF* 31 [1999] 737); because, however, the form *hmt* is clearly attested in Phoenician as a nominative, it might be better to see *h't* simply as a by-form of *hw* with no case-oriented value. However that may be, it is illegitimate to argue from Phoenician for an interpretation that is demonstrably aberrant in Ugaritic.

⁴⁵³Pardee, *Leslau* (1991) 1185, 1187; idem, *Les textes rituels* (2000) 125-27; idem, *Ritual and Cult* (2002) 82 with note 115 (p. 112). On p. 231 (§42.5), p. 535 (§73.32), p. 854 (§92.234b), p. 891 (§96.22a), T. incorrectly takes *hw* in this text as a demonstrative pronoun ("Dies ist ..."); for a full refutation of this analysis, see pages cited in *Les textes rituels*. There I also defend the analysis of the {-n} on {dbħn} as the enclitic particle, rather than the 1 c.s. pronominal suffix, and the {-n} of the verbal forms as that of the N-stem rather than the marker of the 1 c.s. imperfect. It is a shame that T. chose to give his interpretation of this important passage in *Anmerkungen* rather than defending it at length.

⁴⁵⁴Best attested in the Ugaritic *šumma izbu* text, in expression such as *dr' mlk hwt*, 'the (seed-)grain of that king' (RS 24.247+43 [*KTU* 1.103]), where *hwt* is the oblique form (specifically genitive). On this usage, see Pardee, *AfO* 33 (1986) 139.

⁴⁵⁵See Pardee, *Context* III (2002) 103; Bordreuil and Pardee, *Manuel* (2004), text 33 in the *Choix de textes*; Bordreuil, Hawley, and Pardee, publication of the Ugaritic texts discovered in 1994-2002 (in preparation). This text provides a clear example of deictic *hn* (and *ht*, see below, remark to p. 229 [§42.0], etc.) preceding a noun that is in turn followed by a demonstrative adjective, a syntagm that was previously

— p. 212 (§41.131c), p. 754 (§81.64a), p. 856 (§92.24a), p. 860 (§92.533). Rather than interpreting the independent pronoun *ānk* in the formula *lm ānk ksp* in the *Kirta* story as expressing the dative (‘Wozu soll mir Silber und gelbes Gold (dienen) ... ?’ [p. 754]), it is preferable to take it as what it is in the literary structure of the text, viz., as a response to the imperative formula *qh ksp* Though in its first occurrence (restored in RS 2.[003]⁺ i 51 [KTU 1.14]) we do not know what preceded this formula because the context is damaged, in the other extant occurrences (iii 23, vi 17), *lm ānk ksp* ... always follows the request to ‘take silver’. Thus Segert proposed⁴⁵⁶ that the formulation is to be understood as dependent on the offer to which it is responding, namely, *qh ksp w yrq hrš*, ‘Take silver and yellow gold’ (iii 22). The pronoun would thus be a functioning straightforwardly as a nominative: ‘Why (should) I (take) silver and yellow gold?’ This interpretation has recently been defended in a thorough and convincing study of verbal ellipsis (gapping) in Ugaritic poetry.⁴⁵⁷ Though the appropriate English translation in context might be something like ‘What need have I of silver ... ?’,⁴⁵⁸ a more literal, and not uncolloquial, English (at least American English) translation would be ‘Why? me?, silver ... ?’. One may also query the validity of the Hebrew parallels for the datival interpretation cited by T. *wa ʾanīʾ mʾtēʾ mispār* in Gen. 34:30 may express the speaker’s identification with his men, viz., not ‘I have only a few men’, but ‘I am (i.e., my forces are) only a few men’; and, in the formula *wʾattāh šālōʾm* (1 Sam. 25:6), the noun may be taken as an adverbial accusative, viz., not ‘(may there be to) you well-being’, but ‘(may) you (be in) well-being’.

— p. 212 (§41.131f), p. 857 (§92.24c). T. is correct in denying that the independent pronoun in a phrase such as *šmk āt ygrš* (RS 3.367 iv 11'-12' (KTU 1.2) functions as a copula. On the other hand, it is debatable whether this use of the pronoun may be identified as a “Genetivfunktion.” What such expressions do (and T. cites parallels from several other Semitic languages in which the function is identical), is to emphasize the genitival function of the pronominal suffix on the preceding noun; this is accomplished by setting the independent pronoun in apposition to the suffixal pronoun, literally, ‘Your name, you, (is) Yagrušu’. The only difference between the Semitic examples cited and corresponding usage in the modern European languages is that we tend to place the pronoun in first position. Thus, I translated this passage “You, your name is Yagrušu,”⁴⁵⁹ and I would hold that that translation reflects the Ugaritic better than T.’s “Dein Name ist Ygrš.” Unfortunately, no examples are attested

unattested. The absence is remarked by T., p. 231 (§42.4), but on p. 233 (§42.73) he suggests that the sequence *hn* + noun + *hnd* in RS 16.402 31-32 and 37-38 is its equivalent (below, in the remark to p. 229 [§42.0], I accept T.’s earlier suggestion to analyze *hn* in this letter as a local adverb).

⁴⁵⁶S. Segert, *A Basic Grammar of the Ugaritic Language with Selected Texts and Glossary* (Berkeley: University of California, 1984) 121.

⁴⁵⁷C. L. Miller, “Patterns of Verbal Ellipsis in Ugaritic Poetry,” *UF* 31 (1999) 333-72, esp. pp. 366-70.

⁴⁵⁸Pardee, *Context I* (1997) 335, 337; Wyatt, *Religious Texts* (1998) 184 (“Why should I take ... ?”), 195 and 204 (“What would I want with ... ?”).

⁴⁵⁹*Context I* (1997) 249. It may be remarked, however, that there is nothing aberrant in using the order corresponding to the Semitic order in the languages I know. Certainly, in French, ‘ton nom, toi, sera Yagrušu’, corresponds to the standard spoken language.

in Ugaritic of the third-person pronoun in apposition to an object suffix to permit us to determine whether the oblique form of the pronoun (*hwt*) would have been used. Judging from the Hebrew example *wʾlīy ʾanīy ʿabdʾkā* (1 Kings 1:26), ‘and to me, I (who am) your servant(, he did not send an invitation)’, where the independent pronoun functions as the subject of an embedded nominal sentence, one may guess that the nominative pronoun would have been used in Ugaritic.

— p. 212 (§41.132b). The restoration of *hyt* in RS 24.247+:14 (*KTU* 1.103) is entirely hypothetical because the restoration of the last word of the line as {*yḥsl*} is out of the question.⁴⁶⁰

— p. 213 (§41.132b). It is not certain that {*hmʿt*} is the last word of RS 16.401:8', as is indicated in *KTU* 2.32 and accepted here by T.

— p. 213 (§41.2). Though pronouns attached to prepositions are indubitably of the same type as those attached to nouns, on what basis may their *function* be said to be identical and genitival (“haben genitivische Funktion”) in Ugaritic (as opposed to some proto-stage of the language when all such particles may have been sensed as substantives)?

— p. 214 (§41.21), p. 221 (§41.221.51), p. 223 (§41.221.61), p. 225 (§41.222.2a). It is dubious that {-nk}, {-nh} (3 m.s. and 3 f.s.), and {-nkm} are pronominal variants when they are defined as attached only to prepositions; it is rather the prepositions that show simple forms and forms expanded with {-n}. If these were expanded forms of the pronominal suffixes, one would expect them to have arisen by analogy with verbal forms where the /n/ is at home; but, since the forms {-nk} and {-nkm} are not (yet) attested with verbs, that solution must be considered dubious. Seeing the forms in question as variants of the preposition becomes clear from the examples cited on pp. 223 and 225: while one might wonder at {*lpnnh*}, ‘before her’ (RS 3.362+ ii 17' [*KTU* 1.10]), when one considers that such forms are rare while {‘mn} + suffix is much more common, it appears necessary to conclude that the forms of the prepositions expanded with {-n} were common while the same phenomenon with compound prepositions was analogical and infrequent. Another instructive example is {*b dʿnhlm*} in RS 18.031:18 (*KTU* 2.38), which, as Hoftijzer once argued,⁴⁶¹ probably means ‘in/from their hands’. T. admits this example, at least theoretically—he appears worried only about whether the reading is correct or not (p. 226 [§41.222.4]), which it is—without noticing that it demonstrates unequivocally that the /n/ of the forms with a singular suffix belongs with the preposition, not with the suffix, for {-nhm}, like {-nk} and {-nkm}, is never attested as a pronominal suffix. Hence listing these forms in the section on pronouns is organizationally incorrect.

— p. 214 (§41.21), pp. 224-25 (§41.222.1b), p. 228 (§41.31, 33), p. 823 (§89.11a), pp. 834-35 (§89.34). T.’s proposal that the 1 c.pl. pronominal suffix would be /-nā/, /-nê/, or /-nay(V)/ is too strongly influenced by the five cases of that suffix written {-ny}, three in two texts (RS 2.[014]+ v 33-34 [*KTU* 1.3] {*klnyy*} and RS 24.266:28'-29' [*KTU* 1.119] {*tgrny*} and {*hmytny*}), the fourth in a variant form of the first (RS 2.[008]+ iv 45'-46' [*KTU*

⁴⁶⁰ Pardee, *AfO* 33 (1986) 133; idem, *Les textes rituels* (2000) 541.

⁴⁶¹ *UF* 11 (1979) 387. This analysis has been adopted by Bordreuil and Pardee, *Manuel* (2004) II 86, 87, 150.

1.4] {klnyn}), and the fifth in yet another text (RS 3.343+ v 20 [KTU 1.15] {b'lny}).⁴⁶² Though T. allows for the possibility that the {-y} may be the enclitic morpheme, that analysis is not the one he prefers. When one considers that the 14th-century Canaanite form was written syllabically {-nu} (cited p. 224) and that the corresponding form may be attested syllabically at Ras Shamra,⁴⁶³ the vocalization with /ū/, and the corollary that the {-y} is the enclitic particle in the five cases cited, must be preferred (on the problem of the 1 c.du. suffix, see above, remark to p. 196 [§33.322.3b], etc.). In RS 2.[014]+ v 33-34 (KTU 1.3), where the form is {klnyy} and two referents may be evoked, that form may consist either of the noun *kl* + 1 c.du. pronominal suffix + enclitic {-y} or of the same noun + 1 c.pl. pronominal suffix (referring to all the gods) + the enclitic particle repeated.⁴⁶⁴ In RS 2.[008]+ iv 45'-46', where the form is {klnyn} and the possibility of two referents appears ruled out, it would consist of the noun *kl* + enclitic {-y} + enclitic {-n}. This comparison leads to the conclusion that in the first case the suffix is 1 c.pl. and that the enclitic {-y} is repeated, while in the latter this particle is followed by {-n}. T., of course, takes both forms as representing a 1 c.pl. suffix {-ny} with enclitic {-n} or {-y}. (On the pronominal element of the 1 c.pl. /QTLa/, see remark below to p. 467 [§73.332.5].)

— p. 214 (§41.21), pp. 221-23 (§41.221.4), pp. 223-24 (§41.221.6). Without discussing the question of length, T. represents in these paragraphs the suffixal forms of the 3 m.s. and 3 f.s. personal pronouns as “*hū*” and “*hā*.” Because Hebrew shows /-hā/ for the feminine form and Aramaic shows variants of both the masculine and the feminine that could only have arisen from original short vowels (e.g., Ø-vowel for the 3 m.s. form on singular nouns), it is highly unlikely that Ugaritic would already have shown forms with a secondarily(!) lengthened vowel. Moreover, the neutrality shown here by the *anceps*-notation is sometimes not observed in various vocalizations below (e.g., p. 750 [§81.4b], where the 3 f.s. pronoun is vocalized “*hā*”).

— p. 215 (§41.221.11b), p. 217 (§41.221.14). None of the three cases of a m.s. noun in the accusative case with Ø-marked 1 c.s. suffix is convincing⁴⁶⁵ and, since there are clear examples of such forms written with {-y} (p. 216 [§41.221.12b,c]; p. 218 [§§41.221.15b, 16]), T.’s attempt to prove that the suffix was properly /ī/ in such forms (as in Akkadian and Phoenician: p. 217 [§41.221.14]) may not be accepted at face value. That being the case, the conclusion drawn on p. 217 that the “Terminativending” must have been “/-â/ < *-ah” is valueless, for (1) such forms may be simple accusatives, and (2) as is shown by the common usage of the adverbial morpheme {-h} in Ugaritic and Hebrew, the hypothesis that it haphazardly became /-â/ in Ugaritic cannot be demonstrated by a few dubious cases of

⁴⁶²On p. 204 (§33.445), T. had already referred to {-ny} as “eine ‘vollere’ Form” in the cases of {b'lny}, {tgrny}, and {hmytny}, but did not there refer to the other two forms.

⁴⁶³Huehnergard, *Syria* 74 (1997) 219.

⁴⁶⁴Pardee, *Context I* (1997) 255, n. 112 (on p. 822 [§88.24], T. accepts that the enclitic particle {-m} may be repeated and there should, for that reason, be no particular objection to that analysis of {-y}).

⁴⁶⁵On RS 24.293:9 (KTU 1.133) and RS 24.244:2 *et passim* (KTU 1.100), see Pardee, *Les textes para-mythologiques* (1988) 157, 201-2. On the specific case of {āh}, see above, remark to p. 51 (§21.341.21a), etc.

accusative singular substantives that are supposed to bear the 1 c.s. pronominal suffix. His parallel conclusion regarding the “Lokativendung,” viz., that it would be /ū/, is more difficult to assess because the very existence of the morpheme is difficult to assess. On the “Terminativ” as a true case, see below, remark to pp. 320-25 (§54.3).

— p. 215 (§41.221.11c), p. 317 (§54.221a), p. 715 (§76.534). On p. 215, T. translates *bt*, ‘daughter’, in RS 3.340 i 16, 17 (*KTU* 1.18) as “(o) meine(?) Tochter,” on p. 317 as “(o) (meine) Tochter!,” on p. 715 as “(meine) Tochter”;⁴⁶⁶ on p. 215, he translates *šps ūm* in RS 24.244:2 *et passim* (*KTU* 1.100) as “(o) Sonne, (o) meine Mutter!,” on p. 317 as “(o) Sonne, (o) meine Mutter!” T.’s own hesitations regarding whether *bt* bore the 1 c.s. pronominal suffix are well founded and the case of *ūm* is equally dubious.⁴⁶⁷ His argument (p. 215) that *ūm* should be analyzed as bearing the pronominal suffix because *ūmh* does so in the narrative introduction may not be accorded any probative value precisely because *ūm* is in direct speech while *ūmh* is part of the narrative.

— p. 216 (§41.221.12a). T.’s reading of ‘*bdy* in RS 19.181A:2 for {ʿbɖy} in *CAT* 2.67:2 is certainly correct.⁴⁶⁸

— p. 218 (§41.221.15a), p. 429 (§73.142), p. 594 (§74.622.3), p. 623 (§75.237d). The sign {y} of the sequence {rʿyššā} in RS 15.007:5 (*KTU* 2.15) plausibly does not belong with {rʿ}, where it would indicate either the 1 c.s. suffix or the third radical, and the noun is probably not vocative (“(o) mein Freund”). Rather, the division {rʿ yššā} is to be preferred: ‘May my friend cause to go forth ...’.⁴⁶⁹ (On p. 429, T. qualifies this division as “unwahrscheinlich,” but he does not explain why.)⁴⁷⁰

— p. 219 (§41.221.17b). T. queries whether the {n} of *ūlny* // ‘*zmny* in RS 3.367 iv 05’ (*KTU* 1.2) is part of the nominal stem or part of what he takes as a 1 c.s. pronominal suffix.⁴⁷¹ It is, however, highly unlikely that the {-y} is a pronominal suffix; it is rather the

⁴⁶⁶Only on p. 715 is RS 2.[014]⁺ v 27 (*KTU* 1.3—restored to read {ydʿ[tk] bt}) included alongside the text from the ‘*Aqhatu* epic.

⁴⁶⁷I have consistently translated *ūm* as non-suffixal (*JANES* 10 [1978] 74-82; *Les textes para-mythologiques* [1988] 202; *Context I* [1997] 295-97; *Ritual and Cult* [2002] 174-78); in the first two cases a vocalized text was provided, and the vocalization did not reflect the pronominal suffix (“*šapša ’ummi*” in *Les textes para-mythologiques*, p. 201, was a *lapsus calami* for *šapša ’umma* or for *šapši ’ummi*, depending on whether I at that time retained the hypothesis that the vocative was expressed by the accusative [*JANES* 10, pp. 74-82] or had shifted to the hypothesis of the genitive [Bordreuil and Pardee, in *Une bibliothèque* (1991) 158]—on this hypothesis, see below, remark to p. [§54.214b], etc.).

⁴⁶⁸In *AuOr* 13 (1995) 235, T. indicated more explicitly {ʿbɖɖʿy}. My collation shows that only the first sign is epigraphically certain, though the traces remaining of the other three signs leave no doubt concerning the correctness of the reading.

⁴⁶⁹Pardee, *Context III* (2002) 108 n. 157.

⁴⁷⁰In *Kausativstamm* (1990) 29, T. chose the division {rʿ yššā}, and his explanation for now finding that division implausible would have been welcome.

⁴⁷¹In *UF* 31 (1999) 737, T. presented the linking of the {n} with the 1 c.s. suffix in this text as possible.

morpheme by which a noun is transformed into an adjective (the *nisbe*). The proper interpretation is “the powerful one ... the mighty one” (T.’s third option).⁴⁷²

— p. 219 (§41.221.17c,d). Given that the preposition ‘*m* shows two forms, {‘*m*} and {‘*mn*}, and that the first with the 1 c.s. suffix appears only as {‘*my*} while the second appears as {‘*mn*} and {‘*mny*}, it appears necessary to conclude (1) that {‘*m*} ends in a vowel that does not allow contraction with the 1 c.s. suffix /-ya/, and (2) that the situation is the opposite with {‘*mn*}, viz., that {‘*mn*} + 1 c.s. suffix is /‘*immānî*/ while {‘*mny*} consists of this same form to which the affirmative particle /-ya/ has been added (/‘*immānîya*/). These conclusions lead to a third: the base form of the simple preposition was probably /‘*imma*/ (i.e., originally the accusative of the substantive whence the particle descended) while that of the expanded form is /‘*immānu*/, for the 1 c.s. suffix after /-u/ becomes {-Ø}, probably /-î/, while it does not contract with /-a/ and /-i/ (see above, remark to p. 215 [§41.221.11b]). T.’s primary presentation of {‘*mny*} as *plene* writing of /‘*immānî*/ is thus to be rejected in favor of his alternative analysis according to which {-y} would represent the enclitic particle (on this particle attached to the prepositions *b* and *l*, see above, remark to pp. 37-38 [§21.322.5], etc.).

— p. 220 (§41.221.2), p. 648 (§75.524). Because {ṣḥn} and {qrān} in RS 2.[022]⁺ i 22, 23 (*KTU* 1.5) are the only examples of the 1 c.s. pronominal suffix taking the form of {-n} on 3 m.s. perfect verbs, T. might have considered it worth his time to consider the possibility that the verbal forms are in fact imperatives.⁴⁷³

— p. 220 (§41.221.2), p. 230 (§42.2), p. 458 (§73.273.2), p. 548 (§74.412.22), p. 619 (§75.228c), p. 701 (§76.427b), p. 733 (§77.413), p. 789 (§83.122d), p. 822 (§88.22a). Not even “evtl.” (p. 220) may the {-n} of {tlīkn} in RS 34.124:10 (*CAT* 2.72) be plausibly considered a 1 c.s. pronominal suffix with dative force. Below, p. 230, this analysis is not preferred (it is mentioned but only as a third possibility on p. 458, as a second possibility on p. 548, as a possible translation on pp. 789 and 822⁴⁷⁴), but the equally implausible interpretation as a /YQTLØ/ with energetic suffix is put forward on pp. 458, 701 (here among possible cases of /YQTLØ/-perfectives in prose, none of which is convincing), and 733. As his first interpretation, T. offers everywhere except on p. 701 the German present tense, the same that Bordreuil and I preferred in our edition of the text.⁴⁷⁵ (Caquot, in his preliminary edition, translated by the past tense in French⁴⁷⁶; in my preliminary study based on this presentation by Caquot, I translated by the present tense in English.⁴⁷⁷) The real question, however, is not the translation but the reason for the use of the /YQTLu/ form in the text,

⁴⁷²Pardee, *Context I* (1997) 248; Smith *apud* Parker, *Ugaritic Narrative Poetry* (1997) 102 (“the noble ... the mighty”).

⁴⁷³Pardee, *Context I* (1997) 265.

⁴⁷⁴On p. 733, this interpretation is explicitly denied plausibility on the basis of the prepositional complement ‘*m* that is used often with this verb.

⁴⁷⁵Bordreuil and Pardee, *Une bibliothèque* (1991) 144.

⁴⁷⁶*ACF* 75 (1975) 430.

⁴⁷⁷*BiOr* 34 (1977) 3.

which is the obvious analysis of a 2 f.s. imperfective form in a prose text, a question that T. does not address. One may surmise that the act in question had occurred more than once; such may be also the explanation for the D-stem: ‘Why do you repeatedly send a *ḥupṭu* (and not what I’ve asked you to send)?’⁴⁷⁸

— p. 220 (§41.221.2), p. 430 (§73.162b), p. 675 (§75.63). In RS 15.007:3 (*KTU* 2.15), the {y} of {ḥnny} is more plausibly the enclitic particle than a *mater lectionis*, especially when one considers that there is no reason why the 1 c.s. objective suffix should have had a long vowel in Ugaritic (T. vocalizes it /-nī/, but never says why—on the question of /a/ vs/ /ā/ for the 2 m.s. pronouns, see above, remark to p. 207 [§41.1]). The form should, therefore, be vocalized /ḥunnaniya/, i.e., /ḥunn + a + ni + ya/. In addition, if one is to transliterate Biblical Hebrew with indications of length, it should be done according to the Sephardic tradition, where /o/ and /ā/ have not fused as in the Ashkenazic tradition. Thus the first syllable of the Hebrew form cited as “*ḥānnenī*” should have /o/, not /ā/. Moreover, the second syllable should either be closed (/ḥonnenni^y/ or have a long vowel (/ḥonnēnī^y/); it is the second form that is actually attested. Only on p. 675 does T. propose a vocalization of the Ugaritic form and there he follows his erroneous transcription of the Hebrew form and indicates the Ugaritic imperative stem as /ḥann-/; it must, of course, have been /ḥunn-/.⁴⁷⁹

— p. 221 (§41.221.3b), p. 223 (§41.221.52c), p. 503 (§73.623), p. 504 (§73.628), p. 635 (§75.512). T. credits this reviewer with the reading {ṯṯnn ṯ.ṯ nn} in RS 15.174:17 (*KTU* 2.21).⁴⁸⁰ He parses the form as 2 m.pl. with no sign of doubt on pp. 503 and 635, but as 2/3 m.pl. on p. 223. I see no reason, however, why the form should not be singular, addressed to the recipient of the letter, i.e., with the {nn} form of the pronominal suffix attached to the energetic form of the verb (/wa la tatinannannanu/, ‘you must indeed give it’). As we will see below (remarks to pp. 222-23 (§41.221.52c, etc., and to pp. 497-506 [§73.6]), T. does not believe in the existence of {nn} which functions as a pronominal suffix attachable to an energetic verbal form and invents his Energetic II to deal with the problem.

— pp. 222-23 (§41.221.52c), pp. 501-4 (§73.62). It is to my mind inherently implausible to posit a third “energetic” form alongside the two that are well attested in Arabic (and in

⁴⁷⁸ On the unlikelihood that the use of the D-stem is linked with the multiplicity of the *ḥupṭu*-soldiers, see remark above to p. 137 (§33.112.31), esp. note 256.

⁴⁷⁹ This vocalization of the imperative, Hebrew or Ugaritic, is confirmed by the standard Qal imperfect in Hebrew, formed on the stem /yaḥunn-/, in conformity with the fact that the verb is transitive, in spite of the English translation ‘to be gracious to’. The only exception is found in Amos 5:15, where {yḥnn} is vocalized according to the Massoretic tradition as /yeḥ^enan/. Because this is the only imperfect form written with two {n}s in the Hebrew Bible that shows a Qal vocalization pattern, one might guess that it was in fact a Polel form, which is attested elsewhere as an intensive of the Qal (‘to direct favor toward’, in contrast to the Piel, which functions as a factitive of the underlying notion of ‘grace’, viz. ‘to make favorable, gracious’). On the common contrast of Piel and Polel forms of stative geminate roots in Hebrew and Ugaritic, see below, remarks to pp. 575-76 (§74.50), to pp. 577-78, 678-79 (§74.511a, b), and to p. 580 (§74.511c). This root represents a similar situation with a transitive root.

⁴⁸⁰ *JNES* 43 (1984) 244.

Hebrew), viz., /-(a)n/ and /-(a)nna/⁴⁸¹ on the basis only of forms bearing the 3rd person singular pronominal suffixes ({yqtl_{nn}}). T. posits the existence alongside these two energics of a form that would be parallel to the Akkadian ventive ending /-nim/ and to Sabaic {-nn}. Because the {YQTL_{nn}} forms are often written {yqtl . nn}, it appears certain that they were perceived as consisting of a verbal form followed by a quasi-lexical suffixal morpheme. For that reason, and because the ending {-nn} is attested only with pronominal suffixes, i.e., never as a simple “energetic”, form, it appears more plausible to see the form as having arisen through re-analysis of an ending that consisted historically of the energetic ending and a pronominal ending as itself pronominal, i.e., /YQTL_{nn}hu/ → /YQTL_{nn}nu/ with /-(a)nnu/ perceived as pronominal and usable after all the imperfective forms, including the energics.⁴⁸² T.’s theory simplifies this process ({nn} would consist of /-nV_{nn}hu/ → /nV_{nn}nu/), but at the cost of creating a third energetic form. Specialists in comparative Semitics will decide which theory is the more plausible.⁴⁸³ (For more reactions to T.’s view of the energetic forms in Ugaritic, see below, remarks to pp. 497-506 [§73.6].)

— p. 222 (§41.221.52c), p. 687 (§76.331). In 1996,⁴⁸⁴ T. criticized the authors of *CAT* for restoring the verb in RS 2.[014]⁺ v 1 (*KTU* 1.3) as {[ā]mšḥ . nn}, viz., assuming a vocalization /ʾamṣuḥ-/), and asserted that the proper restoration is {[imšḥ . nn}. In the first section cited here he cites the form as “*imšḥ.nn*” but in the second as “[*a*]mšḥ_{nn}.” The former is in keeping with his rule that III-guttural fientive verbs should be of the /yiq_{tal}-/ type (p. 171 [§33.212b]).

— p. 223 (§41.221.52c), p. 689 (§76.341), p. 701 (§76.427c), p. 734 (§77.51), p. 821 (§88.21a), p. 903 (§97.32). Since the form {ylmd_{nn}} occurs in the prose colophon of RS 92.2016 (l. 42') (RSO XIV 53), it is unlikely that it is to be parsed as a /YQTLØ/ perfective (an option considered explicitly on pp. 689 and 701, but not preferred). *ʾIlīmilkū* is claiming here to have inscribed this tradition without having received it by formal schooling and the form is, therefore, according to T.’s classification on p. 689, (negative)-iterative, ‘no one ever taught him’. T.’s own translation with “müssen” (“niemand mußte ihn (dabei) belehren”—see in particular p. 734) also works in English (‘no one had to teach it to him’). But the translation must be seen as an expression of iterativity rather than of obligation or volition:

⁴⁸¹ The forms are listed in all Arabic grammars. In Hebrew, the /-(a)n/ ending is retained after long vowels as the so-called “paragogic *nun*” and with suffixes (/yiq_tʿlennū/ ← /YQTL_{nn}hu/); the /-(a)nna/ ending is attested only rarely with suffixes (/yiq_tʿlenhū/ ← /YQTL_{nn}ahu/).

⁴⁸² Pardee, *JNES* 43 (1984) 244-45, n. 14.

⁴⁸³ In the article cited in the preceding note (cf. also *AuOr* 16 [1998] 256), I posited that the two verbal forms with three {n}s at the end (RS 1.026⁺:11 and RS 15.174:17) may be explained by the hypothesis of a second re-analysis, viz. /YQTL_{nn}annu/ would have been taken as consisting of a verb followed by a pronominal suffix, whence the ‘suffixal’ form written {nn}, which could be used after the long energetic form, giving /YQTL_{nn}annannu/. The existence of the suffix written {nnn} is not yet, however, attested with certainty, for the two examples of that orthography attested to date all occur in poorly preserved passages where the first {n} could be part of the plural morpheme rather than part of the suffix. This is in fact how T. analyzes such forms. Until a certain attestation of a singular form with a suffix {nnn} occurs, the existence of the latter is uncertain.

⁴⁸⁴ *AfO* 42-43 (1995-96) 268.

on p. 734, this passage is treated along with other /YQTLu/ forms that function as weak expressions of volitivity, quite another function of the imperfective, I should think.

— p. 223 (§41.221.52c), p. 426 (§73.121.1c Anm.), p. 449 (§73.243.21). In the first section cited, T. quotes RS 1.037:4 (*KTU* 1.55) as {[...]idm . nn} and translates “... schminkte ihn”;⁴⁸⁵ in the second, he gives the line as {[...]idmnn[...]} and says that the passage is too broken to be interpreted; in the third, the transcription is {idmnn} and the form is listed as a possible example of a /yiqtal/ imperfect.

— p. 223 (§41.221.52c), p. 224 (§41.221.62b), p. 502 (§73.623), p. 655 (§75.531d), p. 665 (§75.534), p. 669 (§75.537d). At the end of a long list of /YQTL/ verbal forms bearing the pronominal suffix form written {-nn}, T. attaches a much shorter list of what he takes as /QTLa/ forms bearing the same suffix (p. 223). Two of these examples may be rejected out of hand. Given the form {y<l>âk} in line 41, I see no plausible basis on which to analyze *yblnn* in lines 38 and 40 of RS 2.[008]⁺ v (*KTU* 1.4) as a /QTLa/ form; in RS 29.093:13 (*KTU* 2.70), *kḥdnn* is to be taken as an imperative.⁴⁸⁶ Other analyses of two other examples are possible. Because the sequence {yrâun ... tt^c . nn} in RS 2.[02]⁺ ii 6-7 (*KTU* 1.5) is paralleled by {yrû ... tt^c} in RS 2.[009]⁺ vi 30 (*KTU* 1.6), both verb forms are perhaps best taken as infinitives with narrative function ({yrû} shows that {yrâun} is apparently representing either /yarâ’una/, i.e., the infinitive + the enclitic morpheme *-na*, or else /yarâ’unnu/, i.e., the infinitive + the secondary pronominal suffix *-nnu*).⁴⁸⁷ In RS 24.258:19 (*KTU* 1.114), {ngšnn} may also be taken as a participle (or, perhaps, an infinitive).⁴⁸⁸ Such analyses imply, of course, that the suffix written {-nn} had become an independent entity attachable to verbal nouns and verbal adjectives (see above, remark to pp. 222-23 [§41.221.52c], etc.). *phnn* (RS 19.022:6’ [*KTU* 2.62]) appears in a broken context and its analysis is uncertain. A possible example of /QTLa/ + *-nn* occurs in a text that may be among the oldest preserved: {Iw^l ytn . nn} in a contract dated to the time of ‘*Ammittamru* II (RS 16.382:11 [*KTU* 3.5]; cf. also RS 15.111:8 [*KTU* 3.2], for which T. has proposed reading the same form⁴⁸⁹) is paralleled by {w . pdy . h[m]} in RS 16.191⁺:12

⁴⁸⁵ He had already proposed the reading with a word-divider in *AuOr* 13 (1995) 234.

⁴⁸⁶ Pardee, *AAAS* 29-30 (1979-80) 28; idem, *Context* III (2002) 110; Bordreuil and Pardee, *Manuel* 2004), text 28 in the *Choix de textes*. See remark below to p. 559 (§74.414.2).

⁴⁸⁷ See above, remark to p. 38 (§21.323), etc. In *Context* I (1997) 266, I took the bicolon in RS 2.[008]⁺ ii as part of *Ba’lu*’s dark view of the future, but the analysis as a return to the narrative may have more going for it (e.g. Smith *apud* Parker, ed., *Ugaritic Narrative Poetry* [1997] 143; Wyatt, *Religious Texts* [1998] 121). Doing so places the only two instances of YR’ // TT^c in tight literary parallel but at a great distance in the poem (in the other instance, it is *Môtu* who is depicted as fearing *Ba’lu* near the end of the Baal Cycle as preserved).

⁴⁸⁸ The former was my analysis in *Les textes para-mythologiques* (1988) 26-27; there also the form should have been /nāgišunnannu/, rather than “*nāgašannannu*” as I indicated on p. 21. The evidence just discussed is from the infinitive, but one might think that when these suffixes were attached to the participle it also maintained the correct case.

⁴⁸⁹ Originally in *AuOr* 13 (1995) 236: “[w . yt]n*. (?)nn”; here p. 223 (§41.221.52c: “[yt]n*. ? nn”), p. 637 (§75.514: “ytn-nn”), p. 705 (§76.523: “[w y]tnnn”—the last presented as a “n[eue] L[esung],” which it certainly is since the {t} was not previously indicated as visible). My collation with Robert Hawley in June of 2004 showed that there is certainly no word-divider before the second-last sign; the third-last sign is

(*KTU* 3.4), which may only be /QTLa/ or the infinitive (/YQTLØ/ is ruled out). On p. 224, three putative examples of the 3 f.s. pronominal suffix {-nn} attached to /QTLa/ verbs are cited. One of these must be rejected out of hand (see second following remark), while the other two are more plausible—more plausible because two non-preformative forms appear in parallel in a bicolon and the text indubitably recounts *Kirta*’s exploits. Nevertheless, the preceding forms of this section are all /YQTL/ forms, and the possibility must be considered that {grnn} and {šrnn} in RS 2.[003]⁺ iv 49, 50 (*KTU* 1.14) are in fact infinitives. The fact that {h} is unattested as a pronominal suffix on /QTLa/ verbal forms must be considered as an argument in favor of the hypothesis that the pronominal suffix alloforms *-n* and *-nn* could be attached to /QTLa/, at least in prose. The small number of attestations remains frustrating. As regards the three legal texts discussed above in this remark (RS 15.111:8, RS 16.191⁺, and RS 16.382), a not unlikely scenario is that the first occurrence of the verb in question was /QTLa/ whereas the repetition of that verb was each time the infinitive (respectively, *w ytnn*, *w pdyhm*, and *w ytnnn*); the purpose of the different forms would have been to provide variation between the first and the second usages of the selfsame verb. In this case, all three examples would be removed from the list of /QTLa/ forms with *-n* or *-nn* suffixes. (For another possible example of *-n* attached to a /QTLa/ form, see remark below to p. 500 [§73.611.2f], etc.)

— p. 224 (§41.221.62b), p. 430 (§73.162b), p. 503 (§73.627), p. 558 (§74.413.2), p. 559 (§74.414.1,2), p. 669 (§75.537d). It is remarkable that T. considers that *grnn* in RS 2.[003]⁺ iii 6 and iv 49 (*KTU* 1.14) and *gr* in RS 24.266:26 (*KTU* 1.119), everywhere meaning ‘attack’, may only be from either GRY or GRR without mentioning that Hebrew shows a hollow root with a similar meaning. This is not of purely lexicographic interest for the reason that T. mentions the orthography without {y} as a factor in remarks having to do with phonetics and morphology. Whatever the root may be, it is not implausible that the two tokens in RS 2.[003]⁺ are to be analyzed, respectively, as imperative and ‘narrative’ infinitive (see preceding remark).

— p. 224 (§41.221.62b), p. 502 (§73.623), p. 635 (§75.512). It is simply out of the question that the form {ytnnn} in RS 16.265:9 (*KTU* 5.9) should represent a perfective form with pronominal suffix, as T. takes it without even indicating the alternative analysis as an imperfective. Lines 7-10 read {īršt . āršt (8) l āhy . l r‘y (9) w ytnnn (10) l āhh . l r‘h}, which may only be translated ‘A request I would make of my brother, my friend, and may he grant it to his brother, his friend, his friend forever’.⁴⁹⁰ The presence of the pronominal suffix written {-nn} after the verbal stem shows that the latter is either volitive /YQTLa/ or indicative /YQTLu/ (expressing a weakly marked volitive). The analysis as a jussive

broken off too close to the right end to determine whether it was originally {t} or {n}. The space in the break is, however, insufficient to consider the restoration of three and a half signs ({w yt]n‘nn}), so one must either abandon the idea that the suffix here was written {-nn} or else be willing to reconstruct the phrase without the conjunction at the beginning (see Hawley and Pardee “Le texte juridique RS 16.382: nouvelle étude épigraphique,” forthcoming in *Semitica* 52).

⁴⁹⁰Pardee and Whiting, *BSOAS* 50 (1987) 11-12, where the analysis of {āršt}, literally ‘I made’ or ‘have made’, as an “epistolary perfect” is presented; Pardee, *Context* III (2002) 115.

/YQTLØ/ appears unlikely because the presence of three {n}s indicates that there was a vowel between the verbal stem and the energetic/pronominal element.

— p. 225 (§41.222.3), pp. 226-27 (§41.222.5b). Only in a remark appended to §41.222.5b does T. indicate his reasons for vocalizing the 2 f.pl. pronominal suffix as “/-*kun(n)ā*” and the 3 f.pl. as “/-*hun(n)ā*.” This is because “in den Sem. Sprachen” the masculine/feminine distinction should be made either by the vowel (/u/ vs. /i/) or by the consonant (/m/ vs. /n/). Without direct evidence, however, it is impossible to know whether Ugaritic followed this pattern or not.

— p. 226 (§41.222.4a), p. 488 (§73.523c). Though RS 19.066 (*KTU* 3.8) is poorly preserved—and I have not yet had the occasion to collate the tablet—, as presently available editions present the text, the partially preserved suffix {h[m]} in line 9 should be dual, not plural as T. presents it, for lines 6-8 read {‘rb b n[...] | w . b . p[–1 | āpš[ny]}, (‘PNN) guarantee N[...] and P[–1 the ‘APŠN as regards their exiting ...’ ({āpšny} is more plausibly a gentilic than a third personal name). Moreover, the correct analysis of *hbth hwt tth* in RS 15.128 (*KTU* 3.3) indicates that YŠ’ in this text may not express flight, as T. translates the verb. The word after *hwt* in RS 19.066 has disappeared, and, though usually restored as {[tth]} on the basis of RS 15.128, there is simply no way of knowing what it was. The assumption is usually made that *ḪBT* and YŠ’ are synonyms in these two texts, and that *hwt tth* was originally present in both texts, but these are only hypotheses based on the supposition that both texts deal with the same legal matter, that of the flight of an indentured servant to another land, a supposition that can no longer go unchallenged. It is equally possible, given the damaged state of RS 19.066, that the purpose of that text was to guarantee that the two persons in question would indeed exit the land of Ugarit for the purpose of serving in another land (the phrase in question is ‘RB ... *b yšihm hwt* [...], ‘X guarantees Y and Z regarding their exiting (to) a/the land (of) [...].’). That the highest echelons of Ugaritic society had international connections and business dealings is now well known; the requirement that a debt slave do his service in one of the extra-territorial enterprises of such a personage or that his service in another country be exchanged for some commodity are possibilities to be considered.

— p. 228 (§41.32). In his examples of enclitic {-m} attached to pronouns, independent or suffixal, T. cites none from prose. Two clear examples are now attested in unpublished texts: in RS 94.2284:26 *ātm*, ‘you’ and in RS 96.2039:21 *‘mkm* are both addressed to a single person.

— pp. 229-34 (§42). This section is entitled “Das Demonstrativpronomen.” Because these particles appear as pronouns or as adjectives (i.e., either in independent usage or to modify a noun) and some have both functions, it would have been preferable to indicate both usages in the title and to include in the organization of the section the distinction between the two (the organization here is strictly morphological, with usage indicated under each form).

— p. 229 (§42.0), pp. 232-34 (§42.7), pp. 749-50 (§81.4a). The basic form of the presentative particle {hn} should not be represented as “**hann(V)*,” but as /han/, which is usually expanded by the enclitic particle /na/—at least the latter process of accretion is what produced the common form written {hn} (on the expanding particle {-n-}, see below remark to pp. 823-25 [§89.1]). The short form of the particle is attested by the spelling {hbt}, ‘the

house’, in RS 29.093:16 (*KTU* 2.70), which consists either of /han/ + /bêta/⁴⁹¹ or of a similar basic particle (/ha/, /ha’/, or /hal/) plus the noun. I prefer to see here /han/ because that is the deictic particle best attested in Ugaritic (/ha’/ is popular in Aramaic, while Ugaritic /hal/ takes on a local nuance, at least in prose—see above, remark to p. 197 [§33.322.42a], etc.). Strangely enough, T. has no trouble admitting the existence of /han/ in proto-Hebrew and proto-Arabic (p. 229 [§42.11]),⁴⁹² while denying it for Ugaritic. A clear case of the same particle prefixed to another (set of) particle(s) is found in RS 29.095:14 (*KTU* 2.71): {(13) ht ât . - (14) dbr . hmh[km] (15) b lk âl tšm}, ‘You, for your part, don’t worry about a thing’, lit. ‘don’t put any matter at all in your heart’. As has been remarked above (*ad* p. 63 [§21.355.1a]), emending out the first {h} of {hmk[km]} is arbitrary. On the other hand, excluding {hn} from the list of demonstrative pronouns and identifying it as a “Präsentationspartikel” (p. 229) is correct, for not a single example of {hn} used as a demonstrative pronoun may be substantiated.⁴⁹³ I would, however, demur to accept at face value T.’s assertion that {hn} in certain passages has a “demonstrativ-identifizierende Funktion” (p. 233) that stands somewhere between that of a pure deictic and a definite article. In the cases of RS 1.002:17’, 25’, 34’, 43’ {hn š} and {hn ‘r}/{hn .‘r} (*KTU* 1.40) or of RS 24.277:26’ {bt hn bnš yqh ‘z}, there is no difficulty in taking {hn} as the presentative particle.⁴⁹⁴ In RS 16.402:31 (*KTU* 2.33), *hn* may be an adverb, ‘here’, as in Arabic;⁴⁹⁵ in

⁴⁹¹Pardee, *AAAS* 29-30 (1979-80) 28; cf. Dijkstra, *HUS* (1999) 159. Speaking of this form as properly articular appears to me now, however, to be improper; it represents the pre-articular deictic stage (see continuation of this remark). T.’s rejection of the analysis of {h-} here as deictic because no ‘house’ has been mentioned up to this point in the letter is frivolous, as is demonstrated by hundreds, if not thousands, of instances of the Biblical Hebrew definite article attached to the first mention of a noun in a literary unit.

⁴⁹²There is no particular reason of which I am aware to see the development of the Arabic demonstrative pronoun /‘alladī/ as having the identical proto-forms as in Hebrew, viz., as developing from /han/ + /la/, when the definite article in Arabic is clearly /‘al/. It is absolutely necessary in considering the various West-Semitic particles to admit that different forms of the various deictic elements were preferred in one language, others in another. Arabic preferred /‘al/, Hebrew /han/, Ugaritic used both /hal/ and /han/, usually expanded with other particular elements.

⁴⁹³As T. points out (p. 232 [§42.6]), the authors of *CAT* have abandoned the reading {hn . kt} that they had proposed in *KTU* 2.16 (RS 15.174:10) and on which Cunchillos based his analysis of *hn* as a demonstrative pronoun (*AuOr* 1 [1983] 155-65; *Syria* 62 [1985] 207, n. 7). The correct reading, already present in the *editio princeps* (Virolleaud, *PRU* II [1956] text 16) and in my transcription of this text made available to the authors of *CAT* and to T., is indicated in my *Les textes rituels* (2000) 126 n. 146 and will be properly documented in my edition of the Ugaritic letters (in preparation).

⁴⁹⁴I translated {hn} in RS 1.002 by “here is” in *Leslau* (1991) 1187 and in *Ritual and Cult* (2002) 81-83 because of the problems with English ‘behold’; but French ‘voici’ works fine in both texts (see *Les textes rituels* [2000] 97-98). Though T. does not like this interpretation, he provided no good reason for his dislike. The text is not a “Beschwörung” as T. would have it, but a prescriptive sacrificial ritual characterized throughout by direct speech. On the function of *hw* in these passages, see above, remark to pp. 211-13 (§41.13), etc.

⁴⁹⁵This was the solution preferred by T. in *UF* 26 (1994) 476, abandoned here (p. 738 [§81.11a], cf. p. 233 [§42.73]) then preferred again (p. 833 [§89.31], with a question mark but without cross-reference or indication of the preferred analysis; in *JSS* 46 (2001) 24, T. presents the analysis as quasi-articular without even mentioning the possible analysis as an adverb.

lines 37-38, *hn ʾlpm ššwm* may well begin a sentence rather than ending one, as Tropper would have it, ignoring line 39.⁴⁹⁶ It is, in any case, certainly neither a demonstrative pronoun nor a definite article in these texts. But what, one may ask, is the function of a presentative particle other than “demonstrativ-identifizierende”? *h(n)* functions in Ugaritic either as a demonstrative pronoun/adjective (for which there is no real evidence), a presentative particle (i.e., a demonstrative adverb), or else it has developed into a true definite article. The real question is whether that particle has in Ugaritic come to be used frequently enough before nouns in any syntactic function to be termed articular. Such is certainly not the case in standard Ugaritic. The thrice-repeated formula {*hn bnš hw*} in RS 96.2039 (see above, remarks to p. 137 [§33.112.31], etc., and to pp. 211-13 [§41.31]) makes one wonder, however, if the queen who dictated that letter did not speak a language/dialect where such a usage was already well developed (twice in that text the sequence *w ht hn bnš hw* is attested, i.e., with two presentative particles before the noun and followed by *hw* functioning as a demonstrative adjective). It should be remarked in this respect that the use of the deictic particle as a definite article did not develop nearly so far, in terms of regularity of usage, in Phoenician as it did in Biblical Hebrew and Arabic (a similar development occurred in Aramaic, with its post-positive particle, but the specific articular function degenerated fairly rapidly there), and one could easily imagine Ugaritic developing along the lines of Phoenician in a relatively short time with either /*han*/ or /*hanna*/, or, for that matter, the most basic particle /*ha*/, at the origin of the development. What can be said with some degree of certainty, however, is that neither /*han*/ nor /*hanna*/ had in Ugaritic lost its presentative function to slip towards that of a definite article—there are simply too few cases to support such a claim.

— p. 230 (§42.3), p. 738 (§81.11d), p. 787 (§83.114d). T. is, in my estimation, misguided in taking *hndt* as a local adverb meaning ‘here’. The structural parallels with *hnd*, the primary demonstrative adjective, and *hnk(t)*, which is attested, though rarely, as a demonstrative pronoun, are too close to be ignored. In both attestations of *hndt* where the context is relatively clear, the morphology of the element *dt* may be compared with that of the relative pronoun (see second following remark), while the function as a demonstrative pronoun is neither that of *d/dt* nor of *hnd* (the latter is presently attested only as an adjective). In RS 18.031:12 (*KTU* 2.38), *hndt* functions as a demonstrative pronoun of which the antecedent is the feminine noun *ʾny*. The passage reads {(10) ʾnykn . dt (11) likt . mšrm (12) hndt . b . šr (13) mtt}, ‘The group of ships that you sent to Egypt, that (group) was wrecked off Tyre’. In RS 3.322⁺ iv 62 (*KTU* 1.19), the usage appears to be pronominal and adverbial (i.e., it is not an adverb, but a pronoun used adverbially, ‘at this [place]’). It should be stressed, nevertheless, that the *-t* is probably not the feminine morpheme,⁴⁹⁷ but the enclitic particle

⁴⁹⁶ Whatever the syntactic division of lines 37-39 may have been in the original text, T.’s interpretation of the problematic word *th* in RS 16.402:37 as providing a local indicator may not be considered an adequate basis for rejecting the analysis of *hn* as an adverb, for the meaning of *th* is still unknown and it is far from certain that the {*-h*} is the adverbial morpheme.

⁴⁹⁷ As I once identified it: “Ugaritic,” in *The Semitic Languages* (ed. R. Hetzron; London: Routledge, 1997) 131-44, esp. p. 136.

that appears on alternative forms of both the feminine and the plural demonstratives. If such be the case, *hndt* will be marked for gender, case, and number in the /d-/ element only (on d/dt, see second following remark).

— p. 230 (§42.4), p. 786 (§83.113i), p. 840 (§91.12e). Against the analysis of {hnhmt} in RS 15.128:8 [*KTU* 3.3]) as a demonstrative pronoun/adjective stands the form *hnmt* in RS 94.2965:20 (*b šdm hnmt*, ‘for these fields’). Given this form, it is perhaps better to take {hnhmt} as consisting formally of the presentative particle plus the 3 m.pl. independent pronoun, oblique form. The passage *w mnm šalm dt tknn ‘l ‘rbnm hn hmt tknn* (lines 5-9) would thus be translated literally: ‘And whatever investigators (see above, remark to p. 178 [§33.231.1b], etc.) should appear, against the guarantors, even these, they should appear’ (/wa mannama ša’alūma dūti takūnūna ‘alê ‘urubānīma hanna humati takūnūna/). Or, in line with T.’s analysis of *šalm* as non-personal: ‘And whatever claims may arise, against the guarantors, even these, they should arise’ (/wa mannama ša’alūma dūti takūnūna ‘alê ‘urubānīma hanna humati takūnūna/). If *hnmt* in RS 94.2965:20 is a demonstrative pronoun/adjective in the narrow sense of the word, one will note that it is not built from the well-known stem *hnd-*, but consists of *hn* + enclitic *-m* + enclitic *-t*.

— p. 231 (§42.5), p. 597 (§74.624), p. 703 (§76.521.2), p. 740 (§81.12e), p. 787 (§83.114a), p. 864 (§93.342). Equally misguided is the explanation of *hnk* as meaning ‘there’. One of the passages cited is too damaged to permit a decision (RS 16.402:11 [*KTU* 2.33]), but, in the other two texts cited, taking *hnk* as a demonstrative pronoun makes more sense of the text than does the analysis as an adverb. In RS 16.402:22-24, the interpretation of *škn hnk* as ‘ordered there’ (p. 231) or ‘prepared there’ (pp. 597, 740, 864) makes no sense: the king has ordered the recipient of the letter to supply horses and the recipient is complaining about this treatment: *w mlk b‘ly ht lm škn hnk ‘l ‘bdh ālpm ššwm*, ‘(As for) the king, my master, now why has he assigned this (responsibility) to his servant: (viz., that of furnishing) 2000 horses?’⁴⁹⁸ In RS 29.095:9 (*KTU* 2.71), *hnk tšm‘m* is better interpreted as meaning ‘hear this’⁴⁹⁹ than as ‘listen there’ (where else would they listen?).⁵⁰⁰

— pp. 234-38 (§43). T. proposes a two-tiered description of the relative/determinative pronoun: (1) a “Deklinable Variante,” wherein the particle is marked for person, gender, number, and case, and (2) an “Indeklinable Variante,” unmarked for any of these grammatical features. Because this assumes the coexistence of a system and an absence of system, one wonders if it might not be preferable to propose the existence of a single system. The basic data are these: (1) the syllabically written form {du-ú} (RS 20.123+ ii

⁴⁹⁸Cf. Pardee, *AfO* 31 (1984) 216; idem, *Context* III (2002) 106. For the reading of {lhtl} at the end of line 22, see *Les documents épistolaires* (in preparation). The interpretation as a demonstrative pronoun is preferred by T. himself everywhere but p. 740.

⁴⁹⁹Cf. Pardee, *Context* III (2002) 111: “Now listen well.” T. does not cite this text on p. 231 as containing a possible example of a demonstrative pronoun, reserving his preference for the analysis as an adverb (p. 740).

⁵⁰⁰T. takes *tšm‘m* as a noun, which makes his interpretation of *hnk* plausible in the context thereby created, but that analysis is not to be preferred (see below, remark to p. 270 [§51.45w], etc.).

29⁵⁰¹) shows that the masculine singular nominative form was /du/ or /dū/, probably the latter (there is no reason to see here the use of {ú} as a disambiguator for the first sign, as is the case with the prepositions {le-e} and {bi-i}⁵⁰²) and that the masculine singular was probably declined according to the three principal cases, for, if only a single form was in use, one would not expect the nominative to have been preserved;⁵⁰³ (2) both *d* and *dt* are attested modifying feminine singular and masculine and feminine plural nouns; (3) *dt* is never attested modifying a masculine singular noun⁵⁰⁴. A simple explanation, but one that results in a rather high incidence of identical forms, would say that the original forms /dāti/ (feminine singular genitive) and /dūti/ (plural oblique)⁵⁰⁵ were confused with /dā/ and /dū/ + the enclitic particle /-ti/; the other forms in the paradigm would as a result of this re-analysis have been dropped while perception of /-ti/ as the particle would have permitted that element to be dropped or retained as a matter of style. The paradigm would then be:

| Masculine singular | Feminine singular | Plural |
|--------------------|-------------------|----------|
| /dū/, /dā/, /dī/ | /dā(ti)/ | /dū(ti)/ |

Another possible set of developments leading to a single paradigm, one with a lower incidence of identical forms, would be this: (1) in proto-Ugaritic, as in Akkadian,⁵⁰⁶ the feminine and plural pronouns⁵⁰⁷ (cf. Akkadian /šāt/ and /šūt/) had lost their case vowels; (2) the loss of the case vowels allowed a variant form without /-t/ to develop (these variants might have arisen in speech through various assimilations of the /-t/ to the first consonant of the following word); (3) the vowel of the secondary forms would have been short (in proto-Northwest Semitic all theoretically long vowels in closed syllables become short, hence /dātu/ → /dāt/ → /dat/ and /dūtu/ → /dūt/ → /dut/); (4) the forms with /-t/ were homogenized by paradigm pressure to forms with the enclitic particle /-ti/⁵⁰⁸ (this last step appears necessary

⁵⁰¹Nougayrol, *Ugaritica* V (1968) text 137.

⁵⁰²See above, remark to p. 52 (§21.341.21c), etc.

⁵⁰³Loeenstamm once claimed that the relative particle was indeclinable in old Semitic (*Lešonenu* 23 [1958-59] 82), but the declinable forms in Old Akkadian (*GAG*, §46) and the difference of vowel attested in later Akkadian, Hebrew, and Aramaic, indicate, to the contrary, that old Semitic had a system declined at least partially for case, gender, and number. The different vowels in later Akkadian (*ša*), Ugaritic (*dū*), Hebrew (*še^h*) and Aramaic (*dī* and *d^o*) are most plausibly to be explained as remnants of a more complete system. As is well known, classical Arabic had a system based on /dū/ that was marked for gender, number, and case (Wright, *Grammar*³ I [1896] 272-73, §347).

⁵⁰⁴I know of no exceptions to this view espoused by T. Loewenstamm once proposed (*Tarbiz* 23 [1958-59] 80; cf. *Comparative Studies* [1980] 69) that *šd ūbdy ilštm^c dt ...* (RS 12.006:1-2 [*KTU* 4.110]) was a singular formulation, but T. more plausibly sees here a triple construct chain with a plural as the head element: “Lehensfelder von ON, die...” (p. 843 [§91.312]).

⁵⁰⁵These are the forms posited by T. in his paradigm of “Deklinable Variante”; according to the development suggested here, the full paradigm would have been present in proto-Ugaritic but no longer in the language as we know it.

⁵⁰⁶This and other Akkadian data evoked below are described in *GAG*, §46.

⁵⁰⁷Here and henceforth I use “plural” for “masculine and feminine plural.” I do not include the dual because no such forms are as of yet certainly attested in Ugaritic (T., p. 236 [§43.133]).

⁵⁰⁸On this particle, see T., p. 836 (§89.5).

to account for a sequence such as {dt tgrk} in RS 16.078+:22 [KTU 2.23], where /dut/ + /taǵgurrūka/ might easily have become one word, written with a single {t}). These hypotheses permit a single paradigm, according to which only the masculine singular was declined for case, but the feminine and plural forms were distinguished both from each other (by vowel quality) and from the masculine singular (by vowel quantity) and had stylistic variants with /-ti/:

| Masculine singular | Feminine singular | Plural |
|--------------------|-------------------|----------|
| /dū/, /dā/, /dī/ | /da(ti)/ | /du(ti)/ |

Though a system where the singular is characterized by long vowels, the plural by short vowels, appears to go against the grain of early Semitic, where length is often associated with plurality,⁵⁰⁹ a similar development may perhaps be posited for Aramaic, where the corresponding particle appears both as /dī/ and as /dʰ/: if the form with *shewa* did not arise through mutation (analogy to the monoconsonantal prepositions?), a proto-Aramaic form with a short vowel must be posited. Nonetheless, because of the unexpected distribution of short and long vowels in this second hypothetical system, the first must be preferred.

As for the usage of the forms with and without /-ti/, T. asserts (p. 238 [§43.3]) that the expected “deklinable Variante” was regularly used before a verbal relative clause, the “indeklinable Variante” more commonly before a nominal phrase (“kann ... stehen” is the formulation in the latter case). One would not be able to draw so firm a conclusion from the examples cited on the preceding pages, certainly not for prose, and one wonders if T. drew up for himself statistics that were not included in the grammar (T. refers to the higher incidence of {d} in prose, but provides no statistics). This being the case, one must remain dubious about proposals to emend {d} to {dt}, especially in prose (see above, remark to p. 60 [§21.354.1a], etc., and various remarks here below).

— p. 235 (§43.12), p. 899 (§97.112). In the first section cited, *bnšm dt l mlk* (RS 18.026:17 [KTU 4.339]) is interpreted, on the pattern of line 1 where the verb *tb* is present, as referring to personnel returning to the town *Mulukku*, in the second as referring to personnel belonging to the king. Neither entry is cross-referenced to the other.

— p. 235 (§43.12), p. 262 (§51.43k), p. 637 (§75.514), p. 704 (§76.521.3). Rather than taking the persons qualified as ‘*rk bʿl ḥlb dt l ytn šmn* (RS 24.292 [KTU 4.728]) as ‘offering-preparers who have not given oil’, it appears more plausible to see ‘*rk* as designating, as in Hebrew, a type of tax, which here would have consisted of oil. If that be the case, ‘*rk* would be a /qitl/ or /qatl/-base noun, rather than a /qātil/-base verbal substantive and the *l* would be ‘emphatic’, rather than negative: /(1) ‘arakū baʿli (2) ḥalbi dūti la yutanū (3) šamnu/ “‘RK-taxes for *Baʿlu* of Aleppo that were properly paid in oil by...” (+ list of PNN).⁵¹⁰

⁵⁰⁹Gelb, *Sequential Reconstruction* (1969) 48-55.

⁵¹⁰Pardee, *Ritual and Cult* (2002) 217; cf. idem., *Les textes rituels* (2000) 749-50.

— p. 236 (§43.2a). In the context of *’Ilū’s* furnishings, the interpretation of *n’l* in RS 2.[008]+ i 36' (*KTU* 1.4) as ‘bed’ or ‘(sleeping) platform’⁵¹¹ is more plausible than the translation as ‘sandals’.

— p. 237 (§43.2b), p. 256 (§51.41e), p. 298 (§53.331.2). Since indubitable examples exist of the relative pronoun in the form *d* modifying feminine antecedents (p. 237), the phrase *lḥt spr d likt*, ‘the letter-tablet that I sent’, in RS [Varia 4] (*KTU* 2.14) provides very shaky grounds on which to base the conclusion that the Ugaritic noun *lḥt* was of masculine gender (pp. 256, 298). Hebrew *lū^aḥ* is, of course, masculine, but then it does not in the singular bear the morpheme {-t} that so often denotes feminine gender. T.’s proposal that Ugaritic would have had a singular noun *lḥ*, ‘tablet’, of which {*lḥt*} is the plural requires that the frequent reference in letters to {*lḥt*}, ‘(letter-)tablet(s)’, as a form of reference to previous correspondence, always refers to a plurality of tablets (not even a duality!⁵¹²) as bearing epistolary documents.⁵¹³ Since most epistolary texts, in Ugaritic as well as in Akkadian, were demonstrably complete on a single tablet, the hypothesis that the singular of the noun for ‘tablet’ was in Ugaritic *lḥt* must be preferred.⁵¹⁴

— p. 237 (§43.2d). Contrary to T.’s explicit assertion, there is a horizontal line between lines 14 and 15 of RS 10.090 (*KTU* 4.53 — against previous editors, the authors of *KTU* indicated the correct reading). Moreover, the {*d*} indicated by T. at the beginning of line 16 was probably preceded by another sign, today destroyed, and the identification of the {*d*} as the relative pronoun is, therefore, dubious.

— p. 239 (§44.12), pp. 240-41 (§44.241). T.’s dogged determination to present every possible explanation reaches new heights in these two paragraphs, where the interrogative particle {*mn*} is explained in the first as personal (‘who?’) in the second as impersonal (‘what’). The first is introduced by “vielleicht,” the second by “wahrscheinlich”; virtually the same texts are quoted and translated in both. In my estimation, the evidence favors the latter interpretation and it is possible that {*mh*} and {*mn*} exist side-by-side in Ugaritic as impersonal interrogative pronouns because the latter is an expansion of the former (/mah/ + /na/ → /manna/)⁵¹⁵. T. is obviously tempted by the analysis of *mn* as personal by the comparative Semitics situation, where *man(nu)* is personal (p. 238 [§44.11]). What is important to note in this respect, is that the Ugaritic and Canaanite situation does not show just the opposite vowel for the personal and impersonal interrogative pronouns as compared with other Semitic languages, but fundamentally different forms: a basic stem /mī/ for the

⁵¹¹Dietrich and Loretz, *Texte aus der Umwelt des Alten Testaments*. Band III, Lieferung 6: *Mythen und Epen* IV (1997) 1153; idem, *UF* 32 (2000) 208, 212-13; Pardee, *Context* I (1997) 256; Wyatt, *Religious Texts* (1998) 92 (“palanquin”).

⁵¹²If the singular were {*lḥ*}, the dual would have been written {*lḥm*} (Vita, *OLP* 28 [1997] 33-41).

⁵¹³For del Olmo Lete and Sanmartín, *Diccionario* I (1996) 243, {*lḥt*} would be a *plurale tantum*, but, since ‘tablets’ do not form a natural collectivity, that solution does not appear likely.

⁵¹⁴Pardee, *Les textes épistolaires* (in preparation), commentary on RS 16.394:47'. The proposal that *lḥt* is a feminine singular noun goes back at least to Virolleaud, *GLECS* 8 (1957-60) 91.

⁵¹⁵T. refers to the particle in his second interpretation as “eine erweiterte Variante von *mh*” without proposing a specific derivation.

personals and a stem /mah(a)/ for the impersonals, i.e., with consonantal /h/.⁵¹⁶ Whatever the case may be for comparison with the other Semitic languages, the two possible interpretations of Ugaritic *mn(m)* could have been presented without creating two largely redundant paragraphs.

— p. 240-41 (§44.241-242), p. 313 (§54.133.2e), p. 754 (§81.65), p. 803 (§83.24f), p. 903 (§97.31). T. creates a special paragraph (§44.242) for the “Bedeutung ‘wieviel?’” for *mn* which would only be attested in *mn yrḥ k m[rš] // mn k dw kr[t]* in RS 3.325⁺ ii 19-20 (*KTU* 1.16). It appears more likely, however, that the meaning of the Ugaritic is not ‘how many months?’ but ‘what month’, viz., ‘(In) what month did he become ill, (in) which one did *Kirta* become ill?’. Without creating any syntactic difficulty, this removes the problems of *mn* (1) having the particular meaning of ‘how many?’ and (2) being followed by a singular noun (a problem that T. attempts to resolve by comparing Arabic *kam*). Logically, the use of ‘month’ may be inferred to mean that he has been sick less than a year and naming the month in which he became ill tells the questioner how long the illness has lasted. There is no objection to translating the passage with ‘how many’ in English or with the equivalent in German, but that translation should not be the determining factor in deciding the grammatical analysis of the particle. T. rarely lapses into ‘translation grammar’ (i.e., into creating Ugaritic categories on the basis of German translation), but this may be one.

— pp. 242-44 (§45.1). Unless the Akkadian equivalent of Ugaritic *mnm*, ‘whatever,’ be taken as deciding the vocalization of the Ugaritic (as I and others have done in the past), no reason appears in this section for not vocalizing the personal interrogative pronouns spelled {mn} with a stem /mī-/ , the impersonals with /ma(h)/ (/h/ in parentheses because it would ultimately be secondary and, in any case, it assimilates in *mnm*). For the personals, T. prefers “/mannV/” apparently because that is the form expected from comparative Semitics (cf. p. 238 [§44.11]). The impersonal {mnm} is vocalized “/mā/īnummV/od. /mā/īnammV/” (p. 243) in spite of the fact that the form *mhk*, ‘whatever’, with {h}, leaves no doubt as to its origins. The Akkadian form that regularly corresponds to Ugaritic *mnm* in the epistolary ‘return-of-news formula’ is *mīnummē*⁵¹⁷ but, because the lexicon of the Akkadian of Ugarit is not necessarily influenced by Ugaritic, nor *vice versa*, the Ugaritic form may reflect the expected Ugaritic kernel /mah-/. If such be the case, the Ugaritic indefinite pronouns would

⁵¹⁶In *UF* 33 (2001) 727, T. cites two data in favor of “mah(V)” being the basic “Sachklasse” form: Ugaritic *mh* and Arabic *mahmā* ← “*mahmah.” To these should be added Hebrew *ma(h)* + gemination of the first consonant of the following word; the gemination can only be explained by the basic form being /mah/ with assimilation of the /h/ producing the gemination while the archaic form was retained in the orthography. The retention of the {h} in the orthography shows that the original form may well have been /maha/ and that the final vowel would have blocked assimilation until it disappeared when final short vowels either dropped or lengthened in proto-Hebrew. This in contrast with the definite article, of which the second consonantal element has consistently assimilated in both Hebrew and Phoenician. (Ugaritic shows rare instances of both /han-/ and /hanna/ in proto-articular function: see above, remarks to p. 137 [§33.112.31], etc., to pp. 211-13 [§41.31], and to p. 229 [§42.0], etc.).

⁵¹⁷On the Akkadian form, see Huehnergard, *Ugaritic Vocabulary* (1987) 138-39.

have been: personals {mnk(m)} /mīnaka(ma)/ and, perhaps, {mnn} /mīnana/;⁵¹⁸ impersonals {mhk(m)} /mahaka(ma)/ and {mnm} /mannama/ ← /mahnama/.⁵¹⁹

— p. 243 (§45.122a), p. 820 (§88.1c), p. 903 (§97.33). As with other parts of RS 29.093 (*KTU* 2.70), T. is very uncertain about the interpretation of *mnm iṭ l* ‘*bdk* (l. 29), whether to take it as an independent clause (“Was auch immer ist, [das teile] deinem Diener [mit]” [pp. 820, 902]) or as dependent on what precedes (“Meinem Herrn gehört, was auch immer deinem Deiner gehört” [p. 903]); on p. 243 and in the first translation on p. 820, these words are translated as a simple phrase (“was es auch immer gibt” and “was auch immer deinem Diener gehört,” respectively). I have proposed that *mnm iṭ* may be taken as an unmarked adverbial dependent on the previous phrase: “... (your servant) will be sure to have a *ḥipānu*-garment made for my master, of whatever (is required) from your servant’s own goods.”⁵²⁰

— p. 244 (§45.122b), p. 904 (§97.42a), p. 904 (§97.42). T.’s interpretation of the epistolary formula *mnm šlm*, lit., ‘whatever well-being’, as an indefinite pronoun in a subordinate clause is correct in my estimation, but that overall interpretation has taken a long time to come to the fore.⁵²¹ On the other hand, his interpretation of the formula as ‘following’ the corresponding Akkadian formula must be nuanced: because the Akkadian formula is known only in the West, it may have been borrowed from a West-Semitic language; since it is attested only within the Hittite sphere of influence, it may even have been borrowed ultimately from Hittite—though that is speculation for the moment.⁵²² The latter solution is also appealing because the syntax of this formula (which basically goes something like *mnm šlm* ‘*m* X *rgm* TTB ‘*my*, ‘whatever well-being there may be with X, may X send back word of that to me’) is not attested in precisely this form elsewhere in Ugaritic. In the case of a Hittite origin, the Ugaritic formula would probably indeed have passed through an Akkadian intermediary stage, for there is not a good deal of evidence for direct Hittite influence on the Ugaritic language.

— p. 244 (§45.122c), p. 821 (§88.21d), p. 822 (§88.22a). The ‘new reading’ (identified as such on p. 821 only) of RS 15.111:18 (*KTU* 3.2) as “[w ṽ]nṭ in[n] bh” will not hold up: the sign after {in} is clearly {m} (tablet collated in June of 2004). Moreover, as T. points out on p. 822, the new reading is not new but old: it rejects that of *KTU/CAT* in favor of the

⁵¹⁸This form would appear only in the address of the school letter RS 16.265:2 (*KTU* 5.9), where {mnn} may be a proper name. I leave entirely out of consideration {mrnmnn} in RS 24.271:22' (*KTU* 1.123) because, despite T.’s conviction that this divine name is “offenbar” to be analyzed as Akkadian *māru*, ‘son’, plus “*mamman* ← **man-man*” [!], ‘whoever’ (cf. p. 275 [§51.5a]), I remain dubious towards that explanation (see my commentary in *Les textes rituels* [2000] 703-4).

⁵¹⁹See now Bordreuil and Pardee, *Manuel* (2004) I 56, II 179. On the vocalization of the expanding element {-n}, see remark below to pp. 823-25 (§89.1).

⁵²⁰*Context* III (2002) 111. On the interpretation of the preceding lines, see below, three remarks to p. 439 (§73.223.41.8), etc.

⁵²¹For a study of the formula with previous bibliography, see Pardee, *Fronzaroli* (2003) 446-75.

⁵²²*Ibid.*, p. 466, n. 66.

editor's original reading.⁵²³ The *KTU/CAT* reading was, however, essentially correct, for on the tablet one finds {[w ù]ṛnṭl . in . mṛnmṭl . bh}, 'There is no *unt*-duty at all (attached) to it'.

— p. 244 (§45.13), cf. p. 241 (§44.31). Because an invitation to a feast that is phrased 'Eat any food' (T.: "Eßt von jeglicher Speise") does not appear particularly inviting, *áy* in RS 2.002:6 (*KTU* 1.23) may be taken as an interjection rather than as an indefinite pronoun (the option is not mentioned here and is not presented anywhere in the grammar).⁵²⁴

— pp. 244-46 (§45.2). I judge it a mistake to have classified "Das Substantiv *kl* und Derivate" among the indefinite pronouns (§45 "Das Indefinitpronomen"). I can think of no reason to have done so other than the semantics of the noun, viz., 'all' is translatable as 'every', 'everyone', 'everything', etc. As regards morphology and syntax, however, there is no reason to doubt that *kl* behaves differently from any other noun. For example, I know of no reason to doubt that when it modifies another noun that noun is in the genitive case (put another way, *kl* is in the construct state), whereas pronouns and numeral nouns may be appositional to the modified noun. One peculiarity of *kl* in Biblical Hebrew illustrates the necessity of recognizing how semantics may affect usage: because of the semantic definiteness expressed by *kl*, noun phrases beginning with *kl* that function as definite direct objects may be introduced by the particle *ʾt*, marker of definite direct objects, whether or not the principal noun of the phrase be otherwise marked for definiteness. But in all other respects Hebrew *kōl/kol* shows the morphological and syntactic earmarks of a noun, not those of a pronoun.

— p. 245 (§45.21c-d), p. 845 (§91.314.1). In the first reference cited, T. cites three phrases in which *kl*, 'all', + pronominal suffix is supposed to be used in "absoluter Gebrauch." The first is the formula *ḥkpt il klh* (RS 2.[004] v 21', 31' [*KTU* 1.17]; RS 2.[014]+ vi 13-14⁵²⁵ [*KTU* 1.3], RS 3.361 iii 1 [entirely restored] [*KTU* 1.1]) which he interprets as "das ganze göttliche/weite Memphis"; the second is *ārš il klh* (RS 2.[009]+ i 65 [*KTU* 1.6]), translated "die ganze göttliche/weite Erde"; the third in RS 24.255:6-7 (*KTU* 1.111) *ʾšrm gdt klhn*, translated "(als Opfer) Vögel, (sie) alle zerstückelt/zerteilt." The usage is compared with Arabic *ʾal-yawmu kulluhū*, "der ganze Tag." For the first two phrases to be so interpreted, however, one must accept that *il* is here serving as an expression of the superlative (p. 845), which is not necessarily the case: each time, because the formula is used with respect to the dwelling of a particular deity, *il* may be interpreted as functioning normally as the common noun for 'god'.⁵²⁶ As regards RS 24.255:6-7, *klhn* is not simply appositional to *ʾšrm* in a single clause, it is the second subject of a complex sentence made up of two nominal sentences: lit., '(As) burnt-offerings: birds, cut up into small pieces (are) they all (to

⁵²³The editor (Virolleaud, *PRU* II [1957] 23 [text 9]) read the last part of the line as {in[n .] bh}; in *KTU/CAT*, one finds {in ṛmnṭl[m] . bh}.

⁵²⁴Cf. Pardee, *Context I* (1997) 276.

⁵²⁵The name for Memphis is here written {ḥkpt}.

⁵²⁶Pardee, *Context I* (1997) 244 (with explanatory note 19), 255, 269 (with explanatory note 250), 346.

be)'.⁵²⁷ This category of usage of *kl* may, therefore, not be attested in Ugaritic; moreover, several examples of the so-called superlative use of divine names and titles may also disappear from the list (on this question, see below, remark to pp. 844-45 [§91.314.1]).

— p. 245 (§45.22b), pp. 568-69 (§74.422). For the interpretation of *dbḥ kl kl ykly* in RS 24.277:7'-8' (*KTU* 1.127), T. considers several possibilities but misses the one that appears most likely to me, viz., linking this phrase with the previous ones, as is indicated by the conjunction *w* at the end of line 6'. The entire section of this text inscribed in discrete units on a clay model of a lung of a caprovid reads: (4') *dt nāt* (5') *w ytnt* (6') *trmn w* (7') *dbḥ kl* (8') *kl ykly* (9') *dbḥ k sprt*, which may be translated 'The ones (*sc.* sacrifices) of *nāt* (PN) and gifts of (= for) *trmn* (DN) and a sacrifice of everyone: everyone may participate in consuming (this) sacrifice, according to the writings'.⁵²⁸

— p. 246 (§45.23a), p. 856 (§92.238b). Though T.'s analysis of *kll* in the epistolary formula *kll šlm*, 'all is well', as an adverbial accusative is certainly plausible, I know of no syntactic reason why it should not be taken as the subject of the verb *šlm*. In this latter respect, T. holds that *šlm* is a substantive, basing his analysis on the same of *šulmu* in the corresponding Akkadian formula. The variety of word orders in Ugaritic indicates, however, that *šlm* is a verb (/šalima/, not /šulmu/ or /šalāmu/—for an explanation of the difference between the Akkadian and Ugaritic formulae, see below, note 1393). It is important to note the variety of word orders attested in the situation report of the writer as opposed to the virtually invariable phrase *mnm šlm* used in the request for return of news; the latter is substantival and functions as the point of reference of the verb in a cleft sentence ('whatever well-being, return word (of that) to me').

— pp. 247-77 (§51). It is traditional in grammars of the vocalized Semitic languages to present substantival stems in order of growing complexity from monoconsonantals through forms with a reduplicated element on to forms with prefixes and suffixes. T. follows this model here but, since there is no tradition of vocalization of Ugaritic, he has had to decide whether to indicate the breakdown according to vocalic patterns (/qal/qil/qul/, etc.) only for the relatively few forms for which internal evidence is available or to fill out each such category on the basis of comparative evidence. He has chosen the latter. Three remarks are in order: (1) his choice of vocalic pattern when the other Semitic languages offer more than one possibility is sometimes open to criticism (some of the following remarks express such criticisms); (2) sometimes the decisions appear arbitrary, as in the case of *nrt*, 'light', vocalized /nûrat-/ with no comparative evidence cited (p. 256), whereas the form *nr* was vocalized /nîr-/, following the Hebrew (p. 254); (3) it is incumbent upon well-informed instructors of Ugaritic to make clear to their students using this grammar how arbitrary the choices often are. We simply do not know, for example, whether Ugaritic *hlm*, 'dream', was

⁵²⁷Pardee, *Les textes rituels* (2000) 622, 626; idem, *Ritual and Cult* (2002) 92 (in neither case was the literal translation indicated). That the phrase *klt klhn* may be taken as constituting a clause separate from 'šrm becomes clear if one keeps in mind the standard offering formulae in these texts, one of which is the simple nominal sentence consisting of 'type of offering + content of offering'; in this case, *gdt klhn* is a supplementary syntactic unit in which a particular disposition of the offering is stipulated.

⁵²⁸Pardee, *Les textes rituels* (2000) 715, 719-20; idem, *Ritual and Cult* (2002) 130.

vocalized /ḥilm-/ , /ḥulm-/ , or /ḥulum-/ (or, for that matter, on yet another pattern—see remark below to p. 254 [§51.41c *ḥlm*]).

— pp. 247-77 (§51), pp. 278-80 (§52.2). T. never addresses explicitly the problem of how to vocalize the syllable bearing the feminine morpheme {-t} when derived from III-y/w roots (see specific remarks above to the phonology section, p. 183 [§33.242b], etc., pp. 183-84 [§33.243.12-15], p. 184 [§33.243.13], etc., p. 193 [§33.312.32b], pp. 199-200 [§33.323.3-4]). Unless the {y} is actually present in the script or unless a vocalization is attested in syllabic script, he in most cases assumes /-iyat-/ → /-îṭ/; once he assumes /-awat-/ → /-ât-/ (p. 267 [§51.45f] “*ma/iṭṭâṭ*” ← “**manṭawat*,” ‘bed’). This particular assumption does not explain why Hebrew has *miṭṭâḥ* (proto-Hebrew /miṭṭât/ would have become /miṭṭôt/ in Biblical Hebrew, cf. /’āḥôt/, ‘sister’) and the broader assumption that most Ugaritic forms had /-î-/ does not explain why so many Hebrew forms show simple /ā/, even those that ended historically in /-iyat-/, e.g., *yāpāḥ* ← /yapiyatu/, ‘beautiful’. The behavior of Ugaritic III-y/w nouns and adjectives appears to be closer to that of Hebrew than to the corresponding situations in Aramaic and Arabic, where the weak consonant tends to be retained in the feminine forms (contrast Hebrew /šʾmōnāḥ/, ‘eight’, and Ugaritic {tmnt} with Arabic /tamāniyat^{un}/ and Aramaic /tʾmānyāḥ/). That being the case, the reconstruction of the word for ‘bed’ as /miṭṭātu/ in Ugaritic cannot be considered likely and, in a grammar which attempts to reconstruct most everything and to place Ugaritic in the broader Semitic context as thoroughly as does this one, an explicit consideration of the problem was expected.

— pp. 248-49 (§51.2-3). In §51.3, T. considers it likely that *āb*, ‘father’, and *āḥ*, ‘brother’, were originally triconsonantal because they show a long case vowel (see further, next remark). It is unclear why he does not consider explicitly the possibility in §51.2 that *p*, ‘mouth’, and *š*, ‘sheep’, may have been originally biconsonantal since in Hebrew the stem of *peḥ* with pronominal suffixes resembles that of *’āḥ* and *’āb* while *šeh* shows similar characteristics. In any case, a statement regarding the origin of what was almost certainly a long case vowel in nouns that show a monoconsonantal form in Ugaritic would have been welcome.

— pp. 248-49 (§51.3a), p. 252 (§51.41a). In these two paragraphs, T. explicitly proposes that *āb* and *āḥ* were originally /qatl/-base nouns (on his hypothesis that these two words behaved as simple /qal/-base nouns in the absolute state, see remarks above to p. 51 [§21.341.21a], etc., and p. 175 [§33.215.22], etc.). Though it is indeed plausible, as was remarked above (see note 107), that these two nouns go back to III-w roots, the identification as /qatl/ types is less clear. In my note just cited, I cited the feminine forms /’āḥôt/, ‘sister’, and /ḥāmōt/, ‘mother-in-law’, as proof that the elements that produced the /ō/-vowel came before the feminine morpheme and the case vowel could not, for that reason, have been involved in the development of that vowel. Positing a base /qatal/ for these nouns would account reasonably well for the forms /’abû/ and /’aḥû/, for /’abawu/, with homorganic /w/ and /u/, could easily have contracted to /’abû/ already in proto-West Semitic, with analogical alignment of the vowel in the other cases to the standard paradigm (/’abû/, /’abâ/, /’abî/). The /ō/ of the Hebrew form /’aḥôt/ represents the Canaanite shift, since /’aḥāt-/ is attested in Late-Bronze proper names; the development of the feminine form in

proto-Northwest Semitic might, therefore, have been /ʔaḥawat-/ → /ʔaḥât-/. (The Arabic form of this noun, viz., /ʔaḥt^{un}/, when contrasted with Ugaritic /ʔaḥātu/, Hebrew /ʔāḥōt/, and Aramaic /ʔaḥāt/, illustrates clearly that this noun has undergone severe modification in the singular absolute in that language. It may also be taken as a strong indication that the Arabic masculine form /ʔaḥu/ may not be considered paradigmatic for the vocalization of the corresponding Ugaritic form — see discussions of *āb* and *āḥ* cross-referenced at the beginning of this paragraph.) Positing the /qatal/ base for these nouns might also provide a reasonable explanation for the difference in Ugaritic between *ʔabû/ʔaḥû* and *zurwu*, ‘resin’, for the latter could represent a true /qatl/ base from a III-*w* root (though proto-Hebrew shows *š^{or}rī^y*, as though from a III-*y* root). On the other hand, *āḥ*, ‘meadow’, seems to represent a proto-form /ʔaḥwu/ (cf. Hebr. *ʔāḥū^w*) which has apparently fallen together phonetically with the word for ‘brother’ (the Ugaritic form of this word should also have been /ʔaḥû/). Finally, I have referred here only to West Semitic, leaving East and South Semitic out of the discussion. It appears, however, that at least for *āḥ*, the triconsonantal root was retained in Ethiopic⁵²⁹, which would mean that the /w/ dropped independently in West and East Semitic and that each group of languages subsequently underwent its own development.

— p. 248 (§51.3a), p. 310 (§54.133.1b). *dm*, ‘blood’, is correctly classified as a /qal/ base in the first paragraph cited, but vocalized “/dāma/” in the second.

— p. 249 (§51.3b), p. 310 (§54.133.1b). In the first paragraph cited, T. unambivalently categorizes *bn*, ‘son’, with /qil/-base nouns, whereas in the second he vocalizes “/bi/una/.” One must surmise that the ambivalence here is owing to his view that Ugaritic *bnš* derives from *bn* + “**ʔunāš* ‘Mensch’ ” (p. 177), in spite of the fact that on p. 177, he indicates the base form of *bn* as “**bin*” (see remark to p. 177 [§33.215.42], etc.).

— p. 249 (§51.3b). It must be judged dubious that {dm[t]} in RS 16.394:50' (*KTU* 2.31:46) is to be equated with Akkadian *dimtu*, “Turm.” The state of preservation of this tablet is poor and its literary classification uncertain. That being the case, the inclusion of data derived solely from this text in a reference grammar must be judged ill advised.

— p. 250 (§51.3g), p. 702 (§76.521.1), p. 774 (§82.411). If {tm^{tt}} in the title *rb tm^{tt}* in RS 18.031:16, 22 (*KTU* 2.38) means “Mannschaft, Besatzung” what other etymology would it have than *mutu*, ‘man’? (T. presents this explanation only as “evtl.”) In any case, the more probable etymology, several scholars have believed, is from M(W)T, ‘to die’. This is because in line 13 the convoy of ships is said to have ‘died’ (*mtt*)⁵³⁰ in a bad storm; that fact admitted, the official in charge of salvage would have plausibly borne the title of ‘master of wreck, lit. of (ships) that have met “death” ’.⁵³¹

— p. 250 (§51.3g-m). Structurally speaking, I do not see why T. has chosen to present the word *tm^{tt}* just discussed in its own paragraph after the /qūl/ base with various dissyllabic

⁵²⁹D. Cohen, *Dictionnaire des racines sémitiques* 1 (Paris: Mouton, 1970) 15.

⁵³⁰T.’s acceptance of the emendation there to {<t>mtt} has already been criticized above, remark to p. 61 (§21.354.1c), etc.

⁵³¹Hoftijzer, *UF* 11 (1979) 386; Pardee, *Context* III (2002) 94.

substantives that include affirmative morphemes indicated immediately thereafter rather than discussing these forms in the sections below devoted to nouns bearing the morphemes in question (pp. 269-74 [§§51.45t-b' and §§51.46a-k]).

— p. 250 (§51.3h), p. 278 (§52.11). As pointed out above (remark to p. 88 [§31.3], etc.), T. glosses *ʾdn* only by “Herr,” in spite of the facts that (1) the polyglot vocabularies cited in each of these sections assigns that word the meaning of ‘father’ and (2) only the meaning ‘father’ is attested in prose.

— p. 250 (§51.3l). Since {mīyt} in RIH 78/20:8 (*CAT* 1.169) designates a place where the suffering one will eat and drink, it probably does not mean “Wassertiefe,” but a place characterized by water; since it is in parallel with a word designating “the heights,” it probably designates “well-watered valleys.”⁵³² On the other hand, T.’s proposal that the {y} would be the *nisbe* ending is certainly plausible (i.e., the stem would have been /mā’-/ , a by-form or a secondary form of /māy-/ , ‘water’), indeed more plausible with my interpretation than his, and my vocalization should, therefore, have been /mā’iyyāt-/ rather than /ma’iyāt-/.⁵³³ If T.’s analysis of this word is correct, it would be the only attestation in Ugaritic of what he above (p. 163 [§33.152a], p. 164 [§33.154a], p. 249 [§51.3e], p. 250 [§51.3l]) posits to be the base form of the word, viz., /mā’-/ (elsewhere only {mh} and {my} are attested).

— p. 251 (§51.4l), pp. 294-95 (§53.322.1). Because plural forms of /qatl/qitl/qutl/-nouns with and without a vowel indicated in the second syllable are attested for the self-same word in syllabic writings,⁵³⁴ it is unlikely that they represent forms with and without syncope of the second vowel as T. holds (in forms where this vowel is absent, “die Pluralbasis ... mit der Singularbasis identisch ist”). Rather, the variant spellings represent either vowel reduction of a type similar to so-called ‘vocal *shewa*’ in Biblical Hebrew (viz., a murmured vowel that constituted a syllable in pre-Massoretic Hebrew—see above, remark to p. 146 [§33.115.44.5], etc.) or else inconsistent use of CV, VC, and CVC signs by the scribes (see above, remark to p. 182 [§33.242a], etc.). An argument for the presence of a vowel in the second syllable of /qatl/qitl/qutl/ substantives not exploited by T. is the form {ḥbtm} in RS 15.098:8 (*KTU* 2.17:1), for the /b/ is retained in this plural form, as contrasted with the singular /ḥpt/, because of the vowel separating it from the /t/ (see remark above to p. 137 [§33.112.31]). The same would be true of *nbkm* in alphabetic script (RS 24.249:10' [*KTU* 1.105]),⁵³⁵ though here the singular is attested both as *nbk* and as *npk*—the plural shows the root to be NBK (in agreement with Hebrew and Arabic) while the form *npk* constitutes a phonetic writing of the singular, reflecting the pronunciation with devoiced /b/ when in

⁵³²Pardee in *Verse in Ancient Near Eastern Prose* (1993) 212; idem, *Les textes rituels* (2000) 886, with previous bibliography.

⁵³³*Les textes rituels* (2000) 877. T.’s vocalization is /mā’iyyāt-/; on the vocalization of the *nisbe* ending with /yy/, see above, remark to p. 197 (§33.322.42c), and below, remark to pp. 273-74 (§§51.46h-k).

⁵³⁴E.g., {na-PA-ki-ma} in RS 16.150:16 (*PRU* III, p. 47) and {na-AB-ki-ma} in RS 16.263:5 (ibid., p. 49) and {NAB-ki-ma} in RS 17.121 ii 5' (*PRU* VI 56) — spellings of what is in all likelihood the same geographical name meaning ‘springs’.

⁵³⁵Syllabic forms cited in preceding note.

immediate contact with /k/—it seems unlikely that *nbk/npk* represent different proto-Ugaritic bases (as I have suggested above, remark to p. 137 [§33.112.31], for *lbš/lpš*) because both appear in the place name *gt nbk/npk*. It may also be pointed out here that exception to the dissyllabic rule is to be made for certain root types, e.g., II-y (e.g., {ilm}, ‘rams’, = /ʔêlūma/ ← /ʔaylūma/), for, as in the later Northwest-Semitic languages, these usually do not show a /qVtal-/ base (e.g., Hebrew /ʔēlīm/).

— p. 251 (§51.41a). T. does not indicate his reason for stating that Hebrew /ʔōhel/, ‘tent’, would be derived from /ʔahl-/ rather than from /ʔuhl-/. There can be no doubt, in any case, that proto-Hebrew had /ʔuhl-/.

— p. 251 (§51.41a), p. 278 (§52.11). The syllabic data for /baʕlu/, ‘lord’, are not to be found in RS 20.123+ iv B 18 (Nougayrol, *Ugaritica* V, text 137), etc., as is indicated in both these sections, but in RS 20.149 iii 14 (ibid., text 130) and in RS 20.123+ ii 30, 33 ({ba-a-lu-ma}). In column iv B, line 17 (!), of the text cited by T., the Ugaritic entry {ba-a-lu} designates the weather deity *Baʕlu* (as is correctly indicated by T. on p. 169 [§33.182]).

— p. 251 (§51.41a). To the gloss “Holzkohle” for the noun /paḥmu/ is to be added that of ‘brownish-red (died wool)’, for the two principal categories of so-called ‘(royal) purple’ are called *iqnū*, which designates the bluer color, and *pḥm*, which designates the redder color. T. regularly translates the textile product as “roter Purpur” (e.g., p. 361 [§62.811]), but for some reason does not include the gloss here—in spite of the fact that the mention of the textile is far more common than is that of coals.

— p. 251 (§51.41a), p. 560 (§74.414.2), p. 769 (§82.36). It cannot be judged at all likely that {qdm} in RIH 78/3+30:24' (*CAT* 2.81:25) has the meaning of “Vorseit,” the interpretation proposed on p. 251 with no cross reference to the other two sections cited, where the analysis as a verbal form is indicated with no cross reference back to p. 251.

— p. 252 (§51.41a). T. correctly glosses *tlṭ* as “Kupfer, Bronze” here, but regularly uses “Kupfer” for that word when translating administrative texts (e.g., p. 355 [§62.41b]). In terms of *realia*, however, it would appear that copper was used primarily for alloying with tin to form bronze and that most references to the metal would have been to that alloy rather than to pure copper.⁵³⁶

— p. 252 (§51.41a). Whether or not {ʔnm} in RS 2.[009]+ i 50 (*KTU* 1.6) be plural, the base form is more plausibly /qāl/ than /qawl/, for the latter identification requires analyzing the attested form as plural and as derived from /ʔawanīma/ whereas /qawl/ and /qayl/ forms do not show dissyllabic plural stems as frequently as do strong-root forms (/ʔawnūma/ would have gone to /ʔônūma/ and been written {ʔnm}).⁵³⁷ On the general problem, see above, seventh general remark and remark to p. 188 (§33.311.1b), etc.

— p. 252 (§51.41a), p. 275 (§51.5a). T.’s handling of hollow roots is well illustrated by the words *dr* and *drdr* analyzed in these two paragraphs. In the first, *dr*, “(Familien-)Kreis,” is

⁵³⁶References in Pardee, *Syria* 77 (2000) 48-49.

⁵³⁷Below in this very section, T. analyzes {ú-ra-tu} in RS 19.028:6 (*PRU* VI 126) as the plural of /ǵôru/, “Haut, Tierfell.” This plural would thus be based on the singular stem /ǵôr-/ rather than on a hypothetical plural stem /ǵawar-/. (On this word, see also third following remark.)

identified as /dôr-/ ← /dawr-/ while, in the second, *drdr*, “Ewigkeit,” is identified as “/dârdâr/.” The latter is compared with Syriac *dârdârîn* while no etymological basis is proposed for the derivation of the first. Hebrew *dôr* could, of course, come from either /dawr/ or /dâr/ and there *dôr dôr* always appears as two words. For the single word, Arabic has *dawr*- and Aramaic *dâr*. There is, therefore, no way of knowing exactly what the Ugaritic forms were, nor whether the simple noun derived from the same base or from different bases. It can be said that Hebrew *dôr* and *dôr dôr* and Aramaic *dâr* and *dârdârîn* (on the morphology of *drdr*, see below, remark to p. 275 [§51.5a]) can all derive from /qâl/, but I know of no criterion whereby to determine whether the Ugaritic forms were built on the Arabic model (/qawl/), the Aramaic model (/qâl/), or on one of these for one word the other for the other.

— p. 252 (§51.41a), p. 747 (§81.24a). In the first reference cited, T. proposes that the noun ‘*d*’ denoting a long period of time would be /qatl/ (/‘ôd-/ ← /‘awd-/), in the second that the corresponding adverb (‘for a long time’) would be /‘ād-/; for the former he cites Hebrew ‘*ôd* and Arabic ‘*awd*-, for the latter Canaanite *adi* and Hebrew ‘*ôwd* in Gen. 46:29. These comparisons lack cogency, for the writing of the Canaanite form with {a} shows that the base form of that word, if it was indeed Canaanite, was not /‘ād-/ but /‘ad-/. T. appears here to be confusing two lexical entities. Factoring in Biblical Hebrew shows that Canaanite had one noun ‘*ad*- of which the meaning was ‘a long time’ (apparently derived from /‘ady-/, i.e., cognate with the verb ‘DY ‘to pass’) and another /‘awd-/ of which the meaning was ‘revolution’ and which appears in Hebrew primarily as an adverb meaning ‘yet, again’.⁵³⁸ There is no doubt that Ugaritic had a noun meaning ‘a long time’ (which would have been vocalized /‘adû/ if the derivation from ‘DY is correct), but the adverb is used rarely there, only in poetry and only in banqueting scenes where it is followed by the verbs LHM, ‘to eat’, and ŠTY, ‘to drink’. T. translates “lange” (p. 747),⁵³⁹ but it cannot be ruled out that we are dealing with the adverbial usage of the other noun which would have expressed the repetition of the acts of eating and drinking.

— p. 252 (§51.41a), p. 298 (§53.331.2). T. does not explain why, if {ú-ra-tu} in RS 19.028:6 (*PRU* VI 126) is Ugaritic and means ‘hides’, it does not show correct Ugaritic morphology: the noun is preceded by the number ‘2’ and it might be expected to be in the dual, rather than in the plural (above, pp. 289-90 [§53.21], various syllabic writings are cited as evidence for the vocalization of the dual forms).

— p. 252 (§51.41a). T.’s new reading of {šm}, ‘fasting’, in RS 24.255:2 (*KTU* 1.111) may not be admitted.⁵⁴⁰ It must also be judged highly unlikely that the same word is to be found in the form {zm} in RIH 78/20:7 (*CAT* 1.169) — see above, remark to p. 114 (§32.144.24).

— p. 253 (§51.41a). As regards T.’s translation of “Rücken” in RS 24.258:5 (*KTU* 1.114), RS 92.2014:14 (RSO XIV 52), and RIH 78/20:5 (*CAT* 1.169), it is not certain that {gbh} in

⁵³⁸ T.’s reconstruction of Hebrew /‘ôd/ from a hollow root is supported by the spelling {‘wd} of the adverb in the Siloam Tunnel inscription, where the use of {w} as a *mater lectionis* for /ô/ ← /ā/ would be nothing short of astounding.

⁵³⁹ The literal English equivalent would be ‘long did the gods eat and drink’.

⁵⁴⁰ Pardee, *Les textes rituels* (2000) 619-20.

the first text is this word⁵⁴¹ while in the other two ‘back’, however etymologically correct, does not appear to be the proper translation.⁵⁴² In both these cases the word is in parallel with *tmnt*, ‘(body) members’, and *gb* is, therefore, better translated ‘body’ than ‘back’.

— pp. 253, 254 (§51.41b). The only putative example of a noun from a strong root which would exhibit the pattern /qatl/ ← /qatVI/ is /malk-/, ‘king’ (with {malkat-/ ‘queen’, cited as the only example of a feminine noun), but the hypothesis must be considered extremely dubious. The only data cited in favor of the hypothesis are Arabic /malik-/ and the divine name {DINGIR.MA.LIK.MEŠ} as the Akkadian entry corresponding to *mlkm* in the Ugaritic divinity lists; T. also assimilates the divinity *mlk*, named in three para-mythological texts and an administrative text,⁵⁴³ to these *mlkm*. None of these associations proves the point. As is well known from proper names, the principal divine name written *mlk* was pronounced /milk-/ , not /malik-/ , an observation borne out by later transcriptions of the divine name *mlk* ʿšrt with /milk-/ , for the deity best known at Ugarit was the *mlk* of ʿttrt.⁵⁴⁴ This leads to the conclusion that the purpose of the writing {MA.LIK.MEŠ}, obviously not a simple transcription of the Ugaritic word because of the {MEŠ} sign, was to indicate that these *mlkm* corresponded to the old Amorite divine group represented in Akkadian as /malikū/, though the Ugaritic word *mlkm* would plausibly have had the local pronunciation of /malakūma/.⁵⁴⁵ Any direct connection between the Ugaritic common noun ‘king’ and these various forms derived from the root MLK appears too tenuous to merit a place in a grammar of Ugaritic. A positive argument for the antiquity of the /qatl/ base is that both Hebrew and Aramaic show the /qatl/ base for the common noun ‘king’ and the expected /qatal/ base for the plural; the antiquity of the monosyllabic base is thus clear, and there is no reason to doubt that Ugaritic shared it.

— pp. 253-254 (§51.41b). As is shown by the fact that they are stative adjectives, all the examples of /qatl/ ← /qatVI/ from geminate roots could plausibly have been classified as originally /qatil/ or /qatul/, a pattern visible in the Hebrew verbal system, where most active verbs are of the /qālal/ type in the third-person forms (e.g., *sābab*, ‘he went around’) while statives are /qall/, i.e., ← /qalila/qalula/ (e.g., *raʿ*, ‘he is/was evil’). Once {mlk} ‘king’, and {ʾny}, ‘(group of) boat(s)’, are removed from this section (see above, preceding remark and remark to p. 192 [§33.312.32b], etc.), these stative adjectives from geminate roots are the only remaining examples of the phenomenon.⁵⁴⁶ As with many other categories of this

⁵⁴¹ Idem, *Les textes para-mythologiques* (1988) 21, 22, 35-38.

⁵⁴² Idem, *Les textes rituels* (2000) 826, 876, 884; Ford, *UF* 34 (2002) 120, 146-47, 155.

⁵⁴³ See my presentation and discussion of the data in Craigie (1988) 55-68 as well as my commentary of the para-mythological texts in *Les textes para-mythologiques* (1988), chs. 2 (pp. 75-118 on RS 24.252) and 7-8 (pp. 193-226, 227-56, on RS 24.244 and RS 24.251, respectively). The administrative text (RS 86.2235) has now been edited by Bordreuil and Pardee as RSO XIV 39.

⁵⁴⁴ The data behind this assertion were gathered in the article in the Craigie volume cited in the previous note. See also above, remark to p. 69 (§21.412g).

⁵⁴⁵ Pardee, *Les textes rituels* (2000) 311-14, esp. n. 140 (p. 314).

⁵⁴⁶ It should be remarked that Hebrew shows some strong-root construct forms where /qatil/ has gone to /qatl/, but the absolute form has retained the dissyllabic stem (e.g., *yārēk* and *yērek*, *kātēp* and *kētep*).

grammar, the internal and comparative evidence shows that the loss of the second vowel in these forms took place far before Ugaritic existed as a discrete entity, and one may doubt the propriety of including a category /qatl/ ← /qatVI/ in a grammar of Ugaritic. Or, if one wished to do so, one could have a single category /qatl/ with sub-categories for proto-Ugaritic derivations (but, as I have just shown, the /qatVI/ category would be sparsely populated, inhabited by one dubious example from a strong root and a number of examples from geminate roots).

— p. 253 (§51.41b), p. 259 (§51.42d), p. 280 (§52.222). In these paragraphs, T. follows the traditional interpretation of *dqt* and *gdlt* in the ritual texts as meaning ‘ewe’ (viz., ‘the small female’) and ‘cow’ (viz., ‘the large female’), whereas he has more recently proposed that the terms designate small and large loaves of bread.⁵⁴⁷ Though any solution is presently hypothetical in that, for lack of explicit comparative evidence, the basis therefore is largely etymological, the following objections to the new hypothesis may be mentioned. (1) The most obvious is the absence of a West-Semitic word for ‘(loaf of) bread’ that would be feminine in gender. On p. 556 of the article in *UF*, T. describes the use of the feminine gender as “unproblematisch,” ascribing the gender to an unknown feminine noun in Ugaritic or to the use of the feminine gender to express a *nomen unitatis*, but the first solution relies on an unknown while the second is not altogether satisfying because he cites no examples of substantivized adjectives used as *nomina unitatis*. This situation is in stark contrast with that of identifying *gdlt* and *dqt* with animals, for in Arabic *daqīqat-* is used for a “pièce de bétail, particulière brebis (*par opp.* à *ḡalīlat-* grosse pièce, chamelle)”⁵⁴⁸ and *daqqāḥ* is used in Hebrew, with and without *b^ḥēmāḥ*, to designate small cattle and alone for small animals of various kinds.⁵⁴⁹ Such data are purely indicative of possibilities, of course; they do not prove that similar usages existed already in Ugaritic. But they do exist, which is more than T. has been able to show for the hypothesis that the terms in question designate bread.⁵⁵⁰ (2) Both *dqt* and *gdlt* appear primarily in the singular and the dual (the exceptions are in two texts, where mention is made in both of seven *gdlt* and fourteen *dqt*: RS 24.250:19-21 [*KTU* 1.106] and RS 24.256:26-27 [*KTU* 1.112]). While it is plausible for ewes and cows to be offered singly and in pairs, for that is the pattern with š, the same cannot be said of loaves of bread. The number of twelve loaves established for the Hebrew *leḥem happanīʾm* is a

⁵⁴⁷“Brot als Opfermaterie in Ugarit. Eine neue Deutung der Lexeme *dqt* und *gdlt*,” *UF* 33 (2001) 545-65. For bibliography on the interpretation of *dqt* and *gdlt* see Pardee, *Les textes rituels* (2000) 23 n. 23; for references specific to *gdlt* see p. 34 n. 105.

⁵⁴⁸A. de Biberstein Kazimirski, *Dictionnaire arabe-français* (Paris: Maisonneuve, 1960) 716.

⁵⁴⁹M. Jastrow, *A Dictionary of the Targumim, the Talmud Babli and Yerushalmi, and the Midrashic Literature* (New York: Title, 1903) 318.

⁵⁵⁰On p. 555, T. cites Hebrew and Arabic evidence for √RQQ [!] used to designate types of bread, but, in contrast with such usages of √DQQ for animals, such forms are usually masculine (the one exception being *ruqāqat-* used as a *nomen unitatis*). It should also be borne in mind that Ugaritic had a term *gdl* that seems to have designated a type of flour (see below, remark to p. 259 [§51.42c]), which may or may not be derived from the root GDL, ‘be big’. Given the comparative evidence for *dqt* = a type of animal and the absence of comparative data for a feminine word for ‘bread’, one may doubt that *gdl* and *gdlt* both designated cereal products.

more plausible number for a sacrificial feast involving the sacrifice of whole animals hence a fairly large number of participants. (3) T.'s argument (*UF* 33, p. 551) that we would expect explicit terms for 'cow' and 'ewe' is certainly unexceptionable as an observation, but it applies equally to '(loaf of) bread' and is hence valueless as an argument in favor of one solution or the other. (4) Though it is clear that various foodstuffs were a part of the feast that accompanied many sacrifices, none of these items is mentioned in the ritual texts with the frequency of *dqt* and *gdlt*. This point may be demonstrated explicitly for wine (*yn*) because wine is prescribed as an offering for only one feast (the full-moon festival of the last month of the year) though we happen to have an administrative text that informs us of shipments of wine for a minimum of nineteen sacrificial feasts for some of which we have the prescriptive ritual wherein there is no mention of wine.⁵⁵¹ In stark contrast, some two hundred each of the *gdlt* and the *dqt* were prescribed in the texts as we know them. (5) T.'s argument that offering the large numbers of bovids and ovids (roughly 500 of the former and 1000 of the latter) "hätten die Ökonomie des Stadtstaates Ugarit wohl überfordert" (*UF* 33, p. 551) borders on the ridiculous because we know virtually nothing about the temporal spread covered by the texts nor of the numbers of persons who benefited from the sacrifices. If we consider that most of the texts reflect the royal cult, that approximately one tenth of the offerings were consumed entirely in honor of the divinities,⁵⁵² and that the sacrificial cult in all likelihood furnished the major source of calories from animal flesh for the palace personnel and, perhaps in some cases, for example the full-moon festival of the last month of the year, for a number of other inhabitants of the city of Ugarit, the numbers do not appear at all out of line. Unfortunately, the type of data provided by the texts from Ras Shamra allows only for generalizations as regards numbers of animals offered and no data whatsoever for the use and distribution of the animals that were not entirely consumed in fire. (6) T.'s argument (*UF* 33, p. 552) that the ratio of females to males among the bovids (roughly 200/300) as compared with that among the ovids (roughly 200/800) is "nicht realistisch" is unsupported by any comparative data. I have observed that the percentage of females offered as holocaust sacrifices was higher than that of males and that this was a reflection of the value of such offerings, the desire on the part of the offerer being to present the more highly valued animal when it is to be consumed entirely by the deity.⁵⁵³ If there is anything to such an argument, one might expect a higher percentage of the more valuable of the two types of animals to have been so offered. (7) Most of T.'s comparative evidence for the importance of bread offerings is from Hittite sources and, in Akkadian, from Emar. Anyone who has studied these two bodies of evidence must have remarked the important differences between them and the Ugaritic cultic texts: there are very few explicit

⁵⁵¹For the details, see Pardee, *Les textes rituels* (2000), commentary of RS 1.003/RS 18.056 (the two prescriptive rituals for the last month of the year) and RS 19.015 (the administrative text). For a correction to the calculation of the quantities mentioned in RS 19.015 (approximately 1000 liters as opposed to the number of 2000 proposed in *Les textes rituels*), see Pardee, *Topoi* 11 (2001) 673, and below, remark to p. 775 (§82.411).

⁵⁵²Idem, *Les textes rituels* (2000) 911.

⁵⁵³Ibid., p. 922.

indications of Hittite influence on Ugaritic cultic practice and, as for the cult of Emar, though the texts are in a Semitic language and the rites were practiced by an ethnic group of certain Semitic origin, it also shows very important differences as compared with what we know from the Ugaritic ritual texts. In contrast, there are very significant similarities in terminology and probably in practice between the Canaanite cult as known from the Hebrew Bible and the Ugaritic cult as known from the Ugaritic ritual texts—and the ideologically important but economically unimportant place of bread in the Hebrew cult is well known (i.e., practically limited to the so called ‘showbread’ in the regular cult and unleavened bread in the Passover festival). If bread held a similar place in Ugaritic cultic practice, there is nothing surprising about its absence from the texts that have come down to us. The best analogy would be the handling of blood: later West-Semitic practice shows that blood was ideologically important and one may surmise that it was not totally unimportant for the Ugaritians—but it is not mentioned even once in the ritual texts. As I have remarked in my conclusions to the study of these texts, the absence of mention of blood in these texts is no doubt owing to the literary genre: these texts prescribe offerings, not techniques for carrying out the various sacrifices mentioned.⁵⁵⁴ The biblical texts, on the other hand, belong broadly to the category of legislation in which offerings, offering types, techniques, and distribution of the offerings and sacrificial body parts are all important. It is highly likely that bread, along with wine (see above), was part of the sacrificial feast, but that does not mean that it should necessarily be mentioned in the ritual texts. In sum, I must conclude that, though T. has raised an interesting question, his data and his opinions regarding plausibility are not of a nature to require that the former hypothesis be abandoned on the spot. His first instincts served him better here than has his later bright idea.

— p. 253 (§51.41b). Many have thought that the word *ql* that designates a ‘messenger’ should be explained, not by *ql*, ‘light, swift’, but by *ql*, ‘voice’ (i.e., the G-participle of the corresponding verb ‘to give voice’, hence meaning ‘he who speaks’).⁵⁵⁵

— p. 253 (§51.41b). T. correctly indicates that the Hebrew cognate of Ugaritic *tm-* /*tamm-*/, ‘complete’, shows *qameš* (unexpectedly so, for most /*qal-*/base nouns show a /*qal-*/ masculine singular form, i.e. with *pataḥ*, in Hebrew); Arabic, however, as expected, has /*tamm-*/, not “*tāmm*.”

— p. 254 (§51.41c), p. 288 (§53.121a), p. 301 (§53.34). If by the glosses “Angehörige; Leute; Dienerschaft” (p. 254) for *inš* in the nominal phrase *inš ilm* T. is subscribing to the theory that these *inš ilm* were the human devotees of the gods, that interpretation must be rejected: this phrase consistently occupies the slot elsewhere filled only by divine names in the ritual texts and the idea that offerings would be made to the human servants of the divinities appears untenable.⁵⁵⁶ Moreover, I see no reason why /*ʾinš-*/ should be preferred over /*ʾināš-*/ as the vocalization of the form, thereby preferring a rare Arabic form over the better-attested form, i.e., in Hebrew and Aramaic as well as in Arabic. Despite these

⁵⁵⁴ Ibid., pp. 923-24.

⁵⁵⁵ For bibliography, see idem, *Syria* 77 (2000) 51.

⁵⁵⁶ Discussion and bibliography in idem, *Les textes rituels* (2000) 88-89.

objections made with regard to the meaning of the divine category *inš ilm* and the vocalization of the first word, the identification of the Ugaritic word *inš* as a collective noun for ‘people, persons’ is quite likely. Hence the interpretation of *inš ilm* as a genitive of identification meaning ‘people (who are = have become) gods’, i.e., the departed dead.⁵⁵⁷ As the phrase *ins ilm* is the only basis cited for the assertion that *inš* is semantically more distant from *nšm*, ‘people’, than one might think (p. 301), the assertion must be dismissed. Hebrew had both ^a*nāšīm*, ‘men’, and ^e*nōš*, ‘people (i.e., a collective)’; why should Ugaritic not have had two similar terms?

— p. 254 (§51.41c). I know of no objective basis on which to prefer /ḥilm-/ over /ḥulm-/ or /ḥulum-/ for the vocalization of the word for ‘dream’ (on this word as an illustration of the general problem, see above, remark to pp. 247-77 [§51]).

— p. 254 (§51.41c), p. 371 (§64.12). On p. 254, *nšp*, “Halbschekel,” is classified with no indication of doubt as a /qitl/ base; on p. 371 as either /qitl/ or /qutl/.

— p. 254 (§51.41c), p. 303 (§54.111). A Ugaritic word /qidš-/ meaning “Heiligtum” is said to be attested in the polyglot vocabulary RS 20.123+ iii 4', iv a 14 (Nougayrol, *Ugaritica* V, text 137). The first reference is incorrect (it should be iii 29"); it might have been observed that in the second text (cited only on p. 254) the term corresponds to what should have been a divine name.⁵⁵⁸

— p. 254 (§51.41c), p. 773 (§82.313), pp. 776-77 (§82.413). On p. 254, the noun *qrb*, “Inneres,” is classified with no indication of doubt as /qitl/; on pp. 773 and 776, the preposition *qrb* is vocalized /qarba/ and said to be derived from a substantive /qarb-/ meaning “Inneres.”

— p. 254 (§51.41c). The noun /šît-/ , translated “Stellen, Legen,” for which the vocalization is provided by a polyglot vocabulary,⁵⁵⁹ is given twice on this page, once under strong roots, again, and appropriately, under hollow roots.

— p. 254 (§51.41c). One detects a hint of refusing to come to terms with the problems of hollow roots in several entries for /qitl/ nouns from hollow roots. The first two entries are reconstructed as /qîl/ ← /qiyl/, but the third, the noun {nr} meaning ‘light’, is indicated, with a question mark, as “/nîr/,” but with no derivation indicated and with other possible bases /qutl/ or /qatîl/. As a matter of fact, the forms of this root across the Semitic languages do not derive unambiguously from a proto-root NYR or NWR, and this example underscores the problems associated with attempting to vocalize every Ugaritic noun and with attempting to assign specific root designations for hollow-root forms.

— p. 255 (§51.41c), p. 716 (§76.541a). The proper reading of the last visible word in RS 24.266:34' (*KTU* 1.119) is not “*šlt[km]*” but {ʃlʔtkʔ[m]}. As a matter of fact, the epigraphic remains favor {b} over {s} as the preferred reading of the first sign, but I have been unable

⁵⁵⁷Idem, *Gibson* (1996) 273-87, esp. 283-84.

⁵⁵⁸Cf. Huehnergard, *Ugaritic Vocabulary* (1987) 173.

⁵⁵⁹RS 20.149+ iii 10' {ši-tu} (Nougayrol, *Ugaritica* V [1968], text 130).

— p. 256 (§51.41e). The glosses “Lieblichkeit; Reiz” for /nu‘mu/ are excessive. The root is used in Ugaritic as the general term for ‘good(ness)’. The distribution is the mirror-image of that in Hebrew where *n‘m* is highly marked, denoting ‘beauty’, ‘pleasantness’, etc., while *ṭb* is the generic word for ‘good(ness)’. In Ugaritic *ṭb* is the rare and specific word while *N‘M* has the broader semantic spread of the two.

— p. 256 (§51.41e). T. might have explained why {1 TÚG.a-ga-su-nu} in RS 19.028:2 [*PRU* VI 126]) is cited as {su-nu} and as providing the vocalization of Ugaritic {sīn}, ‘hem (of a garment)’.⁵⁶⁵

— p. 256 (§51.41e). It must be observed that the state of RS 19.059 (*KTU* 1.94) is too poor to assure that {qdš} in line 1 and elsewhere in the text certainly means either “Heiligkeit” or “Weihegabe.”

— p. 256 (§51.41e). Why would a word like {tqd}, ‘almond’, that designates a nut that must have grown in the area for millennia, be considered an Akkadian loan-word? No reason is given, but the thinking may have been that /tuqdu/ is East Semitic, /lūz-/lawz-/ West Semitic. However that may be, the presence of *šaqēd* in Hebrew makes it unlikely that the word was a loan-word specifically into Ugaritic. The basis in syllabic writing for the vocalization as a /qutl/ base is cited as “šu-uq-du” from RS 19.035B:4' (*PRU* VI 159). Those three signs are, however, followed by {ma}, and Nougayrol read the word as {šu-uq-du-ma}. It must be said that, if Nougayrol’s sign-division not be accepted, there is no reason to see the word as Ugaritic, for *šuqdu* is Akkadian; if it be accepted, then the word provides another case of the plural of a /qatl/qitl/qutl/ form not showing a dissyllabic stem in syllabic writing (see above, remark to p. 251 [§51.4]).

— p. 256 (§51.41e). {ūz}, ‘goose’, is vocalized /‘ûz-/ ← /‘uwz-/ on the basis of Akkadian /ūsū/ and the alternative vocalization /‘ôz-/ ← /‘awz-/ is considered, but the simplest option is ignored, viz., that this is a /qūl/-base noun.

— p. 256 (§51.41e). It is unclear why {ūr}, “Feuer,” is vocalized /‘ûr-/ for the origin of the contracted vowel is not indicated. One would expect T. to have proposed the proto-Ugaritic to have been /‘uwr-/. Here, as in other such cases, the base may have been /qūl/ or even /qawl/ (cf. Hebrew ‘ōr, ‘light’).

— p. 256 (§51.41e), p. 279 (§52.211), p. 303 (§54.111). The syllabic basis for the vocalization of *pwt*, ‘madder’, as /puwwat-/ is cited as RS 23.368:14'. This is an example of a syllabic text that is listed as unpublished in Bordreuil and Pardee, *La trouvaille* (1989) 295, for which T. should, therefore, have indicated his source for the reading.⁵⁶⁶

— p. 256 (§51.41f), p. 366 (§63.124), p. 700 (§76.426). In the first paragraph cited, T. lists RS 2.[003]⁺ i 15 (*KTU* 1.14) as his primary occurrence of the word *ūm*, ‘mother’, in Ugaritic without noting that in the other two he cites his own re-reading of the line as containing *ūn*, “Totenklage,” rather than *ūm* (see below, remark to p. 364 [§63.112], etc.).

— p. 257 (§51.41h). It is unclear why T. lists as possibilities for vocalizing *d‘t*, ‘knowledge’ only /da‘at-/ and /di‘at-/ when Hebrew also shows *dā‘at* ← /da‘t/.

⁵⁶⁵The editor took the last four signs as denoting a single word designating a particular garment (Nougayrol, *PRU* VI, p. 158).

⁵⁶⁶The only reference to this text in my files is van Soldt, *UF* 22 (1990) 348, 350.

— p. 257 (§51.41h), p. 638 (§75.516b). As noted above (remark to p. 57 [§21.352.1 {t} für {m}]), the last word of RS 17.139:31 (KTU 2.34:33) is to be read {l^dl^dtk}, ‘your *dūdu*-jars’, not {l^dtk}, ‘your giving birth’ (see also below, remark to p. 266 [§51.45e *mrt*]). The verbal substantive *ldt* appears, therefore, not yet to be attested in Ugaritic.

— p. 257 (§51.42a). It cannot be judged certain the *h^lb* in RS 17.118:2 [not 1!] and 5 (KTU 4.272), in a text otherwise dealing with metals, is the same word as *h^lb*, ‘milk’.

— pp. 257–58 (§51.42a), p. 272 (§51.46e). In the first section cited, T. identifies {[...]r^gb} in RS 24.247⁺:19 (KTU 1.103) as a /qatal/ noun meaning ‘famine’; in the second, {r^gbn} in line 5⁵⁶⁷ of the same text is identified as a /qatalān/ noun with the same meaning (though, for reasons unclear, he translates “Hunger” in the first case, “Hungersnot” in the second). He does not mention the strong possibility that the {-n} of the latter form is the affirmative particle, which Hoftijzer identified as appearing on each singular absolute noun in first position of the apodosis in this omen text and which, in this construction, was identified as “-n of apodosis.”⁵⁶⁸ If this identification be correct, there is no noun /ra^gabānu/ in Ugaritic, only /ra^gabu/. On the other hand, since the text before {[...]r^gb} in line 19 has disappeared, one cannot be certain that the noun is present there.

— p. 258 (§51.42a). T. does not tell the reader why he identifies *šd*, ‘field’, as the only III-weak noun derived from a III-w root, with several others listed below as III-y.

— p. 258 (§51.42a). “*gem. rbt*,” a feminine noun from a geminate root, should be moved to the end of the section, after the other examples of feminine nouns from the /qatal/ base (on the dubiety of this entry, see above, remark to p. 182 [§32.242b], etc.).

— p. 258 (§51.42a). The problem of syncope of short vowels in words of three or more syllables comes to the fore in the examples provided here of substantives presumed to come from the /qatalat/ base. T. does not state his reasons for classifying the first four as /qatal(a)t/ and the following five as /qatalat/, though one of each category is, for him, clearly /qattalt/, the other /qatlat/: *āht* he believes to come from /ʾahadt/ (for the analysis of this as from a /qattalt/ base, see above, remark to p. 142 [§33.115.11], etc.), whereas {tⁱgt/tⁱqt}, of which the {i} shows the form to have had either /i/ or /ø/ after the /ʾ/, would, if the original base were /qatalat/, show syncope of the second vowel: /t^aʾgat-/t^aʾqat-/ ← /t^aʾagat-/ (the reconstruction of this particular form is based on Hebrew /š^ʾāgāh/). At the middle of the stream where T. changes horses is the word {nhqt}, vocalized as /nahaqat-/. This appears to provide the key to T.’s organization of this paragraph for, with {tⁱgt/tⁱqt}, {g^ct} and {z^gt},⁵⁶⁹ {nhqt} appears in a repeated passage of the *Kirta* epic where all four terms express animal

⁵⁶⁷ The word is in all likelihood not present in line 12, as indicated by T. following KTU/CAT (see Pardee, *AfO* 33 [1986] 120, 123; idem, *Les textes rituels* [2000] 540–41).

⁵⁶⁸ Kraus (1982) 121–23; cf. Pardee, *AfO* 33 [1986] 126, 129; idem, *Les textes rituels* (2000) 541, 553, 555, 556–57, 558 n. 239.

⁵⁶⁹ The last two forms are mentioned elsewhere as derived from III-w and III-y roots. Cf. p. 200 (§33.323.4b): /gaʾāt-/ ← /gaʾayat-/; p. 199 (§33.323.3a): /za^gât-/ ← /za^gawat/ (on p. 189 [§33.311.3b] the reconstruction /zi^gât-/ ← /zi^giy(a)t-/ [!] is proposed, with no cross-referencing of one reconstruction to the other—the inconsistency has been noted above in the remark to p. 189 [§33.311.3b], etc.).

sounds.⁵⁷⁰ There is no solid basis on which to base these reconstructions as /qatalat/ forms, however, and, to the extent that these hypothetical vocalizations have contributed to the theories of syncope and monophthongization proposed above at phonology, they must be regarded as purely heuristic.⁵⁷¹ As may also be the last example of /qatalat/ given here, viz., {tlttm}, for there is no particular reason to believe that this noun, which may or may not mean ‘two groups of three’, was not formed on the stem of either the cardinal or the ordinal number (cf. below, remark to p. 384 [§67.22]).

— p. 258 (§51.42a). How do Syriac /dunb^atā/d^anub^atā/ establish that Ugaritic {dnbtm} was a /qatalat/ form?

— p. 258 (§51.42c), p. 274 (§51.48a), p. 313 (§54.133.2d), p. 372 (§64.21), p. 373 (§64.24), p. 413 (§69.233), p. 832 (§89.28). On p. 258, T. identifies *hsr* as having a /qatil/ base, translates it as “mangelhaft, von schlechter Qualität,” and cites this word as appearing in RIH 83/12 and RIH 84/8; rendering these passages below, he translates “minus (w.: als Abzug)” (p. 313) and “abzüglich” (pp. 372, 413, 832).⁵⁷² If the “wörtlich” translation is meant to indicate that *hsr* is formally a noun, that is the only place where the identification is made. RIH 83/12:7-8 (// RIH 84/8:11-14), however, if correctly understood, leaves no doubt that *hsr* is a noun, for there *hsr* follows and modifies a dual noun (see above, comments on *lth/mlth*, p. 123 [§32.146.23a], etc., where T.’s interpretation of *mlthm* in the RIH texts is criticized). *hsr* is, therefore, not only functionally similar to *kbd* (on which see above, remark to p. 140 [§33.112.52], etc.), but also morphologically similar, for the much better attested *kbd* is invariable in form and hence functions as an adverbial.

— p. 258 (§51.42c). There are good reasons for believing that Ugaritic (and Hebrew) *yph*, ‘witness’, is a /qātil/ form,⁵⁷³ rather than /qatil/, as T. classifies it here.

— p. 259 (§51.42c), pp. 642-43 (§75.521d). Because we have no idea what the precise vocalization of the stative and active verbal adjectives of hollow roots may have been in Ugaritic and because the corresponding Hebrew forms are not easily reconstructable, the unqualified presentation of the forms as /gêr-/ and /qâm-/ has no place in a grammar of Ugaritic. One thing appears certain: proto-Hebrew cannot have had /qâm-/, because that form would have become /qôm/. The irreducible /ā/ of the Hebrew form (as in /qāmēy YHWH/, ‘those who arise against the Lord’) must, therefore, have had another origin. Also dubious is the derivation of /gêr-/ from /gawir-/: according to T.’s own §33.323.1 (p. 198), /awī/ is supposed to go to /ī/. It is, in any case, more likely that the Hebrew stative form /qēl/ arose from a II-y root with paradigm simplification on this pattern—viz., whatever the II-w form was, it has disappeared. The problem of the hollow-root active participle is not even

⁵⁷⁰ RS 2.[003]⁺ iii 16-18, v 8-11 (*KTU* 1.14). The first passage contains {tīgt}, the second {tīqt}.

⁵⁷¹ {tīgt}/{tīqt} may well be /qitl/, a base used commonly for verbal substantives in Ugaritic (cf. here pp. 254-55 [§51.41c]), rather than /qatalat/ as in Hebrew. The proximity of /g/ and /t/ might account for the strange spelling with {q} (though {k} would have been expected instead of the ‘emphatic’ {q}).

⁵⁷² T. first argued for the function of *hsr*, “minus,” as the antonym of *kbd*, “plus,” in *UF* 29 (1997) 663-64, n. 6. The former is not used as is the latter in number phrases, but the basic interpretation in RIH 83/12 and RIH 84/8 appears valid.

⁵⁷³ Pardee, *VT* 28 (1978) 204-13.

mentioned at §33.322.4 (pp. 196-98), devoted to triphthongs with a long vowel in the first syllable. Since there are no internal data for the form of the Ugaritic verbal adjectives of hollow roots, a reconstruction should at least take into consideration the later data and do so explicitly.

— p. 259 (§51.42c). The vocalization “/birkat/” for *brkt*, ‘pool’, must be an error for /barikat-/ since the word is listed under /qatil/ bases and the classification as a /qitl/ is indicated only as an alternative.

— p. 259 (§51.42c), pp. 473-77 (§73.42). Because the Ugaritic verbal system, demonstrably in prose but plausibly in poetry as well, is of the West-Semitic type, where /qatal/qatil/qatul/ have become perfective verbal forms, the presentation of /qatil/ as a valid alternative for the vocalization of the Ugaritic passive participle because that is one of the Akkadian ‘permansive’ forms does not merit the consideration that it receives in §73.42, where /qatil/ is indicated as a valid alternative alongside /qatīl/qatūl/. (On the question of the form of the G-passive participle in Ugaritic, see remark below to p. 473 [§73.422], etc.) This appears to be proven by the /qatila/ verbal forms that are transitive in meaning, e.g., {lik}, /la’ika/, ‘he sent’, common in prose and well attested in poetry. It must be admitted, therefore, that in Ugaritic, as in the later Northwest-Semitic languages, one of the adjectives with a long vowel in the second syllable had assumed the role of the G-stem passive participle. What remains uncertain is to what extent the /qatila/qatula/ forms retained the function of statives in Ugaritic, as in the later West-Semitic languages (see remark below to pp. 664-65 [§75.534]).

— p. 259 (§51.42c). If *n’r* in RS 1.012:7 (restored; not indicated in *KTU* 4.14) expresses some form of ‘flour’ (see above, fourth general remark), then *gdl*, which appears next to *n’r* in several administrative texts (RS [Varia 22] [*KTU* 4.786], RS 94.2479, and RS 94.2600) must also denote a type of flour (or a cereal) and not be the simple adjective meaning ‘large’, identified here as a /qatul/-adjective.⁵⁷⁴

— p. 259 (§51.42c). T. proposes that Ugaritic {‘mq}, “tiefliegende Stelle; Tal,” should be vocalized /‘amuq-/, citing RS 20.024:18⁵⁷⁵ {a-mu-q[uʔ]} as evidence. As has been indicated by Pardee and Xella, however, the text in question is to be read {A^{mu-ú}}, i.e., ‘water(s)’.⁵⁷⁶ This reading of the Akkadian text is confirmed by the corresponding entry in RS 24.643:41, where *thmt*, ‘waters (of the primeval deep)’, is preserved almost intact as the element corresponding to {A^{mu-ú}} in the deity list,⁵⁷⁷ and by the corresponding entry in other syllabic texts.⁵⁷⁸

⁵⁷⁴For the identification of this comestible with Eblaite *gadalum*, see del Olmo Lete and Sanmartín, *Diccionario I* (1996) 144; for a new detailed study of RS 1.012, see Pardee, *AuOr* 20 (2000) 163-82 (on the reconstruction of *n’r* and the meaning of *n’r* and *gdl*, see pp. 171-72).

⁵⁷⁵Nougayrol, *Ugaritica V* (1968) text 18. On the history of reading this entry in the syllabic text and the corresponding entries in the Ugaritic texts RS 1.017 and RS 24.643, see Pardee, *Les textes rituels* (2000) 306-7, 804.

⁵⁷⁶The reading of the text is thanks to D. Arnaud (see above, remark to p. 175 [§33.215.31s], etc.).

⁵⁷⁷See Pardee, *Les textes rituels* (2000) 786, 804.

⁵⁷⁸Cited *ibid.*, p. 804 (texts to be published *in extenso* by D. Arnaud, cf. RSO XIV 22).

— p. 260 (§51.42j), p. 280 (§52.222). As a point of etymological connection, virtually all recent scholarship on *sglt* (\approx Hebr. /s³gullā/) ⁵⁷⁹ has preferred to compare Akkadian *sikiltu*, ‘personal possession’, over the only form cited by T., viz., Akkadian *sugullu*, ‘herd’. Of course, if the two Akkadian words have a common etymology, the distinction loses much of its force: because *sglt* is used in the West not only in its basic economic meaning but also in political-theological contexts, while *sikiltu* and *sugullu* are used almost exclusively in economic-legal contexts in Mesopotamia, sometimes with negative polarity,⁵⁸⁰ it is already clear that the words had separate developments in East and West. That being the case, *sglt* could represent the western semantic development of the word that already in Old Babylonian times meant ‘herd’ in Akkadian.

— p. 260 (§51.42j). T.’s proposal that Ugaritic *bkr*, ‘firstborn’, would be a secondary /qutul/ form, derived from /qutl/ (/bukr-/ → [buk^ur-]), appears to be based solely on the fact that Hebrew shows /b³kōr/—there is no internal Ugaritic evidence. To the extent that this reconstruction was influenced by the other form classified here, viz., putative /puḥur-, that analogy is to be rejected (see above, remark to p. 169 [§33.181.2], etc.).

— p. 261 (§51.43c). Instead of following his usual method of vocalizing a word for which no direct internal evidence exists on the basis of the Hebrew or Akkadian cognate, in the case of *rpū*, ‘shade (of the dead)’, for which Hebrew shows a /qatal/ base, T. chooses /qatīl/, which he translates “der ‘Heile’; der In-Frieden-Seiende.” He suggests as preferred alternatives /qātil/, “Heiler,” and as other possibilities /qatil/ or /qatal/, which he does not translate. He does not state why in this case he prefers to ignore the Hebrew evidence; why he prefers /qatīl/ over /qātil/ (which is well attested for this root in Amorite/Ugaritic proper names, e.g. /‘ammurāpi’/); nor why he prefers /qatīl/ over /qatil/ (the more common stative adjectival base). Because this is a quasi-divine name (i.e., it denotes the shades of the dead who have joined the realm of the nether-world divine), one might expect its basic form not to have been lost in the Hebrew tradition, while the change from /qatīl/ to /qatal/ does not appear of the type to qualify as the disphemy that was applied to some divine names, e.g., /‘aštōret/.

— p. 261 (§51.43d), p. 365 (§63.113), p. 369 (§63.214), pp. 382-86 (§67), p. 393 (§69.133.21a). Several of T.’s suggestions to analyze number substantives as nouns (he proposes that the base was /qatūl/, which is plausible, though no direct evidence exists to support the hypothesis), rather than as ordinal numbers, make eminent sense, either for purely semantic or for morpho-syntactic reasons (e.g., *b šb^c šnt*, cited here below note 584). I cannot, however, say the same for the case where the counted entity is introduced by the preposition *b*: in RS 2.[004] v 3’-4’ [*KTU* 1.17] one finds *w hn šb[] b ymm*, “Und siehe, bei der Siebenzahl an Tagen (d.h. am siebten Tag).” Why not ‘on the seventh in (the count of the) days’? If such an analysis of *šb^c b ymm* is possible, what about *mk b šb^c ymm* (RS 2.[008]⁺ vi 31-32 [*KTU* 1.4])? The analysis of *šb^c* as a substantivized adjective in construct with the following noun that I suggest for other cases is perfectly plausible here also (‘in the

⁵⁷⁹For example, del Olmo Lete and Sanmartín, *Diccionario* II (2000) 398-99.

⁵⁸⁰See *CAD S* on *sakālu* and *sikiltu* (pp. 68-69, 244-45).

seventh of days’).⁵⁸¹ Nor is the case of RS 24.248:15 (*KTU* 1.104) terribly convincing. In that text, the interpretation of *b ym* ‘šr as “am Tag der ‘Zehnzahl’” is based entirely on the presence of the word *ym* in the formula, for the usual expression of days of the ritual month omits the word *ym*. There may, however, be two other cases of *ym* appearing in such expressions: {*b ym* [tmn]} in line 7 of the same text⁵⁸² and {[b] y[m] [.] [šb’]} in RS 24.266:1 (*KTU* 1.119)⁵⁸³—unfortunately, both passages are damaged and the readings are not certain. Since, however, the point of reference of the formula *b* + ordinal is the day of the lunar month, it does not appear implausible that the word for ‘day’ may occasionally appear in the formula. I would also query whether the interpretation of *šb’* as a noun in RS 24.256:10 (*KTU* 1.112) is the best. There the phrase *b šb’ ym ḥdṭ* may be vocalized /bi šabī’i yamī ḥudṭi/ and interpreted literally as ‘on the seventh of the days of the new moon’.⁵⁸⁴ T. admits that the phrase has an “ordinale Funktion” (p. 386 [§67.43]) and I fail to see why this morpho-syntactic analysis based on the interpretation of *šb’* as a substantivized ordinal number should not be preferred. Finally, I see no need for the nominal analysis of the number words in a passage in the *Kirta* text, *ḥmš ... ṭdṭ yrḥm* (RS 2.[003]⁺ ii 30-31 [*KTU* 1.14]). For T., this must mean ‘for five ... six months’, and both number words are taken to be nouns from the /qatūl/ base. It appears just as plausible, however, that the author was expressing the length of *Kirta*’s campaign by its extent in time (‘for the fifth ... the sixth month’), rather than by the total. The syntax would, of course, be that of the substantivized adjective in construct with the plural noun (‘for a fifth ... a sixth of months’). T. deems it impossible that *ṭdṭ* should be an ordinal number modifying *yrḥm* (p. 385) without considering the possibility of genitival syntax. The vocalization would be /ya’pi laḥma dā ḥamīši // maḡdā ṭadīti yaraḥīma/, ‘He must bake bread for the fifth (month), fine foods for the sixth month’ (lit. ‘he must bake bread of the fifth, fine foods of the sixth of months’).⁵⁸⁵ — p. 261 (§51.43d), p. 349 (§62.110.2), p. 386 (§67.51). Though the reading of {r} at the end of RS 2.[004] ii 45’ (*KTU* 1.17) may well be correct, the restoration as {[‘š]r]} is highly

⁵⁸¹ On p. 386, T. accepts that *šb’* in the formula *mk/hn špšm b šb’*, ‘At sundown on the seventh (day)’ is an ordinal number. Why is it so much more difficult to conceive of *b šb’ ymm* having the same number component?

⁵⁸² Cf. Pardee, *Les textes rituels* (2000) 566, 567, 569. T. has no entry for ‘tenth’ in his list of ordinal-number forms (p. 368), only an *Anmerkung* to the effect that it is not attested.

⁵⁸³ *Ibid.*, pp. 662, 665, 670.

⁵⁸⁴ *Ibid.*, 634, 638-39. In these comments, I also considered taking *ym* as a singular, but the analysis as a plural is more plausible and supported by the passage from the *’Aqhatu* text cited on p. 639, *b šb’ ymm*, lit. ‘on the seventh of days’ (RS 2.[004] i 15’ [*KTU* 1.17]). T. includes the latter text among his examples of the common noun (p. 385; cf. p. 365 [§63.112]), but this interpretation is not required, from either the semantics or the morpho-syntax of the passage. This genitival syntax may also be attested in RS 3.222 iv 14-15 (*KTU* 1.19) *’d šb’ t šnt*, ‘until the seventh of years’ (/’adē šabī’ati šanāti/), which is followed a few lines later by the formula *b šb’ t šnt*, “bei der ‘Siebenzahl’ der Jahre” (p. 368 [§63.172]), where *šnt* is certainly plural (*šb’* cannot, in this case, be a substantivized adjective because of the absence of gender agreement). T. contradicts himself by later identifying *šb’ t* as an ordinal number (p. 419 [§69.43]), but saying only that it precedes the noun it modifies (on this, see more below in the remark to p. 419).

⁵⁸⁵ See Pardee, *Context I* (1997) 334 (with note 20), 336.

dubious because it is in the company of several incorrect readings and restorations in *CAT* and is based on an over-all restoration of the meaning of the passage that is itself highly dubious. In any case, this attestation of the word deserves no place in a serious grammar and its removal weakens the case for the attestation in Ugaritic of a /qatūl/ noun meaning “Zehnzahl.”

— p. 262 (§51.43f). T. glosses *ilh* as “‘Gott’, auch GN [Göttername] und PN [Personenname],” but the usage as a common noun is, to my knowledge, not known from Ugaritic. There is no doubt that *ilht* functions as the plural of *ilt*, ‘goddess’,⁵⁸⁶ but the masculine form is attested only as the divine name *ʾIlāhu* and, in the plural, to designate a specific group of deities, plausibly those who were classified as the direct offspring of *ʾIlū*.⁵⁸⁷

— p. 262 (§51.43h), p. 288 (§53.121a). In the first paragraph cited, T. translated *phd* in RS 2.[004] v 17', 23' (*KTU* 1.17) as “Lamm,” while in the second he categorizes the word in the same passage as a collective meaning “Lämmerherde.” The phrase in both instances is *imr b phd*, ‘a lamb in/from the *phd*,’ and there can be no doubt that *phd* denotes a group and that the second translation better suits the passage.

— p. 262 (§51.43h). The proper notation for the alternative feminine singular endings on a noun with a long vowel in the second syllable is not “/ḥurāp(a)t/,” but /ḥurāpat-/ḥurapt-/, i.e., the hypothetical /ā/ would have become /a/ if the syllable were closed (T. does not discuss the date at which this proto-Semitic phenomenon ceased operating in West Semitic). T. seems not to be unaware of the problem, for this is the only form I noted of bases with a long vowel in the second syllable for which he proposes the alternative feminine ending /-t/, whereas he did so more frequently for bases with two short syllables (see above, remark to p. 258 [§51.42a]). For the principle in action, see discussion above (remark to p. 182 [§33.242a], etc.) of the gentilic *ʾugartiyyu*.

— p. 262 (§51.43k), p. 478 (§73.431c). With regard to T.’s translation of *nqd* as “Hirte,” the same as for *rʿym* (p. 263), I can only repeat what I once wrote apropos the English translation of *nqdm* by “shepherd”:⁵⁸⁸ “The facts that (1) the *nqdm* are listed alongside *khn̄m*, ‘priests’, in the lists of personnel and (2) the high state official and scribe *ʾIl̄milku* describes his master *ʾAttānu-purulini* as *rb nqdm*, ‘chief of the *nqdm*’ (*CTA* 6 vi 55), indicate that these persons were not the simple equivalents of *rʿym*, ‘shepherds’ (p. 426, cf. p. 494, n. 240, p. 514). If the association with animal husbandry is accepted on the basis of apparent attestations of the word in texts in other languages, these persons must have been major players in the livestock business, perhaps, given the association with *khn̄m*, suppliers of the royal sacrificial cult, which consumed large numbers of beasts (Pardee 1997a: 273, n. 283; Pardee in press: ch. 83).⁵⁸⁹ There is, however, no specific evidence from Ugarit permitting a precise definition of their functions there.”

⁵⁸⁶Pardee, *Les textes rituels* (2000) 37, n. 116.

⁵⁸⁷*Ibid.*, pp. 35-39.

⁵⁸⁸*BASOR* 320 (2000) 73-74 (in a review of *Handbook of Ugaritic Studies* [1999]).

⁵⁸⁹The references are to *Context I* (1997) and *Les textes rituels* (2000).

— p. 262 (§51.43k). I know of no evidence proving that Ugaritic *psl* means “Steinmetz” (for the specific cases of the *psl ḥzm*, ‘arrow makers’, and the *psl qšt*, ‘bow makers’ see below, remark to p. 478 [§73.431d]).

— p. 262 (§51.43k). As hypothesized nearly two decades ago by J. Wesselius,⁵⁹⁰ and accepted by Huehnergard⁵⁹¹ and van Soldt,⁵⁹² RS 14.084:14' (*KTU* 4.126:31) probably does not contain a professional title *tknm*, which is unattested elsewhere, but a gentilic *tknym*, ‘men of the town of *tkn*'.⁵⁹³

— p. 263 (§51.44a). With regard to T.'s hypothesis according to which ‘wr, ‘blind’, would be /‘awwir-/ in Ugaritic and that Hebrew /‘iwwēr/ is a secondary form of the /qattil/ base, it may be remarked that proto-Hebrew already had /qittil/, for, in the proper phonetic environment, the actual Hebrew forms show the expected variant of proto-Hebrew /i/ in the first syllable, e.g., /ḥērēš/ ← /ḥirriš/, ‘dumb’.

— p. 263 (§51.44e), p. 478 (§73.431c). In the first paragraph cited, *ḥrt*, ‘plowman’, is identified as a /qattāl/ formation, in the second, as a substantivized participle. T. is not thinking of different words in different passages, for the same text is referenced in both paragraphs (RS 3.325+ iii 12' [*KTU* 1.16]). In these same two paragraphs, *ḥrš*, ‘artisan, builder’, also undergoes the same divergent analyses, though here different texts are cited (*ḥrš* alone is identified as /qattāl/, *ḥrš* in construct with a second term as the participial form)—there is no reason that I can see, however, to prefer the different forms in the different contexts.⁵⁹⁴

— p. 263 (§51.44e), p. 450 (§73.243.22). In the first paragraph cited, T. takes the first word in RS 18.113A+B:39' (*KTU* 2.42:27) as *mkr*, ‘merchant’, whereas in the second he accepts the reading of the line that I proposed in 1987, viz., {ʾāʾmrkn},⁵⁹⁵ which he translates “ich werde/will verkaufen.” As regards the reading of the first sign, in preparing the hand-copy of this text for the edition of the Ugaritic letters which I am preparing, I was unable to confirm the trace of another wedge to the left of the one clearly visible, and the reading {ʾtʾmrkn} now appears more plausible to me than the one I formerly proposed.⁵⁹⁶ The meaning in this case would be ‘they will sell (them—viz., the boats)’ = /tamkurūna/.

— p. 263 (§51.44e), p. 472 (§73.411). As noted above (remark to p. 178 [§33.231.1b], etc.), the word *šáb*, ‘drawer of water’, does not exist in Ugaritic. In RS 17.073:2 (*KTU* 6.25) the correct reading is {šal[...]}, whereas *šib*, viz., /šāʾibu/, is clearly attested in RS

⁵⁹⁰ *UF* 15 (1983) 315.

⁵⁹¹ *Ugaritic Vocabulary* (1987) 187.

⁵⁹² *UF* 28 (1996) 689.

⁵⁹³ See my re-edition of the tablet, *Semitica* 49 (1999) 59-64. The tiny trace of a sign after {n}, which the authors of *KTU/CAT* took for a {m} may as well belong to {y}, which permits the reading {tknʾyʾ[m]}.

⁵⁹⁴ On p. 263, {ḥa-ra-šu} in RS 20.189:7 (Laroche, *UF* 11 [1979] 479) is cited in support of /ḥarrāšu/, “Handwerker.” Cf. p. 481 (§73.513.1), where {[ḥ]a-ra-š[u]} in RS 20.123+ iii 18' (Nougayrol, *Ugaritica* V [1968] text 137) is also cited for the vocalization and meaning of this word.

⁵⁹⁵ *UF* 19, pp. 206, 209.

⁵⁹⁶ See the pertinent textual remark in *Les textes épistolaires* (in preparation).

19.016:15 (*KTU* 4.609), as is generally recognized. (T. does not include *šib* under /qātil/ substantivized forms on p. 262 [§51.43k], as its use in RS 19.016 would indicate to be the correct functional analysis,⁵⁹⁷ but as a participial form on p. 472.)

— p. 264 (§51.44e). In refuting the hypothesis that the Hebrew and Ugaritic *nomen professionalis* was /qattal/, instead of the common Semitic /qattāl/, T. omits to point out that Biblical Hebrew actually has two forms, one typified by *gibbōr*, ‘hero’, the other by *ḥaṭṭā* (‘), ‘sinner’. The former has assumed the form expected for Hebrew/Canaanite (disimilation of the /a/, shift of /ā/ to /ō/), whereas the latter shows neither of these changes. In the latter, however, the /ā/ is irreducible (cf. *ḥaṭṭā*’ēṽ, ‘sinners of’), which shows that the *qameš* in this category of nouns represents historical /ā/. This leads to the conclusion that all such words were relatively late Aramaic loan-words into Hebrew, a conclusion that historians of the Hebrew language and of Israelite culture need to consider, either to refute or else to examine for its implications for Hebrew antecedents. Whatever the upshot of that may be, the weight of the evidence, I believe, favors the hypothesis according to which the Hebrew /qattāl/ forms had a /qattāl/ base.⁵⁹⁸

— p. 264 (§51.44l), p. 478 (§73.431c). In the first paragraph cited *lmd*, ‘Lehrling, Schüler,’ is identified as a /qu/ittū/ formation, with the alternative of /qattāl/; in the second, it is classified as a substantivized participle, though it is not specified whether the participle was active or passive. The semantics of the word would lead one in the direction of a passive form, but the /qattāl/ base normally denotes active occupations, so T.’s thinking here is not quite clear. As so often in such cases, the different categorizations are not cross-referenced.

— p. 265 (§51.44m), pp. 282-84 (§52.4). Unmentioned in either the paragraph on *nmrt*, ‘heller Glanz,’ or in the section on the feminine morpheme {-y} is the word *nmry* in RS 18.113A+B:9, which some scholars interpret as cognate with *nmrt*⁵⁹⁹ rather than as designating Amenophis III.⁶⁰⁰ More plausible, however, than taking *nmry* as a feminine

⁵⁹⁷ Cf. Pardee, *Semitica* 49 (1999) 41.

⁵⁹⁸ The retention of the *qameš* in plural construct forms appears to me to outweigh the datum that may be taken to indicate that the proto-vowel was short, viz., the fact that some of these forms show a m.s. construct form with *pataḥ*. The development of an analogical construct form (i.e. like /dābār/ → /d^obar/ in construct) appears a more likely secondary phenomenon than the retention of *qameš* in an open pro-pretonic syllable, something that occurs regularly only in this class of nouns and in the active participle of hollow roots (e.g., *qāmē*^y YHWH, ‘those who rise up against the Lord’). See now Fox, *Semitic Noun Patterns* (2003) 258-59, who, like Tropper, prefers to derive the Hebrew *nomen professionalis* from the /qattal/ base, and my discussion of the problem in my review of Fox’s work, forthcoming in *JNES*.

⁵⁹⁹ Rainey, *JAOS* 94 (1974) 188; van Soldt, *BiOr* 40 (1983) 693; idem, *UF* 22 (1990) 345, n. 164; idem, *Studies* (1991) 88; Dijkstra, *HUS* (1999) 158; Singer, *HUS* (1999) 678; Pardee, *Context* III (2002) 104 n. 126.

⁶⁰⁰ Virolleaud, *CRAI* 1955, p. 75; idem, *PRU* V (1965) 15. In 1962, Liverani expressed his dubiety (*Storia di Ugarit nell’età degli archivi politici* [StS 6; Rome: Pontifical Biblical Institute, 1962] 28, n. 6) regarding Virolleaud’s Egyptian interpretation of *nmry*, but in his article on the history of Ugarit in the *Supplément au Dictionnaire de la Bible* (vol. 9 [1979] 1298) he judged the identification probable. Over the years, Virolleaud’s interpretation has been predominant; the prestige of van Soldt and Singer may swing the balance in the other direction. The problem in the identification with Amenophis III is the very existence of a letter in Ugaritic that dates to the early fourteenth century. Other than the phrase here under discussion, nothing in the letter bears resemblance to other correspondence with Egypt (one group of interpreters takes

by-form of *nmrt*—because *nmrt* is very likely an Akkadian loan-word into Ugaritic—would be the analysis of the {-y} as the enclitic morpheme attached to a masculine by-form of *nmrt*.⁶⁰¹ The phrase is *rgmt ... nmry mlk* ‘Im, ‘I (hereby) pronounce (to various deities) the splendors of (your) eternal kingship’ (perhaps something like /ragamtu ... namurriya mulki ‘ālamī/).⁶⁰² The use of enclitic -y in a construct chain is well illustrated by *ily ūgrt* in RS 15.008:4-5 (on which, see above, note 88).

— p. 265 (§51.45a). In addition to the /ʾ/-preformative proper nouns listed here (*ʾqht*, *ʾrwd(n)*, *ʾdḏd*, and *ʾaḥnapu*), the Ugaritic for ‘Ashqelonite’ is now attested: *ʾtqlny* in RS 94.2392+:13 (unpublished).

— pp. 266-68 (§§51.45e-i). T. illustrates that wisdom is the better part of valor by not attempting to classify /maqtal/ vs. /miqtal/ according to precise semantic categories. No amount of reconstruction allows the simple retrojection of the Arabic situation, where /miqtal/ tends to denote instruments and /maqtal/ places, into earlier West Semitic. That said, most of the classification of given Ugaritic words into original or secondary /maqtal/miqtal/ categories is, as we have seen to be the case with other noun types, based on comparative evidence and, hence, essentially arbitrary.

— p. 266 (§51.45e). One may doubt that {mīḥd[...]} in RS 11.779:1 (*KTU* 4.81) is a common noun meaning ‘port’ while {māḥd} is the same word used as a proper noun: {mīḥd} may be the place name in its basic form /maʾḥad-/, while {māḥd} may show the variant pronunciation /maʾaḥad-/ (see above, remark to pp. 33-35 [§21.322.1]).⁶⁰³ There is, in any case, no obvious reason why {mīḥd[...]} in RS 11.779:1 should not be a place name.

— p. 266 (§51.45e). The indication “[he. *maṣṣāb*]” after the definition “eine Weinart” for Ugaritic *mṣb* is misplaced, for Hebrew /maṣṣāb/ is not attested for this meaning, but only for the first meaning of Ugaritic {mṣb}, viz., “Ständer, Gestell,” i.e., there is no Hebrew word /maṣṣāb/ that designates a type of wine. Indeed, the etymology of *mṣb* as a designation for a type of wine is uncertain⁶⁰⁴ and classing *mṣb*, “eine Weinart,” here without a question mark is misrepresentative of the present situation in Ugaritic lexicography.⁶⁰⁵

it in that sense), and there is no reason to suspect that a Ugaritic official writing to the king of Ugarit from Cyprus (the other principal interpretation) would bless Amenophis III or bless his king by Amenophis III.

⁶⁰¹The most appealing candidate semantically speaking is *namrīrrū*, a *plurale tantum* meaning “supernatural, awe-inspiring luminosity” (*CAD* N₁, pp. 237-38), “schreklicher Glanz” (*AHW*, pp. 728-29); but the Ugaritic form cannot, of course, have been identical since it is written with only one {r}.

⁶⁰²This explanation appears more plausible also than Rainey’s suggestion (reference note 599), offered without translating the passage, to take the -y as the *nisbe* ending: ‘I pronounce ... the glorious one ...’ does not appear to fit the context of an epistolary blessing.

⁶⁰³Van Soldt has demonstrated that {mīḥd} and {māḥd} are variant forms of the same place name (*UF* 28 [1996] 675-76).

⁶⁰⁴Pardee, *Les textes rituels* (2000) 515-18.

⁶⁰⁵T. himself once recognized the fluidity of the situation, mentioning an explanation based on a I-w root (*Kausativstamm* [1990] 176-77).

— p. 266 (§51.45e). The entry for *mšbt*, “Griff,” should be moved up from the I-N root section to the strong root section.

— p. 266 (§51.45e). It is the verb NTK, ‘to pour’, that is attested in RS 1.003:12 (*KTU* 1.41), not the noun *mtk*, ‘libation’. In the ritual texts, the latter is attested only in RS 24.266:25' (*KTU* 1.119).

— p. 266 (§51.45e). Though one may applaud the use of two question marks, T.’s classification of {*mr̥*} in RS 17.139:30 (*KTU* 2.34:32) as a *m*-preformative noun from WRT̥, ‘to inherit’, cannot be accepted. The text reads {(30) hn . mr̥ . d . štt (31) āššū . b . ḏdtk}, and the two lines clearly constitute a syntactic unit of which the first line cannot mean “the estate is ... settled,” as Dijkstra has proposed—the interpretation accepted here by T.⁶⁰⁶ If the reading of the last word in line 31 be accepted (see above, remark to p. 257 [§51.41h]), the reference is to ‘jars’ and *mr̥* must, as most scholars have believed, refer to some kind of agricultural product. The translation would be: ‘Behold, the *mr̥* which you stipulated, I will have (it) sent out in your *dd*-jars’. Below, p. 589 (§74.622.3), T.’s translation by “Most,” though in contradiction with the entry just discussed, better represents this consensus. It is highly unlikely, however, that *mr̥* designates ‘must’ in the narrow sense of the word.⁶⁰⁷

— p. 267 (§51.45e). Why is a triconsonantal base not proposed for each of the hollow-root substantives “/*maqâm*/,” ‘place’, “/*maqâr*/,” ‘spring’, “/*mahânat*/,” ‘emplacement’, and “/*marâmat*/,” ‘height’? The circumflex indicates contraction (see above, seventh general remark), and T. usually does not hesitate to indicate the hypothetical earlier form.

— p. 267 (§51.45e). It is clear from several passages in the letters that the meaning of *mlâkt* is not abstract, as T.’s translation “Sendung” might lead the unwary to believe, but concrete, as the *m*-preformative indicates.⁶⁰⁸ It denotes the party with whom a messenger (*mlâk*) traveled. This is clear from the very text that T. cites as an example of *mlâkt*. RS 15.098:11-14 (*KTU* 2.17:4-7) reads {w ht . lûk ‘m ml[âkty] (12) pğsdb . šmlšn (13) w tb‘ ank-- (14) ‘m mlâkth šm‘h}, ‘And now PĞSDB the ŠMLŠN has been sent (to you) with [my] messe[nger-party], and I am leaving with his messenger-party. Listen to him.’ *lûk* is a passive participle (/la’ûku/) used predicatively to designate a person sent along with the message inscribed on the tablet RS 15.098 (unfortunately, the name and the title are both obscure).⁶⁰⁹ The author of this letter plausibly designated the group with whom his messenger traveled ‘my messenger party’, though that cannot be certain because the end of the word is destroyed. But his reference to his own departure with PĞSDB’s party has been preserved.

— p. 267 (§51.45e). From an epigraphic perspective, it is truly astounding to find two readings of the same word in the same text within twelve lines on the page of each other, with no indication of preference: {*mḥrtt*} and {*mḥrt*} are both listed as occurring in RS

⁶⁰⁶ *UF* 19 (1987) 47.

⁶⁰⁷ Pardee, *Topoi* 11 (2001) 678-79.

⁶⁰⁸ My dictionary indicates two primary meanings for *Sendung*, “consignment,” and “mission.” Neither of these fits the usages of the Ugaritic word, as we shall see.

⁶⁰⁹ It is equally clear from this and other passages that *mlâkt* does not simply mean “message” (Watson, *JNSL* 25/2 [1999] 1-2, n. 2).

6.028:3 (*KTU/CAT* 6.14).⁶¹⁰ The reading {mḥrtt} is, in my estimation, beyond doubt⁶¹¹ and {mḥrt}, which would be attested only in this passage, has, therefore, no place in a grammar of Ugaritic. Moreover, the meaning of “Ackerland” proposed by T. for *mḥrtt* does not fit RS 6.028 and, for that reason, we proposed that the word means ‘plow’ in the stele inscription, as opposed to ‘plowland’ in the mythological text where it is attested.⁶¹² If these interpretations be correct, it may not be judged certain that *mḥrtt*, ‘plow’, and *mḥrtt*, ‘plowland’, were both /maqtal(a)t/ forms, i.e., the semantic distinction may have been expressed by different vowel patterns.

— p. 269 (§51.451). Because T. usually indicates in parentheses the proto-root of a Ugaritic form that contains a secondary consonantal element, one is surprised that the root of Ugaritic *mdb* is indicated simply as “√*dw*b” (cf. Hebrew ZWB).

— p. 269 (§51.451). One is also surprised to see T. proposing a single etymology for Ugaritic *mšmt*, ‘treaty’, viz., √*ŠMM*, citing Arabic. Others have thought the word to be derived from √*SMD*, with assimilation of the third root consonant to the feminine morpheme, something like /mašmadtu/ → /mašmattu/.

— p. 269 (§51.45n), p. 370-71 (§63.32), p. 477 (§73.427), p. 570 (§74.423). T. misses one of the key elements of the structure of RS 2.[003]⁺ i 12-21 in taking {mṭltt} (l. 16) as the first D-passive participle of the passage. The entire sequence depends on {mtrḥt}, l. 13, which, because the G-stem of TRḥ in Ugaritic means ‘to marry’ (said of the bridegroom) and the D-stem ‘to marry off’ (said of the father), is plausibly analyzed as a D-passive participle, i.e., ‘she who has been married off (by her father)’.⁶¹³ On p. 477, T. presents the possibility that {mtrḥt} is a G-passive participle, but prefers the analysis as a /maqtāl(a)t/ formation, citing Akkadian *marḥītu* as the basis of this reconstruction. That comparison misses the point of the Ugaritic lexeme, however, which appears to be a denominal verb from a noun such as Akkadian *terḥatu*.⁶¹⁴ As such, it will have had its own internal usages independent of Akkadian formations built from the root *reḥû*. Thus the sequence *mtrḥt* ... *mṭltt* through *mšb‘t*, with the number ‘two’ implied but not expressed, describing *Kirta*’s seven brides, is solidly built on a sequence of morphologically identical forms (/mutarahḥatu/ ... /mutallatu/ ... /mušabba‘atu/).⁶¹⁵ (On the dubious reading of *ṭnt ūm* in line 15 and its contribution to the

⁶¹⁰In *KTU*, one finds {mḥrt}, in *CAT* {mḥrṭlt}.

⁶¹¹Bordreuil and Pardee, *Semitica* 41-42 (1991-92) 24-32; Pardee, *Les textes rituels* (2000) 396-99; see now the new photograph and copy in Bordreuil and Pardee, *Manuel* (2004), text 15 in the *Choix de textes*.

⁶¹²On the contrast between RS 6.028 and RS 2.[009]⁺ iv 1-3, 12-14 (*KTU* 1.6), see Bordreuil and Pardee, pp. 29-32 in the *Semitica* article cited in the previous note.

⁶¹³Pardee, *Context I* (1997) 333, nn. 6, 8; idem, *JNES* 61 (2002) 124-25; Bordreuil and Pardee, *Manuel* (2004) II 20-21, 206.

⁶¹⁴TRḥ, ‘to marry’, is clearly not a recent borrowing from Akkadian, for the {ḥ} shows it to be an old West-Semitic root (a recent borrowing would show {ḡ}). On the other hand, its meaning makes it difficult to separate it etymologically from Akkadian *terḥatu*.

⁶¹⁵Despite citing the history of the interpretation as a passive participle (*UF* 31 [1999] 146, 156) and being aware of my structural interpretation of the passage in *Context I* (reference note 613), Dietrich and Loretz adopt another structural interpretation without bothering to refute mine (*ibid.*, pp. 133-64). Because their

misapprehension of the structure of this section, see remark below to p. 364 [§63.112], etc.).⁶¹⁶

— p. 269 (§51.45q-s). One must ask oneself what /naqtāl/, the base of the N-participle, /naqtāl/, the base of the N-infinitive, and /šVqtVl/, the base of Š-stem deverbal nouns, are doing under the heading “Bildungen mit *m*-Präfix” (pp. 266-69 [§51.45e-s]). For example, T. says here simply that “*naqtāl* ... dient zur Bildung des N-Ptz.” and only when one checks the appropriate paragraph (§74.35 [p. 541]) does one learn that one might expect “auf der Basis des sprachvergleichenden Befundes” that the base might have been “*munqatil*,” but that “*naqtāl*” is “wahrscheinlicher” for Ugaritic. At the very least, the statement on p. 269 should have been qualified in terms of the preferred explanation on p. 540; however, since there is no evidence from Ugaritic for *m*-preformative forms of the N-stem, paragraphs q and r might better have been eliminated entirely. (On the question of the vowel length in the N-participle, see remark below to pp. 540-41.) The problem of Š-deverbal nominal forms without *m*-preformative is a very different one, for one finds nouns without the *m*- (discussed in §74.626.3, pp. 600-2]) and participles with the *m*- (discussed in §74.625, pp. 599-600). Strangely enough, in §51.45s, T. refers only to the former, not to the latter.

— p. 270 (§51.45u). Given that the form in Massoretic Hebrew that corresponds to Ugaritic {tânt} has a strong /ʔ/ (the Hebrew form is *taʾaniyyāh*), it must be considered highly unlikely that {tânt} represents /tânî-/ , i.e., that the /ʔ/ has quiesced and the {â} is used as a *mater lectionis*, as T. proposes here. It is equally unlikely that the by-form {tunt} represents the /aʔ/ → /ô/ shift, with the {û} here also used as a *mater* (on this subject, see above, remark to pp. 33-35 [§21.322.1]).

— p. 270 (§§51.45v, w). Though there are not a few redundancies in the sections on noun bases, I can see no reason for listing {tmn}, “Gestalt,” twice, first as /taqtul/, then as /taqtVl/.

— p. 270 (§51.45w), p. 432 (§73.223.1). In the first paragraph cited, *tbšr* in RS 17.072:1 (*KTU* 6.24) is classified as a {t-} preformative noun meaning “Kontrolle,” in the second as a proper name of which the {t-} is perhaps the 3 f.s. /YQTL/ morpheme.

— p. 270 (§51.45w), p. 722 (§77.322b), p. 740 (§81.12e), p. 830 (§89.24e). In RS 29.095:9 (*KTU* 2.71) {tšmʿm} is more plausibly verbal than a *t*-preformative noun, as T. proposes, once explicitly rejecting the interpretation as a verb (p. 830), twice considering the latter analysis as an alternative (pp. 270, 722). The passage reads: {(9) hnk . tšmʿm (10) ʿdn . yštāl (11) ʿmnk . pm yqh (12) bk . p ʿpr}, which may be interpreted: ‘Now listen well: (As) ʿDN has been continually requesting of you, so he may take a *bīku*-jar (of wine), by permission of ʿPR’.⁶¹⁷ In two of the paragraphs cited, T. proposes a translation of lines 9-10, both times with a question mark and ellipsis points: “Er soll sich dort nach Gerüchten ... (?) erkundigen” (p. 740) and “dort soll er ... (?) sich nach Gerüchten erkundigen” (p. 830)—

interpretation passes over without comment what I consider to be the key to the structure of the passage (/mutarraḥatu ... muṭallaḡatu .../), I do not find it plausible.

⁶¹⁶Cf. Pardee, *JNES* 61 (2002) 124-25 n. 4.

⁶¹⁷Pardee, *Context* III (2002) 111.

he does not attempt an interpretation of the passage as a whole.⁶¹⁸ His interpretation of line 12 is sapped at the base by the false reading {t‘pr} (see above, remark to p. 139 [§33.112.36]). He correctly interprets {pm} in line 11 as the conjunction *p* + enclitic *-m*, though with a great deal of hesitation (p. 788 [§83.12], p. 790 [§83.123b], p. 832 [§89.29]—see remark below to p. 788), but he makes no attempt to discuss how this conjunction, of which the function is to mark a cause-and-effect relationship, links the preceding words with the following.

— p. 270 (§51.45w), p. 444 (§73.233.4), p. 645 (§75.522). Though the word {tīntt} (RS 2.[004] vi 40' [KTU 1.17]) is valuable in that it shows explicitly that the Ugaritic word for ‘woman’ is from the root ʾNT, it is hardly plausible that it is the plural of {ātt}, as T.’s translation “Frauen” (p. 270, with a question mark) indicates his analysis to be (in the other two paragraphs cited, he leaves open the possibilities of {tīntt} being a singular or a plural). As most *t*-preformative nouns are abstracts, the first assumption must be that the word is singular and that it means something on the order of ‘womankind’.⁶¹⁹ T. lists the form under /taqtVI/ but does not propose a specific vocalization. Because of its frequency in the Semitic languages, /taqtilat/ is the most plausible, but all that one can say with some certainty is that the writing with {i} makes it plausible that the first syllable was closed.

— p. 270 (§51.45x). If one is to compare Hebrew *tīyrōwš* with Ugaritic {trt}, as does T. here, one must conclude that the base form was not /taqtāl/ but /tiqtāl/ and that the Ugaritic word was not “/têrāt/” but /tîrāt/. The Eblaite form {ti-rî-šu} that T. cites is, however, closer to {DINGIR.SIRIŠ} the Sumerian equivalent of Akkadian *Sirāšu*, with which the Ugaritic divine name *trt* is equated in RS 94.2004:25.⁶²⁰

— pp. 270-71 (§51.45z). One fails to understand why, when in his introduction to this paragraph T. proposed that *t*-preformative nouns from III-y roots are either /taqtīy-/ or /taqtiyy-/, he proposes only the vocalization “/tôšīyat/” for {tšyt} when the Hebrew tradition shows *tūwšīyyāh*.

— p. 271 (§51.45a'). T. gives no explanation for treating “*tagabbir*” under the heading /tuqattil/.

— p. 271 (§51.45e'), p. 609 (§74.645), cf. p. 671 (§75.537g), p. 868 (§93.352). Because of the absence of reference to cultic prostitution elsewhere in Ugaritic literature, a translation of *mšt'ltm* in RS 02.002:31 (KTU 1.23) less explicitly sexual than “(zwei) Prostituierte/Dirnen” is preferable. On the other hand, recognizing the non-divine nature of these ‘women’ is, in my estimation, plausible, indeed to be preferred.⁶²¹

— p. 271 (§51.46a). The context of {[...]rhbn} in RS 16.394:67' (KTU 2.31:63) is too damaged for us to know whether it is an adjective meaning “weit, breit,” the river name *Rahbānu*, or, for that matter, a verbal form. Contrary to T.’s opinion, the editors of RS

⁶¹⁸On pp. 525 (§74.232.21) and 571 (§74.432), he proposes tentatively that {‘dn} is a proper name—it is unclear why he does not work that interpretation into the translations cited.

⁶¹⁹Pardee, *Context I* (1997) 347; Parker *apud* Parker, ed., *Ugaritic Narrative Poetry* (1997) 62; cf. del Olmo Lete and Sanmartín, *Diccionario I* (1996) 460 (“el género femenino, las mujeres”).

⁶²⁰Pardee, *Les textes rituels* (2000) 796, 803 (reading of RS 94.2004 thanks to D. Arnaud).

⁶²¹Cf. Pardee, *Context I* (1997) 280 with notes 44 and 45.

92.2016 (RSO XIV 53) take {rḥbn} in line 18' (cf. CAT 9.432:18—mistakenly cited here as occurring in line 17) as the river name.⁶²²

— p. 271 (§51.46a), p. 823 (§89.11a), p. 853 (§92.232b). The reading of the last word in RS 2.[004] i 16' (KTU 1.17) has caused no end of trouble. The editor read {ābynt}⁶²³ but on his copy marked the end of the last sign as damaged; Herdner followed this reading but in a footnote pointed out the difficulties associated with it.⁶²⁴ The authors of KTU/CAT read {ābyn ṯāt} while T., citing J.-P. Vita as collaborator in the collation, has read {ābynt} or {ābynn!}.⁶²⁵ The latter reading appears to have won his fancy because only it is cited in this grammar, i.e., the form would be ābyn + enclitic -n. No certain reading is attainable today because the middle of the horizontal form in the clay is lost where damaged has occurred between the two vertical lines of which the column divider is formed.⁶²⁶ Thus both {n} and {āt} are theoretically possible readings (the head of each wedge after the first would have been placed exactly on one of the vertical lines), while {t} appears unlikely because of the total length of the horizontal form. Because the 2 m.s. independent pronoun appears to be out of place here, the reading as {ābynṯn!} appears preferable. The notation should, however, be that just given, not {ābynn!}, because the final sign was not written faultily by the scribe, it has only suffered damage since the tablet was inscribed.

— p. 272 (§51.46d). T. classifies ḥṯbn, 'account', as /qutlān/ on the basis of Arabic /ḥusbān-/ without mentioning that Hebrew /ḥešbōn/ might favor /qitlān/ (depending on one's view of the origin of the *seghol*).

— p. 272 (§51.46e). The reader deserves an explanation for why Hebrew *šikkārōn* is cited to establish the vocalization /šakarān-/ for Ugaritic.

— p. 272 (§51.46e). Equally mysterious is why Hebrew *ʿaqallātōn* is cited in favor of the vocalization /ʿaqalatān-/ for the corresponding Ugaritic word. If one grants that the /l/ may have been secondarily doubled in Hebrew, why did not one of the short vowels in the Ugaritic form disappear through syncope?

— p. 273 (§51.46g), p. 741 (§81.13a), cf. p. 148 (§33.115.52) and p. 300 (§53.331.4). In the first section cited, T. translates {ḥ[lpn]} in RS 3.322+ iv 44 (KTU 1.19) as "Mantel" (and does not note that the word is almost entirely reconstructed), in the second, he translates by "D[olch]" (noting the reconstruction in the transliteration as well as in the translation). On p. 300, the plural of the word in other texts is translated "Umhänge" and on p. 148 the word is compared with Akkadian *ḥulāpu*, "Decke," and *naḥlaptu*, "Gewand, Mantel." An explanation for the aberrant translation on p. 741 would have been in order.

⁶²² Caquot and Dalix, RSO XIV (2001) 397.

⁶²³ Virolleaud, *Danel* (1936) 186.

⁶²⁴ CTA (1963) 80 n. 3.

⁶²⁵ *AuOr* 16 (1998) 292.

⁶²⁶ What looks like a very short horizontal wedge appears just to the right of the second vertical line on the photograph published in the *editio princeps* (see note 623) and reproduced in CTA; it is still visible on the tablet today. Its length appears, however, insufficient for it to be qualified as a full wedge and the appearance of a head of a wedge must be owing to a grain of sand that was part of the temper of the clay.

— p. 273 (§51.46g). The word *gpn* is generally taken as denoting some part of a riding animal's paraphernalia, not the animal itself (T. translates "Reittier").

— pp. 273-74 (§51.46h-k). On the *nisbe* ending as /-Vyy-/ , rather than /-īy-/ , see above, remark to p. 197 (§33.322.42c). T.'s main presentation is here, but he assumes /-īy-/ throughout the grammar. As regards the assertion in the introduction to this paragraph that Ugaritic had two suffixal morphemes, one /-īy-/ , the other /-āy-/ , it is no doubt correct. It is necessary, however, not to confuse the function of the two endings: /-iyy-/ (or /īy/ , as T. represents it) is the only form clearly attested as a gentilic morpheme, whereas /-āy-/ , if such be the correct vocalization, is only attested as the final syllable of geographical names, not gentilics therefrom, as van Soldt makes clear in the reference cited here by T.⁶²⁷ A given geographical name may appear with and without this ending and van Soldt was not able to determine any functional difference between the two forms of a name. Though T. admits that only the form with /īy/ is attested in gentilics, he appears tempted to identify the /-āy/ ending with the Aramaic gentilic morpheme—which is perhaps historically correct, though the function of the ending is clearly not the same in Ugaritic and in Aramaic. It may be noted that T., no doubt correctly, vocalizes most non-gentilic substantives with {-y} affixed to the stem as bearing the morpheme that corresponds to the gentilic morpheme.⁶²⁸ Thus, in spite of his statements that both suffixes may function as *nisbe* endings and that a certain distinction between the two is not possible, he in practice subscribes for the most part to the distribution that the available data suggest to be correct.

— p. 273 (§51.46h). As perceived by van Soldt,⁶²⁹ {tmrym} in RS 14.084:3' (*KTU* 4.126:20) refers to persons from the town of {tmr} , not to "Dattelpalmenzüchter, -bearbeiter," as T. translates here.⁶³⁰

— p. 274 (§51.46k), pp. 282-83 (§52.41). T.'s position on the nature and vocalization of the morpheme {-y} of *n'my* , 'good one (a substantive in apposition with a feminine noun)' is obscured by inconsistent presentations: on p. 274, the vocalization /nu'may/ is classified under the heading /qutlāy/ , whereas, on pp. 282-83, the form is explained on the basis of the Arabic feminine elative /fu'lāy/ but vocalized /nu'may/ , with a cross reference to §51.46h said to present the base "{qutlay}." In addition, on p. 283, the Hebrew proper name *no'omīy* is compared with the Ugaritic word, in spite of the fact that /-īy/ is probably not there a feminine morpheme (the name is more plausibly a hypocoristic for *no'omīy* + DN). The confusion seems to have arisen from the fact that Arabic has a feminine morpheme /-ay/ that

⁶²⁷ *UF* 28 (1996) 653-55.

⁶²⁸ On the one putative example of the /-āy/ ending in syllabic writing, see above, remark to p. 85 (§23.523), etc. On the basis of this example, T. classifies the feminine form of the word {ūhry} as bearing the /-āy/ ending ("ūhṛāyat"), a logical enough conclusion given the postulate regarding {ūhry} . But, with no indication in the Ugaritic data, he also classifies {n'my} here on the basis of Arabic /nu'māy/ (it is, however, uncertain why he vocalizes the Ugaritic word with a short /a/: "/nu'may/" — on this word see further below, remarks to p. 274 [§51.46k], etc., p. 276 [§51.5c], etc., and pp. 282-84 [§52.4]).

⁶²⁹ *UF* 28 (1996) 689 + n. 295; cf. *UF* 30 (1998) 743.

⁶³⁰ See my re-edition of the text in *Semitica* 49 (1999) 59-64.

is not limited to a single base form like /fu^ʿlāy/ (see below, remarks to p. 276 [§51.5c] and to pp. 282-84 [§52.4]).

— p. 274 (§51.48a). The absence of question mark or alternative proposal attached to the interpretation of the word *mhbn*, attested only as the second element of the compound divine name {ršp mhbn}, as derived from the root WHB (no gloss provided) may not go unchallenged. Not only have other plausible proposals been made, but there is presently no clear criterion for choosing among the various possibilities.⁶³¹

— p. 275 (§51.5a). If *drdr*, ‘very long time’, is a “/qVlqVI(l)j” form, it cannot have been “*dârdâr*,” for the vowel in the first syllable would have shortened because the syllable is closed (see remark above to p. 252 [§51.41a], etc.). If such a form had existed in proto-Hebrew, it would have become /dardôr/, which is not, of course, the case. Unless Ugaritic went its own way on this, it is more likely, therefore, that {drdr} represents simply two nouns in construct, either /dāru dāri/ (or /dawru dawri/ → /dôru dôri/), as in proto-Hebrew, or with different bases, as in Akkadian (*ana dāri dūri*). I see no reason to doubt that Aramaic *dārdārīn* is a relatively late form that arose after the loss of case vowels and at a time when long vowels had come to be tolerated in closed syllables (that would plausibly be after vowel length had ceased being phonemic). As regards the representation of the vowels in the Ugaritic form as showing contraction, one would like to know what the proto-Ugaritic elements were that contracted to /â/. (No such forms are listed above, p. 252, where /qatl/ nouns from II-w roots are presented; on the general problem, see above, seventh general remark.)

— p. 275 (§51.5b), p. 301 (§53.34), p. 301 (§53.35), p. 333 (§54.423d), p. 627 (§75.42), p. 661 (§75.532). These paragraphs contain a glaring inconsistency in the treatment of {‘r‘rm} and {ssnm} in RS 24.244:65, 66 (*KTU* 1.100). Because these forms are paralleled in the following lines by feminine forms bearing enclitic {-m} ({‘dtm} and {ybltm}), and because {‘r‘r} appears in the singular in line 64, it is likely that both {‘r‘rm} and {ssnm} are also to be analyzed as singulars with enclitic {-m}.⁶³² In the first three paragraphs cited, however, T. explicitly analyzes {ssnm} as a plural;⁶³³ in the fourth, he transcribes “*ssn-m*” and translates this and the three parallel terms in the passage as singulars; in the fifth and sixth, the verb, respectively in emended and unemended forms (see above, remark to p. 58 [§21.352.2 {y} für {h}], etc.), that modifies {ssnm} is parsed as a singular (as is to be expected in this grammar since it is {y-} preformative). None of the four forms in question is cited in the section on enclitic {-m} (pp. 825-32 [§89.2]).

— p. 276 (§51.5c), p. 282 (§52.4), p. 842 (§91.242a). Comparison of these three paragraphs reveals a muddled and ultimately incorrect presentation of the poetic pair *n‘my//ysmt/ysmsmt* in RS 2.[022]⁺ vi 5-7 (*KTU* 1.5) and RS 2.[004] ii 41'-42' (*KTU* 1.17). In the first paragraph *ysmsm-* is presented as nominal, in the second *n‘my* as adjectival, and

⁶³¹Pardee, *Les textes rituels* (2000) 580.

⁶³²I have argued the point explicitly in *JANES* 10 (1978) 95; cf. *Les textes para-mythologiques* (1988) 202, 203, 216-17. This or a similar analysis is accepted by recent translators, e.g., Parker *apud* Parker, ed., *Ugaritic Narrative Poetry* (1997) 222; Wyatt, *Religious Texts* (1998) 385.

⁶³³In the second, {ssnm} is cited as a type example of “Plural als Grundnumerus.”

in the third *ysmt* is said not to show agreement with *šd* ‘field’ in the first text cited. The correct view, recently presented in some detail by Loretz,⁶³⁴ is that *n‘my* is a noun, not an adjective (at least in all attested cases in Ugaritic the substantive in question functions as a noun⁶³⁵); because the /qataltal/ formation is basically adjectival while *ysm* could be either nominal or adjectival, both forms should probably be classified as adjectival and both are in these two passages, therefore, substantivized adjectives (on this topic, see remarks to p. 261 [§51.43d], etc., and p. 419 [§69.43]). The phrases *n‘my ʾrš dbr // ysm̄t šd šhl mmt* in the first passage are to be translated literally ‘the goodness of the land of pasturing // the beautifulness of the field on the edge of death’s realm’⁶³⁶, while in the second passage *n‘my ʾrš // ysm̄smt ʾrš* may mean ‘the goodness of the bed // the beautifulness of the bed’.⁶³⁷

— p. 277 (§51.62). The different bilabials in Ugaritic *ʾlgb̄t* and Akkadian *algamišu*, both of which designate a type of stone that has yet to be defined with certainty, suffice to indicate that the Ugaritic word may not have been borrowed directly from Akkadian, as T. proposes here.⁶³⁸

— p. 277 (§51.7). The morpho-syntactic analysis of RS 02.[008]⁺ vii 43 (*KTU* 1.4) does not require that {blmlk} be taken as a compound noun meaning “Nicht-König; gewöhnlicher Mensch.”⁶³⁹

— p. 277 (§51.7). T. here analyses {šm‘rgm} in RS 14.176:3 (*KTU* 4.128) as a compound noun meaning “Auditor.” Another text shows that the phrase corresponds to a title for one of the functions in the royal service corps: in RS 19.016:10, 11 (*KTU* 4.609) a {šm‘rgm} and a {šm‘} (abbreviation or mistake for {šm‘rgm}?) are listed among other *bnš mlk*. RS 92.2010:18-19 (RSO XIV 50) {šm‘ rgmk | n‘m}, ‘the one (or those) who hear(s) your good word’, where the adjective modifies the second element of the preceding phrase, shows that the two words were perceived as independent entities. The morpho-syntactic characteristics of this new attestation (which T. does not cite despite having it at his disposal)⁶⁴⁰ appear to outweigh the absence of a word-divider between the two parts of the phrase in deciding whether we are dealing with a true compound noun or two nouns that happen to occur together because they express a particular function.⁶⁴¹

⁶³⁴ *UF* 33 (2001) 303-24.

⁶³⁵ An important example omitted by Loretz, because he does not, like T., analyze the form as a feminine noun, is *mr̄hy* (see remarks to p. 52 [§21.341.21b], etc., and pp. 306-7 [§51.121.2a]).

⁶³⁶ For this interpretation of *šhl mmt*, see Pardee, *Context I* (1997) 267 n. 231, 234.

⁶³⁷ The uncertainty arises from the broken context. This analysis was proposed in *ibid.*, p. 345 n. 22.

⁶³⁸ Pardee, *Syria* 77 (2000) 37-38.

⁶³⁹ Pardee, *Context I* (1997) 263.

⁶⁴⁰ Curiously, the phrase is cited below, for its syntax (p. 841 [§91.23], p. 846 [§91.315.1]), but not here.

⁶⁴¹ {n̄gr krm}, ‘guards of the vineyard’ (the next entry in RS 19.016), or any of the other two-word titles for functionaries, are not included in this paragraph, apparently because T. does not consider them to be true compound nouns. Such should have been the classification of *šm‘ rgm*.

— p. 277 (§51.7). In the ²*Aqhatu* text (RS 02.[004] [*KTU* 1.17]), which T. cites as textual reference for the term {ilib}, that word does not appear as a divine name in the narrow sense of the word, for at each occurrence it bears a pronominal suffix and means ‘his/my father’s god’.⁶⁴² In the ritual texts, on the other hand, {ilib} does appear as a true divine name, both in deity lists and in the corresponding sacrificial texts,⁶⁴³ and designates the theogonic first principle.⁶⁴⁴

— p. 278 (§52.12), p. 284 (§52.5b), p. 288 (§53.121), p. 392 (§69.132), p. 397 (§69.142.2), p. 398 (§69.143.21b), p. 402 (§69.162.2b). T.’s remark that “die Abgrenzung von Nomina generis und Kollektiva [§53.12] ist in Einzelfällen schwierig” is certainly correct, though perhaps as much for lack of data as for any other reason. The distinction between the two for T. is that the former denotes a type (“Gattung”), the latter a group of beings or things. I would observe that, for nouns that do not have a singular but that may be numbered, it appears best to reserve the term “collective.” *šin* (Hebr. *šōʾn*), ‘mixed herd of sheep and goats’, is a paradigmatic example of a collective in this sense, but it is classified by T. as a “Nomen generis” rather than as a collective. It is, in any case, attested in the morpho-syntactic construction most typical of collectives, viz., with a number noun but remaining in the singular (e.g., RS 1.005:6-7 [*KTU* 1.43] *š alp w tlt šin šlmm*, ‘a ram, a bull, and three caprovids (as) a *šlmm*-sacrifice’).⁶⁴⁵ T. observes on p. 392 that *š*, ‘male ovid’, is only attested in the singular and dual in administrative texts, while *šin* is used with plural number nouns. The sequence in the text just cited, i.e., with *š* and *šin* in the same brief sacrificial list, may indicate that in the ritual texts at least, where *š* also occurs only in the singular and the dual⁶⁴⁶ and *šin* is also used only with plural number nouns,⁶⁴⁷ *šin* does not designate male ovids in the narrow sense but any caprovid. Doubt about whether or not *šin* includes caprids arises primarily from the fact that the offering of a single goat is very rarely prescribed in these texts. The case of *alp*, ‘male bovid’, and *gdlt*, ‘female bovid’ (on this meaning of *gdlt*, see remark above to p. 253 [§51.41b], etc.), is not the same, for there is no collective term under which they are subsumed;⁶⁴⁸ *alp* is used in the singular, the dual, and the plural, *gdlt* in the singular and the plural.⁶⁴⁹ The word *lhm*, ‘(loaf) of bread’, is not

⁶⁴²Pardee, *Context I* (1997) 344, 345.

⁶⁴³References in Pardee, *Les textes rituels* (2000) 1111.

⁶⁴⁴*Ibid.*, pp. 296-99, 799.

⁶⁴⁵On p. 392, this noun is cited as an example of unexpected usages of the singular with number nouns from ‘three’ to ‘ten’—a surprising entry, since the noun is a paradigmatic collective in Hebrew as well as in Ugaritic.

⁶⁴⁶Pardee, *Les textes rituels* (2000) 1161-65.

⁶⁴⁷*Ibid.*, p. 1199—the only uncertain case is RS 24.250:12-13 (*KTU* 1.106), a damaged passage.

⁶⁴⁸The term that one would expect on the basis of Hebrew usage, *bqr*, is not attested in the ritual texts. The more common generic term *bhmt* for ‘cattle’ in Ugaritic is also absent from the sacrificial rituals (*ibid.*, p. 1122).

⁶⁴⁹*Ibid.*, pp. 1112-1114, 1128-29. There is no evidence that *alp* is used in prose as a collective like *šin*. In proof of the classification of *alp* as a “Nomen generis” (i.e., in the same paragraph with *šin*), T. cites one use of *alp* in a poetic text (where *KTU/CAT* read a plural!: 1.4 vi 40) and ‘*šr bmt alp mri*’ from an

included here, either as a generic noun or as a collective, but *ʿšr lḥm*, ‘ten (loaves of) bread’, in RS 6.216:3’ (*KTU* 4.34)⁶⁵⁰ shows this word to have been a collective in Ugaritic, as it was in Hebrew. Under the heading of “Kollektiva” (p. 288), T. groups two categories of nouns: (1) collectives in the narrow sense of nouns that do not have a plural,⁶⁵¹ and (2) nouns that semantically denote a group but that also have a plural form to designate a plurality of such groups (*ʿm*, ‘people’, *šbū*, ‘army’).⁶⁵² In Hebrew, at least, *ʿam*, ‘people, ethnic group’, can function either as a singular or as a collective when designating a single group (i.e., the singular form can take singular or plural modifiers) and it has a plural that is used to designate a plurality of ethnic groups (e.g., *ʿammēy hāʾāreš*, ‘the peoples of the earth’ [not ‘the people of the earth!’]). Though the data are fewer in Ugaritic, there is no reason to doubt that these two formal categories existed there, and it would have been useful to have kept them distinct. It would also have been useful to provide some explicit argumentations in favor of the identifications of the various nouns cited as generic or collective.

— p. 278 (§52.12), p. 285 (§52.5c). Translating *npš* as “Sklaven” without nuancing the presentation (p. 278) does not take into account texts where such is clearly not the meaning, e.g., *npš* in RS 18.031:20 (*KTU* 2.38) means ‘persons (on board the rescued ships)’.⁶⁵³ “Leute, Personal, Sklaven” (p. 285) better reflects the variety of usages. In many cases of *npš* meaning ‘individual(s)’ in the administrative texts, the reference is plausibly to *bnš mlk*, ‘royal personnel’, and how many of these were slaves in the strict sense of the term remains to be determined (though the status of the *bnš* mentioned on the text cited above, in the remark to p. 137 [§33.112.31], is clearly very low, the translation ‘servant’ appeared preferable to that of ‘slave’ because more neutral).

— p. 278 (§52.12b). It would have been appropriate to point out that in the text cited for *dbb*, “Fliege,” which is supposed to be a noun designating a “Tiergattung,” the word is a personal name (RS 19.018 i 7 [*KTU* 4.611]). That being the case, it would also have been appropriate to provide proof that the name is in fact derived from the animal name. This might have come from the syllabic spelling of the name, viz. {da-bu-bi} (*PRU* VI 70:2), for Hebrew shows /ū/ in the second syllable of this word (*zʾbūb*)—Aramaic and Arabic, on the other hand, have /ā/ in this position. (On the common noun *dbb*, see further above, remark to p. 101 [§32.142.32].)

administrative text (RS 16.399:17 [*KTU* 4.247]). The latter is, however, clearly a use of the word as a classifier to a numbered noun (*bmt* is to be parsed as a feminine plural: ‘ten *bmt*-cuts of fattened beef’).

⁶⁵⁰See Pardee, *Or* 70 (2001) 237-50.

⁶⁵¹T. categorizes *ḥzr*, an administrative term of uncertain etymology and meaning (cf. Pardee, *Semitica* 49 [1999] 57), as a collective, but then below classifies it with unexpected usages of the singular with number nouns from ‘three’ to ‘ten’ (p. 392 [§69.132]).

⁶⁵²Below, p. 294 (§53.312b), T. translates {šbim} as “Soldaten.” Does the text refer to multiple groups of individuals or to multiple individuals?

⁶⁵³For the general interpretation of this text, see comment above to p. 61 (§21.354.1c), etc.; for more detailed interpretations, Pardee, *Context* III (2002) 93-94; idem, *Les textes épistolaires* (in preparation).

— p. 280 (§52.222). The reading {mnḥt} in RIH 78/02:9 (CAT 4.771) cannot be considered new, as T. claims here, when the editors suggested it.⁶⁵⁴

— p. 280 (§52.222). It cannot be considered likely that *mnḥt* in RS 94.2392⁺:6 is a *nomen unitatis* of *mnḥ*, meaning ‘a single gift’ while *mnḥ* would mean ‘gifts’. More plausibly, the feminine form reflects an abstract, because (1) in this text it does not in fact designate a gift (‘ebony *d mnḥt*’ is exchanged for olive oil) and (2) it is structurally parallel with the masculine noun *mkr*, ‘merchant’. The terms *mkr* and *mnḥt* function, therefore, to define two sorts of ebony, either according to type (the types that are appropriate for ‘merchandise’ or for ‘tribute’) or in terms of their intended use (for ‘resale’ or for ‘tribute’). The appearance of *d mnḥt* in RS [Varia 13]:6 (KTU 4.709) without the parallel phrase *d mkr* leads to the conclusion that *d mnḥt* designates the purpose for which the article in question was acquired, i.e., for tribute to the king of Hatti.⁶⁵⁵

— pp. 282-84 (§52.4). Whether or not the noun *nʿmy*, ‘good one’, bears an /-āy/ suffix identical to the Arabic feminine elative /fuʿlāy/ (see remarks above to p. 274 [§51.46k], etc., and to p. 276 [§51.c], etc.), the more widely attested feminine morpheme {-y} should, as T. accepts, more plausibly be vocalized /-ay/, cognate to the Arabic feminine morpheme /-ay/. The only datum cited in favor of his reconstruction of the Ugaritic morpheme as “/-ayV/,” however, is the personal name {ṭá-la-ia}. No reference is made to the syllabic spelling of the divine name *Pidray* as {pi-id-ra-i} (RS 17.116:3 [PRU IV, p. 132])—that the spelling with {a-i} represents /ay/ is rendered virtually certain by the fact that the name is in the nominative case. One may explain the discrepancy between these two forms by the common tendency to ‘nominalize’ personal names, i.e., to attach a case vowel to the end of the name that may not reflect the morpho-syntactic structure of the name or the nature of the second component. Thus *Tallayu* would reflect this tendency in personal names, while the divine name *Pidray* would have retained the more archaic form without marking for case.

— p. 283 (§52.43). T.’s preference to take the {-y} on various feminine proper names as a hypocoristic ending rather than the feminine morpheme appears to be too broad: while such may well be the case with several of the personal names cited here, the syllabic writing of the divine name *Pidray* cited in the previous remark, but not included as an example in this list, indicates that that divine name at least contained the feminine morpheme. That being the case, the same is almost certainly true of *ṭly* and *ārṣy*, *Pidray*’s sisters, names that are included here. Almost as strong a case could be made for *ḥry*, the name of *Kirta*’s bride. Given the writing with {ḥ}, the name is more plausibly Semitic than Hurrian⁶⁵⁶ (one might expect a name with a phoneme represented in syllabic writing with {ḥ} to show up in Ugaritic with {ḡ}—see T., p. 125 [§32.146.313]). Her archetypal role in the story renders an archaic name form, on the pattern of the divine names just discussed, plausible (cf. *Šāray*

⁶⁵⁴Bordreuil and Caquot, *Syria* 57 (1980) 362. Cf. Pardee, *Syria* 77 (2000) 56-57.

⁶⁵⁵In the third occurrence of the sign sequence {d mnḥt}, which T. here takes as equivalent to the two just cited (the text is RIH 78/02:9, on which see previous remark), the phrase is plausibly verbal (see Pardee, *ibid.*, pp. 57, 60).

⁶⁵⁶Watson, *AuOr* 13 (1995) 222.

in the biblical Abraham story): *Hurray* would be ‘the noble lady’ just as **Šarray* was ‘the princess’.

— p. 283 (§52.43). If the purpose of this paragraph were to provide a list of feminine proper names bearing the feminine morpheme {-y}, one would query the inclusion of {tlgdy}, for one might doubt that this non-Semitic name would bear that morpheme. As T. actually prefers to take this element as a hypocoristic ending, however (see preceding remark), the inclusion is perhaps more plausible.

— p. 283 (§52.43). There is no explicit indication in the text of RS 92.2010 (RSO XIV 50) for the assumption that the name *nkly* (l. 15) was borne by a female.

— p. 284 (§52.44). T. prudently avoids taking a stand on whether the presence of {-y} in the Ugaritic writing of many place names—the same toponym often occurs in two forms, with and without {-y}—represents the feminine morpheme, but he does not say why he remains so prudent. One may surmise that it was because the personal names and divine names show /y/ in the alphabetic forms (see preceding remarks), whereas the most frequently attested syllabic orthography for what appears to be the same ending on place names is {a-a}, which van Soldt takes as an abbreviation of /-āyu/.⁶⁵⁷ It is more plausible, in light of the Arabic data, that the archaic feminine ending was /-ay/ (without a case vowel), that this ending and alternative forms were in more or less free variation among the scribes when writing place names. The alternative forms would have been (1) /-ay/ + case vowel (viz., /-ay + u/a/i/), (2) a form reflecting the dropping of the /-y/, which may be /-ā/ or /-a/, with preference for the former in light of the syllabic spellings,⁶⁵⁸ or (3) forms in which the old feminine morpheme without the case ending had been replaced by the nominal stem plus the regular case system, which could be triptotic or diptotic according to the structure of the place name (viz., simply /u/a/i/ or /u/a/ if diptotic). For example: {ūbr’y} would be /’ubur’ay/ or /’ubur’ayu/a/i/ while {ūbr’} may be /’ubur’ā/ (or /’ubur’a/) or /’ubur’u/a/i/ (the only syllabic spelling for this particular place name shows /-a/: {u-bur-a}).⁶⁵⁹

— p. 284 (§52.5c), p. 885 (§95.12). It is not at all “offenbar” (p. 284) that *yd* in RS 4.475:11 (*KTU* 2.10) is modified by a masculine adjective (see further below, remark to p. 330 [§54.423a], etc., where other elements of the passage are discussed). Indeed, I have argued that such is not the case at all.⁶⁶⁰ T. should have realized that something was wrong with his analysis when this passage provides the only example from prose or poetry of gender discord between a noun and a modifying adjective (p. 885—see further below, remark to this page). Also on p. 885, he proposes that the form may be masculine plural construct,⁶⁶¹ without explaining why divinities would have had many hands instead of just two.

⁶⁵⁷ Van Soldt, *UF* 28 (1996) 653-55; idem, *UF* 31 (1999) 773-75.

⁶⁵⁸ Cf. also the place name Jericho, in Hebrew *y’rīḥō^w*, perhaps ← /yarīḥā/.

⁶⁵⁹ Van Soldt, *UF* 28 (1996) 656.

⁶⁶⁰ “‘As Strong as Death’,” *Love & Death in the Ancient Near East. Essays in Honor of Marvin H. Pope* (eds. J. H. Marks and R. M. Good; Guilford, CT, 1987) 65-69. On the proper interpretation of this passage, see further below, remark to p. 330 (§54.423a), etc.

⁶⁶¹ Already above, p. 739 (§81.11e), p. 799 (§83.231b), p. 853 (§92.232a), he had translated “die ‘Hand’/‘Hände’” without providing the basis of the translation.

— p. 285 (§52.5c). T. takes the variants {t̥br} and {yt̥br} in RS 2.[014]⁺ iii 33' (*KTU* 1.3) and RS 3.322⁺ ii 46 (*KTU* 1.19) as proof that the noun *ksl*, which he translates “Lende,” was variable in gender. Because, however, the personage whose organs are being described is feminine in the first case but masculine in the second, the possibility should be considered that, in the second verse of the sequence in RS 3.322⁺, the subject of the verb is not the body part but the person and the body part is in an adverbial case (literally, ‘behind, he breaks in the sinew, he rattles in the points of his sinews’).⁶⁶²

— p. 285 (§52.5c), p. 513 (§74.222.3), p. 611 (§75.212.11). RS 24.261:9 (*KTU* 1.116) is not the text to prove that *pnm*, ‘face’, is of feminine gender in Ugaritic and singular in number, since that noun is plausibly the object, not the subject, of the verb *t̥izr* in that passage (‘you will veil [her] face’).⁶⁶³ One may also quibble about citing the corresponding Biblical Hebrew form as “*pānəh*,” when, as in Ugaritic, only the plural form of the noun meaning ‘face’ is attested. On p. 611, he analyzes *t̥izr* as a 3 f.s. active form, but does not translate the passage, providing only a general gloss (“Gesicht verschleiern”). The only feminine noun that could serve as subject is the divine name *Ta’utka*. As this is a prose ritual text, however, in which the deities never play active roles, it cannot be considered plausible that this noun is the subject of the verb (‘*Ta’utka* veils the [viz., her own] face’). This, then, is another example of T. straining the evidence to avoid taking a *t*-preformative form as 2 m.s. (see above, remark to p. 211 [§41.12], etc.).⁶⁶⁴

— p. 286 (§52.5d). One would think that the use of single quotation marks around the word for ‘sun’ as a translation of *špš* should signal to the reader that T. believes the word is being used metaphorically. That is certainly the case of RS 18.038:21 (*KTU* 2.39), where the reference is without doubt to the Hittite overlord—indeed, in this letter from the king of Hatti, it is a self-reference as ‘the Sun’. It appears equally certain, however, that such is not the reference in RS 15:125:2'-3' (*KTU* 2.19), where the ‘purity, brightness, innocence, freedom’ (√BRR) of the beneficiary of the contract is likened to that of the sun (*km špš d brt kmt br štqšlm b unt d Im*, ‘like the sun, which is “pure,” so is Šitqišalimu “pure” of *’unuttu*-duty in perpetuity’). Surely the point of the comparison is the brightness of the celestial orb, not that of the Hittite sovereign. T.’s translation of the second text, “wie die ‘Sonne,’ die frei ist,” correctly represents the force of the metaphor as expressing socio-economic freedom, but it is hardly likely that the Hittite king is being cited as a paragon of such freedom rather than the brightness of the celestial orb. Far more likely that the explicit simile was meant to underscore the use of the root BRR signifying ‘bright, pure’ (cf. Akk. *zaqû*) as a metaphor for socio-economic freedom. Hence the translation, it appears to me, should have been “wie die Sonne, die ‘frei’ ist.”

⁶⁶²Cf. *KTU/CAT*, where the verb parallel to {yt̥br} in text 1.19 is reconstructed as {yḡš}, instead of {tḡš}, the form attested in RS 2.[014]⁺.

⁶⁶³Pardee, *Les textes rituels* (2000) 655; idem, *Ritual and Cult* (2002) 95.

⁶⁶⁴One might consider an analysis of *t̥izr* that T. does not: it would be a G-passive and *pnm* an accusative of respect, ‘*Ta’utka* is to be veiled as to the [viz., her own] face’. If, however, one admits the existence of 2 m.s. forms addressed to the officiant in these text, such acrobatics are not required.

— p. 286 (§52.5e), p. 394 (§69.133.22b), p. 402 (§69.161.2), p. 402 (§69.162.2a). Because the plural of *tq*, a textile product of uncertain identification, is attested as *tqt*, T. assumes that it is feminine in gender. But *tš‘m tq tn kbd*, ‘ninety-two *tq*-textiles’ (RS 18.[539]:1 [*KTU* 4.595]), shows that the gender is masculine, for the number noun *tn/tt*, ‘two’, shows gender agreement⁶⁶⁵ and *tn* is the masculine form of that noun. Compare the identical morpho-syntax of *tš‘m mrh aḥd kbd*, ‘ninety-one spears’ (RS 15.083:9-10 [*KTU* 4.169]), with the masculine form of the number adjective ‘one’. *tq/tqt* provides, therefore, an additional example of masculine nouns with a plural in *-t* (on this phenomenon, see T., pp. 297-300 [§53.331] and my remark below to that section).

— p. 286 (§52.5e), p. 674 (§75.62a), p. 880 (§93.449.1). *k tgd arz b ymnh* in RS 2.[008]⁺ vii 41 (*KTU* 1.4), a notoriously difficult passage, cannot bear the burden of proving that *arz*, ‘cedar’, is of feminine gender. Because, as T. recognizes, *arz* is masculine elsewhere in the Semitic languages, it is far more likely either that *arz* here is feminine because it has a special meaning (‘cedar shaft’, or the like), and perhaps is not a /qatl/ form like the word for ‘cedar’, or else that *tgd* is a deverbal noun following *k* analyzed as a preposition (/ka taḡiddi ‘arzi/, ‘like the X of cedar’) rather than a finite form following *k* analyzed as a conjunction (/kī taḡḡudu ‘arzu/).⁶⁶⁶

— p. 286 (§52.5f). I know of no proof that Hebrew *gōren* is feminine in gender, as T. asserts. That noun with its plural in {-t} in both Ugaritic and Hebrew plausibly belongs, therefore, to the category of masculine nouns with plurals in {-t} rather than to that of feminine nouns with Ø-ending in the singular.

— p. 287 (§52.5f), p. 299 (§53.331.2). In the first paragraph cited, *kt*, a type of vessel which was used as a measure, is cited as an example of a feminine noun without the feminine morpheme {-t} on the basis of its plural *ktt*; in the second, the plural *ktt* is cited as a possible example of a masculine noun with a plural in {-t}. In this case the two opposite viewpoints are not easily ascertained through the indices, for, in the index of Ugaritic words, *kt* is indicated only for p. 287 and *ktt* only for p. 299, while the text index, usually the most reliable way of tracking down divergent explanations, does not come into play here because different passages are cited for the plural *ktt* (on p. 287, *KTU* 4.60:4 [RS 11.[913]; on p. 299, *KTU* 4.161:7 [RS 15.066:7]).

— p. 287 (§52.5f,g), p. 299 (§53.331.3), p. 707 (§76.524.3a). Though T. may be correct in asserting that *tgr*, ‘gateway’, shows plurals in both {-m} and {-t} (on the proper terminology for expressing this phenomenon, see note below to pp. 297-300 [§53.33]), the only text that T. cites as showing the masculine-type ending in the plural, RS 24.266:26', 28', 35' (*KTU* 1.119), may be interpreted otherwise.⁶⁶⁷ In the first two passages cited, he refers to *klāt tgrt bht* ‘nt (RS 2.[014]⁺ ii 3-4 [*KTU* 1.3]) as possible indicators of a feminine noun meaning

⁶⁶⁵T., p. 391 (§69.127).

⁶⁶⁶Cf. Pardee, *Context I* (1997) 263 with note 191.

⁶⁶⁷Pardee, *Les textes rituels* (2000) 681 (the form may be singular and refer to the royal entrance located on the west side of the city).

‘gate’, in Ugaritic,⁶⁶⁸ but abandons that attempt on p. 707, where a contextually less satisfying interpretation is presented without a cross-reference to the other analysis, viz., “Sie (sc. ‘Anatu’) verschloß die Tore des Palastes der ‘Anatu ...’.”

— p. 289 (§53.14), pp. 290-91 (§53.221). Because one tends to find singular/dual distributives in the West-Semitic languages (e.g., as in French ‘les pompiers ont mis leur chapeau sur la tête’ rather than ‘the firemen put their hats on theirs heads’ as in English), it must be considered very unlikely that {rīshm} means “die beiden Köpfe von ihnen beiden” (p. 290) and that {[li]šnhm} means “die beiden [Zun]gen von ihnen beiden” (p. 291).

— p. 291 (§53.222), p. 541 (§74.35). The reading of the sixth sign in RS 1.001:2 (*KTU* 1.39) is not in doubt, as T.’s citation {mntnm/r/w kbd} (p. 291) would lead the reader to believe. The reading as {w} is certain.⁶⁶⁹ The reading of the word *mntnm*, a dual meaning ‘two loins’, is thus as certain here as in RS 24.253:7 (*KTU* 1.109). Though the singular of this word is unattested, the dual form *mntn*- leads to the conclusion that it was probably *mntn*, for, as T. makes clear in this paragraph, the dual was usually formed on the singular stem. The plural *mntn* is visible in RS 18.056:23 (*KTU* 1.87) and read/restored in RS 1.003:21 (*KTU* 1.41).⁶⁷⁰ Because the Ugaritic word for ‘kidneys’ is, as in Hebrew, *klyt*, and because it is unlikely that ‘sinews’ would have been an important offering material, these two possible interpretations considered by T. must be rejected in favor of the one he prefers, viz., ‘loins’, that is, a cut of meat from the upper mid-section of the animal (cf. English ‘loin’, ‘tenderloin’, ‘sirloin’).⁶⁷¹ Whether {mttm} in RS 24.284:4 (*KTU* 1.130:19) is a scribal error (T., p. 60 [§21.354.1b]) or a true variant (/matuttāma/ ← /matuntāma/) remains uncertain.⁶⁷²

— p. 292 (§53.231). Because there is no obvious reason why a noun designating a garment that is derived from the root ʾZR, ‘to gird’, would be dual, it may be preferable to analyze the -m of *mizrtm* as enclitic⁶⁷³ rather than as the dual morpheme.

— p. 292 (§53.232a), p. 442 (§73.223.5), p. 460 (§73.273.8), p. 568 (§74.422), p. 735 (§77.51b). In the first section cited, T. quotes RS 12.061:5 (*KTU* 1.78) as containing *kbdm*, ‘two livers’, with no indication that the reading is uncertain. On pp. 292, 442, and 735, he indicates that the reading of {b} is questionable, but says nothing about the uncertainty of the {k}. In fact, both the reading of ‘two livers’ and the interpretation of the following verb, BQR, as meaning ‘inspect (livers)’ are uncertain.⁶⁷⁴

⁶⁶⁸Cf. Pardee, *Context I* (1997) 250.

⁶⁶⁹Pardee, *Les textes rituels* (2000) 17, 30 n. 71, 1263.

⁶⁷⁰Ibid., pp. 31, 149, 177-78, 470.

⁶⁷¹Ibid., pp. 31-32.

⁶⁷²Ibid., pp. 732, 734.

⁶⁷³Del Olmo Lete and Sanmartín, *Diccionario II* (2000) 257.

⁶⁷⁴For a full epigraphic and philological discussion, see Pardee, *Les textes rituels* (2000) 417-18, 425-27 (with references to earlier literature, to which may be added Pardee, *JAOS* 113 [1993] 615; idem, *AuOr* 16 [1998] 89 — the latter shows that T. was aware of my epigraphic and philological reservations but chose to ignore them; indeed this article, of which T. was well aware because I sent him a copy before publication and because it appeared before the cut-off date for the present work, is not even listed in the bibliography).

— p. 295 (§53.322.1). The agreement among the Massoretic vocalization of Biblical Hebrew and the Jewish and Christian Aramaic traditions on the vocalization of /qatl/qitl/qutl/ plurals with aspiration of third-radical /bgdkpt/ letters shows that these forms had dissyllabic stems not only in proto-Hebrew but in proto-Aramaic as well. That being the case, it is necessary to group these West-Semitic languages together with Ugaritic over against Arabic, where a very different system of plural stems is attested.

— p. 295 (§53.322.1a), p. 444 (§73.233.41), p. 642 (§75.521b). On p. 444, T. refers to Pardee⁶⁷⁵ in favor of the analysis of *mādn*, and hence of the dependent verb *qln*, as feminine plural in RS 24.247⁺:1 (*KTU* 1.103⁺); but he does not refer to the vocalized text in the same article (p. 145) where *mādn* is vocalized as a singular. This preference for the singular would become even stronger if one were to accept the classification of the noun as a /qutl/ base (p. 295), for true plurals of nouns from this base, of which the primary function in the Northwest-Semitic languages is to express abstract concepts, are rare.

— pp. 297-300 (§§53.331-332). The propriety of the formulae “Genusmarkierung im Plural versus Genusmarkierung im Singular,” “Feminine Pluralendung bei formal maskulinem Singular,” and “Maskuline Pluralendung bei femininem Singlar” is debatable, at best imprecise and at worst misleading. Masculine nouns with a plural ending in {-t} are not ‘marked’ for “feminine gender,” as T.’s formulation might lead a non-Semitist to believe, they only bear the plural morpheme that is more commonly attested with feminine nouns. Fewer feminine nouns show a plural ending in {-m}, which is more commonly attested with masculine nouns. Though the grammatical gender of such plural forms is rarely demonstratable for Ugaritic, one cannot seriously suppose, and T. does not, that the Ugaritic situation was structurally different from Hebrew, where verbal and adjectival agreements demonstrate the gender of such nouns (e.g., *n³hārōt g³dōlīm*, ‘great rivers’, *nāšīm ṭōbōt*, ‘good women’). It would thus be preferable to refer to masculine nouns with feminine-type endings and vice versa.

— p. 298 (§53.331.2), p. 302 (§53.36d), p. 311 (§54.133.2a). Collation of RS 16.382:15 (*KTU* 3.5) shows that {‘lmt}, supposed since the *editio princeps* to be the plural of ‘l^m, ‘eternity’,⁶⁷⁶ but the only attestation of that form, is not in fact the correct reading. The line is to be read {šḥr . ṭlṭlt} and to be translated ‘at dawn (or) on (day) three’ and its meaning in context is ‘at no time in the future’.⁶⁷⁷ It has long been recognized that the formula is the functional equivalent of Akkadian *urra šēra*,⁶⁷⁸ where the second word is etymologically cognate to the first word of the Ugaritic formula; but it now becomes clear that the Ugaritic formula is in fact good Ugaritic and, though perhaps an attempt to reflect the Akkadian formula, not a slavish calque.

— p. 298 (§53.331.2). The masculine gender of *qrn* cannot be proven for Ugaritic by the texts cited: *ādr qrnt* in RS 2.[004] vi 22’ (*KTU* 1.17) may mean not “die mächtigsten

⁶⁷⁵ *AfO* 33 (1986) 127.

⁶⁷⁶ Virolleaud, *PRU* II (1957) 21.

⁶⁷⁷ Hawley and Pardee, forthcoming in *Semitica* 52.

⁶⁷⁸ Virolleaud, *PRU* II (1957) 22.

Hörner,” but ‘I will vow horns’,⁶⁷⁹ while the restoration of {[qr]nh} in line 14 of the same text is simply incorrect (a trace of the sign preceding {nh} is visible and it cannot possibly correspond to a {r}) and any possible link between the noun *qrn* and a masculine verbal form is thus eliminated.

— p. 298 (§53.331.2), p. 598 (§74.624), pp. 708-9 (§76.524.3d), p. 828 (§89.232b), p. 839 (§91.12a). Though one may admit that the structure of the passage shows that *rhbt* in RS 2.[008]⁺ vi 53 (*KTU* 1.4) is plausibly—though not certainly—masculine in gender, the interpretation of the phrase *ilm rhbt* as “Amphorengötter” is less convincing. An argument can be made for analyzing the noun following *ilm/ilht* in each entry of the list in lines 47-54 as expressing an accusative complement to the verb *špq*, rather than in apposition to *ilm/ilht*, e.g., ‘he provides the gods with jars of wine’,⁶⁸⁰ rather than ‘he provides the jar-gods with wine’.

— p. 299 (§53.331.2). In the highly stylized list of substitute gifts offered by various personages in the *Kirta* epic to the eponymous hero, it is unlikely that {trbšt} in col. iii, line 37 (RS 2.[003]⁺, *KTU* 1.14), represents the plural while the singular *trbš* is used elsewhere. Given the high number of scribal errors elsewhere in this text, T.’s alternative explanation along those lines appears preferable.

— p. 299 (§53.331.3). One can only agree with T.’s cautious rejection of the analysis of the signs {lšnt} in RS 17.100A+B:40' (*KTU* 1.84:9) as expressing the plural of *lšn*, ‘tongue’.⁶⁸¹

— p. 300 (§53.331.4). If *hpnt*, ‘a type of garment’, is correctly interpreted as feminine in gender, this is more plausible because masculine and feminine forms of the word existed (viz., *hpn* and *hpnt*) than because the word was grammatically masculine in the singular and dual, feminine in the plural. The former phenomenon is well attested in Biblical Hebrew, the latter not—and if the Hebrew sources were more abundant there would probably be even fewer of the latter. In any case, the phrase {[...]hpnt . dqt} (RS 34.180,10:4 [*KTU* 4.765]) appears in isolation in a badly broken text⁶⁸² and cannot be cited as proof of the plural in {-t}—it may in fact be the feminine singular.⁶⁸³

— p. 301 (§53.34). Though T. may well be correct in identifying *šlmm*, ‘sacrifice of well-being’, as an example of “Plural als Grundnumerus,” a case can be made for *šlm* in RS 34.126:31 (*KTU* 1.161) providing an example of the technical term appearing, as perhaps in Punic and certainly once in Biblical Hebrew, in the singular.⁶⁸⁴

⁶⁷⁹Pardee, *Context I* (1997) 346 with note 37; see also below, remark to p. 313 (§54.133.2e), etc.

⁶⁸⁰*Ibid.*, p. 262 n. 178.

⁶⁸¹The editor of this text, Herdner, printed the four signs without separation (*CTA* [1963] 135). In *KTU/CAT*, the division {lšnt} is indicated. This division into two words was also present in the transliteration of this text that I made available to the editors of *CAT* in 1994 and subsequently to T. Cf. Pardee, *Les textes rituels* (2000) 451, 454, 456.

⁶⁸²See editions by Bordreuil in *Semitica* 25 (1975) 28-29 and by Bordreuil and Pardee, *RSO VII* (1991) text 95 (pp. 166-67).

⁶⁸³Bordreuil and Pardee, *ibid.*, p. 167, translated “HPNT mince.”

⁶⁸⁴Bordreuil and Pardee, *Syria* 59 (1982) 128; *idem*, *RSO VII* (1991) 162; Pardee, *Context I* (1997) 306-7, n. 13; *idem*, *Les textes rituels* (2000) 818, 819, 824.

— p. 301 (§53.34). In the *Anmerkungen* section at the end of §53.34, T. asks the reader to compare (“Vgl. in diesem Zusammenhang”) some other words that may qualify as examples of “Plural als Grundnumerus.” Two examples of the semantic category ‘body-parts’, viz., *ûšbt*, ‘fingers’, and *šl’t*, ‘ribs’, appear to be highly unlikely examples because both words designate parts of the body that can be numbered and a singular of the latter word is attested in Hebrew (*šélaʿ*). As a *lexical/grammatical* concept, *plurale tantum*, the Latin term cited by T. at the head of this paragraph, does not mean that a word is only attested in the plural but that the plural form has the semantic value of a singular, i.e., that it is the *Grundnumerus*. In languages as poorly attested as the ancient Northwest-Semitic languages, it is necessary to apply logical, grammatical, and comparative reasoning to a word that happens to be attested only in the plural in order to determine whether it is to be classified as an example of *plurale tantum*. If T. only wished in this paragraph to point out words of which the only attestations in Ugaritic happen to be in the plural, he should have clearly stated so.

— p. 301 (§53.34), p. 302 (§53.36). Another dubious category in this *Anmerkungen* section at the end of §53.34 is provided by the terms *kšmm/kšmm*, ‘emmer’, and *šʿrm*, ‘barley’.⁶⁸⁵ *ḥṭm*, ‘wheat’, is not cited here, apparently because a feminine singular form *ḥṭt* is attested (see p. 255 [§51.41c], p. 300 [§53.332]). Given T.’s usual willingness to categorize Ugaritic forms on the basis of comparative data, the view of the three nouns that lay behind this presentation is difficult to comprehend, for feminine singular forms are attested for both of the other terms in Biblical Hebrew, respectively *kussémet* and *šʿōrāh*; consequently it appears likely that feminine singular forms of KSM and ŠʿR existed in Ugaritic and that the situation of the two nouns in question was, therefore, no different from that of *ḥṭt/ḥṭm*. A similar case may be made for *ḡnbm*, ‘grapes,’ for a masculine singular form of this word is attested in Biblical Hebrew (*ʿēnāb*). On p. 302, *kšmm*, *šʿrm*, *ḡnbm*, and *šmqm*, ‘raisins’, are all cited as examples of the plural being used because the entity in question is uncountable (“nicht zählbar”). As regards the use of the plural form, it is to be explained not, strictly speaking, on the basis of these items being uncountable but on the fact that they are so small as to be normally handled, both in everyday use and as economic entities, in groups rather than as individuals. However that may be, the certain existence of masculine and feminine singular forms used as collectives to designate the same entities in other languages or a similar entity in Ugaritic (*ḥṭt/ḥṭm*) shows the unlikelihood that Ugaritic lacked such collective terms alongside the plurals. That must be judged all the more likely for easily countable items, such as ‘ribs’ or ‘wailing women’ (see next remark).

— p. 301 (§53.34), p. 478 (§73.431c). Yet another dubious category in this *Anmerkungen* section at the end of §53.34 is that of words for professional weeping women, *bkyt* and *mššpdt*. Both are parsed on p. 478 as participles in substantival usage and participles are usually fully productive as to number. I see no reason, logical or linguistic, to doubt that a Ugaritian could refer to a single weeping woman.

— p. 302 (§53.36f). Because Hebrew *mēʿīm* designates a part of the body, usually translated along the lines of ‘entrails’, one may doubt that Ugaritic *mmʿm* means “(viel) Blutgerinsel.” The parallelism with *dmm*, ‘(shed) blood’, must not be inferred to suggest

⁶⁸⁵ Cf. also p. 408 (§69.212).

strict parallelism of meaning, but similarity, viz., the entrails that run out when a body is torn to pieces.

— p. 303 (§54.111). There is no doubt that $\sqrt{\text{RP}}$ normally functions to designate an “Ahnenbezeichnung” in Ugaritic; nevertheless, the precise meaning of *rpi* in the title of *ʾAqhatu*’s father that runs *mt rpi*, ‘man of RP’, must be classified as uncertain.

— p. 303 (§54.111). Asserting without a full-scale argument that the Ugaritic triptotic case system {/u/-i/-a/} goes back to an earlier system that would have been {/um/-im/-am/} leaves unexplained why the West-Semitic languages show /m/-n/ in the plural while Akkadian shows /m/ in the singular.

— p. 304 (§54.112.2). Because both /raḥbāna/ and /raḥbāni/ are attested in syllabic writing for the genitive case of the river name *Raḥbānu* (modern Nahr el-Kebir), T. concludes that nouns ending in /V:n-/ were inflected both diptotically and triptotically. Is it not possible, since all such data come from syllabic texts, that the triptotic inflection reflects the language in which the texts were written, the diptotic inflection the local language? If the diptotism of nouns of these types is a West-Semitic phenomenon, one would expect it to influence the use of these words in Akkadian but not necessarily that it should totally replace the proper Akkadian inflection.

— pp. 306-7 (§51.12). As we will see in the following remarks, not a single case of the plural oblique form being used in syntactically nominative slot is convincing. Indeed, this section must be regarded as one in which T. takes one of his more ‘maximalist’ positions (see above, first general remark). Though anything may be possible, the absence of case confusion in the singular and the dual make such a confusion in the plural *prima facie* unlikely. When one further considers that what is taken as the primary marker of the confusion in the plural, viz., {-y} in its putative use as a *mater lectionis*, also functions as an enclitic particle, the inherent implausibility of the grammatical category becomes, in my eyes at least, obvious.

— pp. 306-7 (§51.121.2a), p. 835 (§89.35). On the analysis of the {-y} of *ily ūgrt* in RS 15.008:4-5 as the enclitic particle (an analysis that T. on p. 306 considers only “theoretisch” possible), rather than as a *mater lectionis* proving the degradation of the case system, see above, note 88, and remark to p. 265 (§51.44m), etc.

— pp. 306-7 (§51.121.2a). On the analysis of *mrḥy* in RS 24.247+:7 and 47' (*KTU* 1.103+) and in RS 24.302:10' (*KTU* 1.140) as a feminine singular noun, rather than as a masculine plural noun, in the wrong case and with a *mater lectionis* used to designate the wrong case, see above, remark to p. 52 (§21.341.21b), etc. It truly boggles the mind that so rare a combination of phenomena can seriously be proposed to have been repeated three times in two texts.

— p. 307 (§54.121.2a). Though the interpretation of RS 24.247+:33'-34' (*KTU* 1.103+) must be deemed uncertain because of its broken state, even if one admit with T. that {ymy} is more plausibly nominative than oblique, the {-y} remains more plausibly the enclitic particle⁶⁸⁶ than a *mater lectionis* for the incorrect oblique case vowel.

⁶⁸⁶Pardee, *AfO* 33 (1986) 125 (with note 42), 136, 146; idem, *Les textes rituels* (2000) 551.

— p. 307 (§54.121.2b). Since the editors of RS 34.126 (*KTU* 1.161) have consistently interpreted {rpim} in line 8 of that text as accusative in function,⁶⁸⁷ it can hardly be taken at face value that “Aufgrund des Kontextes” the word must be taken as a case of an oblique form used in the nominative case. T. prefers his argument that the form must be nominative because the surrounding verbal forms are passive; we found it more plausible to say that the case system is used correctly elsewhere in this passage⁶⁸⁸ so the form must be considered as saying what it says. The ‘context’ as an argument corresponds, therefore, to T.’s interpretation of the passage and nothing more. With that in mind, the verb in this line, written {qrū}, cannot be a passive form, as it is interpreted on p. 465 (§73.332.1), etc. (see remark below to this page).

— p. 307 (§54.121.2b). For a recent interpretation of {rpim} as accusative rather than nominative in RS 2.[009]⁺ vi 45 (*KTU* 1.6), with literary arguments, see Pardee, *Context I* (1997) 273 (with note 279). T.’s argument that √ḤTK elsewhere in Northwest Semitic means “(ab)schneiden, bestimmen,” rather than ‘to rule’, as is required here if {rpim} be in the accusative, cannot carry the day, for Ugaritic has two or three nouns based on this root that express parental relationships,⁶⁸⁹ and the verbal usage in RS 2.[009]⁺ may well be denominative, expressing a benevolent rule of the familial type.

— p. 310 (§54.133.1b), p. 513 (§74.222.2), p. 650 (§75.527a), p. 863 (§93.33). Because no statement of the application of the medication is mentioned in the text if {yšt} in RS 24.258:31' (*KTU* 1.114) is derived from Š(Y)T, ‘to put’, the verb in question is more plausibly ŠTY, ‘to drink’.⁶⁹⁰ On the formal parallel between this text and medical texts, see below, remarks to p. 646 (§75.522), etc.

— pp. 310-13 (§54.133.2). For a noun in the accusative case where the accusative is expressing an adverbial notion and which is expanded “durch ein enklitisches -m,” T. proposes two analyses/vocalizations: /-am/, which would consist of the accusative vowel plus “Mimation,” and /-amma/, which would consist of the accusative vowel plus “Mimation” plus the “Enklitische Partikel -m = /-ma/.” I see no reason to resort to mimation to explain the

⁶⁸⁷Bordreuil and Pardee, *Syria* 59 (1982) 123, 126; idem, in *Une bibliothèque* (1991) 155, 156-57; Pardee, in *Verse in Ancient Near Eastern Prose* (1993) 209; idem, *Les textes rituels* (2000) 818, 821; idem, *Ritual and Cult* (2002) 87; Bordreuil and Pardee, *Manuel* (2004) II 63, 64.

⁶⁸⁸T.’s view expressed below (p. 316 [§54.215]) that the case system appears not to be used consistently in this text has no foundation and is to be rejected. It appears to be a remnant, otherwise rather thoroughly eradicated in this grammar, of T.’s earlier view that this text is representative of “late Ugaritic” (cf. “Morphologische Besonderheiten des Spätugaritischen,” *UF* 25 [1993] 389-94, and my criticisms thereof in *Les textes rituels* [2000] 202 n. 243; 821 nn. 26, 29). Whether or not one accept that *ʾIlīmilku*’s work was late (cf. Dalix, *CRAI* 1997, p. 819-24; idem, *Semitica* 48 [1998], p. 5-15; Pardee, *Context I* [1997] 241 n. 3; idem, “Le traité d’alliance RS 11.772⁺,” *Semitica* 51 [2001] 5-31), a very high percentage of the other Ugaritic texts date to the last decades of the kingdom of Ugarit; if one accept that the texts inscribed by *ʾIlīmilku* date to the same period, then there is virtually nothing but “late” Ugaritic.

⁶⁸⁹Del Olmo Lete and Sanmartín, *Diccionario I* (1996) 183.

⁶⁹⁰Pardee, *Les textes para-mythologiques* (1988) 22, 23, 72. On parsing the form as indicative imperfective, rather than as a jussive as I did here, see remark below to p. 662 (§75.532).

Ugaritic forms, a very rare phenomenon in the singular in West Semitic,⁶⁹¹ when accusative plus the enclitic particle are all one needs, i.e., /-ama/. In any case, such Hebrew forms as *yômām*, ‘by day’, could have arisen from /yômama/ or from /yômam/, but probably not from /yômamma/, for /a/ in a sharpened syllable at the end of a word in proto-Hebrew most commonly shows up as *pataḥ*, not as *qameṣ*, in the Massoretic tradition. Remarkably, though T. includes dual and plural forms in his list, he does not discuss their vocalization. Apparently this is because mimation is part of the marking of the dual and plural absolute forms, and none of them shows an additional {-m}—hence such adverbial accusatives (i.e., obliques) were not expanded with another enclitic {-m}. I would deduce from this fact that the vocalization of the two was probably similar, if not identical and hence that the adverbial enclitic was /-ma/ (perhaps with a dissimilatory form /-mi/) just like the dual/plural element. It is well known that enclitic {-m} is very rarely attached to masculine dual/plural absolute forms.⁶⁹²

— p. 311 (§54.133.2a). As has already been observed,⁶⁹³ the reading {ymm} is incorrect in *KTU/CAT* 5.9:4 (RS 16.265): what these authors and the original editor⁶⁹⁴ took as a second {m} is in fact one of two {t}s inscribed on the right edge of the tablet. This word does not, therefore, provide an example of enclitic {-m} attached to a noun in the adverbial accusative.

— p. 311 (§54.133.2a). Neither does {llm} exist in RS 24.255:2 (*KTU* 1.111) to provide an example of *ll*, ‘night’, in the adverbial accusative with enclitic {-m}: the {m} is in fact two signs, {ǧz}.⁶⁹⁵

— p. 311 (§54.133.2a), p. 747 (§81.25). {āḥrm} in RIH 78/14:12' (*CAT* 1.163:5), though certainly used adverbially, may mean ‘thereafter’, rather than “hintereinander.”⁶⁹⁶

— p. 312 (§54.133.2c). TB^c, ‘to depart’, is indeed construed with the preposition ‘M in RS 15.098:13-14 (*KTU* 2.17:6-7), but the meaning there is ‘to depart with’, not ‘to depart to’, as the listing here would lead one to believe.

— p. 312 (§54.133.2d), p. 477 (§73.431b), p. 482 (§73.513.5a), p. 666 (§75.536). T. does not explain why an adjective or a participle that is in apposition with a subject noun should be an adverbial accusative “der Art und Weise.” It appears just as plausible, if not more so, that *bky* in the phrase *ytn gh bky*, ‘He gave forth his voice, crying’ (RS 3.325+ i 13-14 [*KTU* 1.16]), should be in the nominative. Indeed, in this particular case, since *g* would be an

⁶⁹¹The phenomenon is rare in West Semitic but even rarer in T.’s subject index, where the word does not appear.

⁶⁹²Below, pp. 825-32 (§89.2), very few are listed: a rapid scan discovered only {‘šrmm} (RS 24.291:17 = *KTU* 1.132)—the identical form twice repeated in RS 24.266:20'-21' (*KTU* 1.119) is not cited here, though the passage is translated on p. 367 (§63.14, 15). {‘šrmm} should in all three passages have been in the nominative case, though it is uncertain whether the noun is in the dual or the plural (Pardee, *Les textes rituels* [2000] 676-77; T. translates both passages as containing the dual).

⁶⁹³Pardee and Whiting, *BSOAS* 50 (1987) 11 n. 24.

⁶⁹⁴Virolleaud, *PRU* II (1957) 39.

⁶⁹⁵Pardee, *Les textes rituels* (2000) 619, 620, 1282.

⁶⁹⁶*Ibid.*, pp. 863, 869-70.

accusative of direct object, *bky*, if expressed in the accusative, would appear to the listener to be agreeing with that noun rather than saying something about the subject. What T. has done in this section is to present as morpho-syntactically identical (1) adjectival phrases that provide an element of description of the subject of the sentence and (2) nouns with an adverbial function—the reader deserves an explanation for why these two categories have been considered to have been marked identically. Finally, as regards *bky* in this phrase, on pp. 482 and 666 T. presents as an option the analysis as an infinitive, which analysis seems to be proven correct by the phrase *ttn gh bky*, ‘she raises her voice in weeping’, in RS 3.325⁺ ii 36 (*KTU* 1.16).⁶⁹⁷ In this passage at least, the infinitive may indeed be in the accusative, functioning as an adverbial noun (‘she gives forth her voice as regards weeping’).

— p. 313 (§54.133.2d). It is equally uncertain that the terms *šrp* and *šlmm*, which express the category to which a series of offered items belongs (‘burnt-offering’ and ‘peace-offering’, respectively) and which are placed in apposition to the list of offered items, were adverbial accusatives. It appears equally likely that they are morphologically as well as syntactically appositional to the list of items offered.⁶⁹⁸ T.’s translation “als Brandopfer / *šlmm*-Opfer” may be taken to indicate that he has classified them as adverbials because they require translation by prepositional phrases. The preposition in question, however, expresses equality,⁶⁹⁹ and, in the absence of an argument to the contrary, one may be allowed to surmise that the semantic equality was expressed by morpho-syntactic identity.

— p. 313 (§54.133.2e), p. 844 (§91.314.1). T.’s solution for the different distributions of *adr* in RS 2.[004] vi 20-23 (*KTU* 1.17), viz., that the form would always be a substantival adjective (“die mächtigsten Hörnen” [l. 22], “die größte der Eschen” [l. 20]) but in line 23 not be in the construct with the noun it modifies (instead, the noun would be in the adverbial accusative: *adr b gl il qnm*, “das größte an Schilfrohren (*qnm*) im göttlichen(?) Röhricht”), is not altogether convincing. When the solution of taking *adr* as the 1 c.s. /YQTL/ form of NDR, ‘to vow’ (see above, remark to p. 298 [§53.331.2]), does not involve this morpho-syntactic difficulty, and when it accounts for other features of the list better than does the analysis of *adr* as adjectival (the principal problem is that *adr* shows no variation in form though one might expect *qrnt* in line 22 to be feminine [see above, remark to p. 298, §53.331.2] while *qnm* in line 23 is certainly plural⁷⁰⁰), it should be given more serious consideration.

— p. 313 (§54.133.2e), p. 402 (§69.162.1a), p. 408 (§69.211), p. 879 (§93.448). I see no reason why *ksp* in the sentence *ksp hmšm is*‘, ‘silver, fifty (shekels), I will draw forth

⁶⁹⁷The proposal to emend to {*bky*<*t*>} here (p. 60 [§21.354.1a], p. 204 [§33.441], p. 482 [§73.513.5a]) is not even mentioned on p. 666, where only the analysis as an infinitive is registered.

⁶⁹⁸Pardee, *Les textes rituels* (2000) 19.

⁶⁹⁹I have introduced these phrases with “(comme)” in French (*ibid.*, p.18 *et passim*—the parentheses, in this more literal translation, are intended to express the absence of a lexical marker of equivalence in Ugaritic) and with “as” in English (*Ritual and Cult* [2002] 28 *et passim*—without parentheses in this translation, intended to be more idiomatic, see p. 99 n. 4).

⁷⁰⁰This fact requires T. here to translate the adjective as singular, while in the two other instances cited he translates as dual/plural.

(= pay)', should be an adverbial accusative (this analysis is proposed on p. 313, where T. translates "an Silber"; on p. 402, he translates "an/in Silber") rather than an accusative of direct object (on pp. 408 and 879, T. translates without "an" or "in"). T. explicitly parses *is*^c as from NS^c (p. 171 [§33.212b] p. 449 [§73.243.21], p. 598 [§74.624], p. 627 [§75.42]) and as a transitive verb (p. 598), and there is no obvious reason why *ksp* should not be taken as the direct object of that verb and *ḥmšm* as an appositional explicative to *ksp*. The vocalization would be the same, /kaspa ḥamišīma/, in either analysis.

— pp. 314-15 (§54.214a), p. 319 (§54.221c), p. 319 (§54.222), p. 771 (§82.39). In the first section cited, T. manages to use over half a page (twenty-six lines) to argue that {lksh}, which is to be emended to {l kšⁱ!}, in RS 34.126:20 (*KTU* 1.161) is a vocative formula ('O throne'), rather than a prepositional one ('from the throne'), without mentioning the fact that the editors of the text have always taken the formula as prepositional and, in the official *editio princeps*, have provided explicit arguments against the analysis as a vocative.⁷⁰¹ The most important of these arguments are: (1) the vocative of this very word appears in line 13, without the *l*, and (2) this passage is indubitably imitating three passages from the Baal Cycle (RS 2.[022]⁺ vi 12, 25 [*KTU* 1.5], RS 2.[009]⁺ i 7 [*KTU* 1.6]), where someone is said to descend 'after *Ba'lu*'; in the first of these, the god 'Ilu 'descends from the throne' (YRD + *l kšⁱ*), the very same verb-preposition combination as in RS 34.126. In §54.222, T. provides another argument against his analysis of the particle when he remarks that RS 34.126:20 provides the only example of the vocative particle occurring at the end of a poetic line-segment that depends syntactically on the following line.

— p. 315 (§54.214b), pp. 316-17 (§54.215), p. 317 (§54.221a), p. 336 (§54.52), p. 337 (§54.62). T. considers {kšⁱ} in RS 34.126:13 (see preceding remark) to represent an "endlose Form" /kussi'/ but provides no argument in favor of that hypothesis. The form *ūmy*, 'O my mother', in RS 16.379:21 (*KTU* 2.30), if it indeed be vocative, makes it very plausible that the vocative not introduced by a vocative particle did in fact bear a case ending, for there is no evidence that pronominal suffixes were attached to zero-ending forms — the evidence for such forms in Ugaritic is limited primarily to proper nouns, which normally may not receive pronominal suffixes.⁷⁰² Bordreuil and I have argued that, if the vocative particle *l* was identical with the preposition, which would always have been followed by the genitive, the vocative expressed without a vocative particle may have been in the genitive case by analogy with the prepositional expression thereof.⁷⁰³ T. accepts below (p. 804 [§84.12]) the

⁷⁰¹ Bordreuil and Pardee, *Une bibliothèque* (1991) 154, 155, 159-60.

⁷⁰² I have translated the form as vocative with pronominal suffix (*Afo* 31 [1984] 225; on p. 230, I vocalized "ummaya," assuming at the time that the accusative case was used to express the vocative) as does T. (p. 52 [§21.341.21a], p. 218 [§41.221.15a], p. 319 [§54.221d], p. 729 [§77.392], p. 883 [§94.21]). On p. 52, T. explicitly identifies the {y} of {ūmy} as a *mater lectionis*, but he does not say why that should be the case. The only possibility appears to be that he is taking the vocative case here as the nominative, for that is the only case where consonantal {-y} is not expected. {kšⁱ} in RS 34.126:13 cannot, however, be in the nominative case, which would be written {kš^u}, so T. must be assuming that different base forms were used in the vocative depending on whether or not a pronominal suffix was attached to the noun. Is that plausible?

⁷⁰³ *Une bibliothèque* (1991) 158.

hypothesis that this *l* is identical with the emphatic particle /la/, rather than with the preposition, in spite of the fact that only Arabic shows a parallel usage, viz., a particle /la/ or /li/ inserted between the vocative particle /yā/ and the noun, which is in the genitive, and the origin of which T. accepts to have been in the preposition. Unfortunately, if the example of RS 34.126:20 be rejected, as it must be (see preceding remark), there is no other example yet attested of this particle *l* attached to a singular noun ending in /ʾ/ to provide information regarding the case with which the particle was used. The Arabic use of /la/li/ followed by the genitive may not be ignored, however, in identifying the Ugaritic particle and its syntactic relationship to the following word.

— p. 317 (§54.221a), p. 505 (§73.634a), p. 634 (§75.512). T. does not mention the possibility of taking *tqh* and *tqyn(h)* in RS 3.367 i 18', 34' (*KTU* 1.2) as derived from separate roots, the first from WQH, 'to obey', the second from WQY, 'to fear' or 'to protect'.⁷⁰⁴ If such were the correct analysis, the parallelism would have been based on the similarity of the two roots, rather than on repetition of the same root.

— pp. 320-25 (§54.3). T.'s classification of the adverbial morpheme {-h} as a case in the narrow sense of the word appears to rest entirely on his identification of the /h/ with the /š/ of the Akkadian dative case (/iš/). Thus he considers Ugaritic nouns bearing pronominal suffixes that require an adverbial translation as examples of "terminative ohne *h*-Marker" alongside his putative examples of defective writing of the adverbial morpheme (p. 322 [§54.316]). For the analysis of the West-Semitic forms as consisting of a deictic morpheme attached to the accusative case, see remark above to pp. 151-52 (§33.131.1), etc. That the Ugaritic/Hebrew morpheme could not have been a case morpheme is shown unequivocally by the fact that, when it is attached to (dual and) plural nouns, it appears after the mimation (e.g., *šmmh*, 'to the heavens'), i.e., after the case and number markings (cf. /mišráyma/ in Hebrew).⁷⁰⁵ One must also note that the Akkadian form is /-iš/, whereas the Ugaritic-Hebrew forms certainly had an /a/-vowel, a difference that T. makes no attempt to explain, neither here nor in *UF* 33 (2001) 626-29. For a possible remnant of proto-Semitic /-iš/ in Northwest Semitic that is phonetically more appealing, see above, remark to p. 152 (§33.132). If the Northwest-Semitic adverbial morpheme {-h} is in any sense related to the Akkadian dative morpheme, it cannot be as a remnant of that same case in proto-West Semitic because of the problems of morpheme sequencing just stated (see also above, note 293, where the Hebrew data are cited); it would have to be as some kind of a secondary borrowing and the identification as a deictic particle appears, for that reason, more plausible.

— p. 321 (§54.315.1), p. 323 (§54.316), p. 366 (§63.123), p. 831 (§89.27a). T. must be congratulated for going against the trend of considering the {-h} of {l 'nth} in RS 1.005:13

⁷⁰⁴Pardee, *Context I* (1997) 246.

⁷⁰⁵T. considers *pnm* a possible case of the terminative case without {-h} on the analogy of Hebrew *pānīmā^h*, 'on the inside' (p. 323 [§54.312.2f]) without remarking that the case and numbers markers occur before the *-mā^h*. In *UF* 33 (2001) 628, T. avers that his putative case ending is attached "im maskulinen Plural sehr wahrscheinlich an das Nomen mit Endung *-ūm(a)*" with no attempt to explain how such as thing is possible. It goes against the grain of Semitic morpheme structure for a case ending to follow a case/number marker that consisted of the case/number marker itself and enclitic /-ma/.

(*KTU* 1.43) as only pronominal:⁷⁰⁶ the possibility must be considered that the {-h} is the adverbial morpheme.⁷⁰⁷ On the other hand, his assertion that the second sign may be {t} rather than {ʿ} must be rejected.⁷⁰⁸

— pp. 321-23 (§54.315.2). T. argues that the locative morpheme {-h} may sometimes be written “defektiv,” whatever that might mean for Ugaritic. When used for the writing systems of the later Northwest-Semitic languages, the term refers to a long vowel not followed by a one of the consonants used as *matres lectionis*. But T., to the contrary of some earlier scholars,⁷⁰⁹ considers the {h} of the locative morpheme to be consonantal in Ugaritic, viz., not a *mater lectionis* for /ā/ (pp. 151-52 [§33.131.1], p. 320 [§54.311]). In any case, none of T.’s arguments for the presence of the locative morpheme without a graphic indicator is convincing. For example, the absence of {-h} in the Baal Cycle may reflect the oral tradition proper to that cycle—it would, in any case, be astonishing that this most archaic of the Ugaritic poetic cycles should show an orthographic/phonetic feature that must, according to T.’s view of the origin of the morpheme, be secondary. For another, the occurrence of a self-same word with and without the particle in parallel passages in the *Kirta* cycle may reflect simply the interchangeability of the particle and the adverbial accusative; or, since that text contains a great many writing errors, the omission of {-h} could be erroneous. The most basic counter-argument, however, is that if the /-h/ dropped from the morpheme that he considers to be /-ah/ or /-ā/—and the latter is highly implausible—, the resultant form would in the singular be identical with the adverbial accusative (see above, remark to pp. 151-52 [§33.131.1], etc.). One can only conclude, as I have already done in the remark just cited, that, just as in proto-Hebrew, with remnants visible in Biblical Hebrew, the use of the locative morpheme {-h} and of the adverbial accusative were in something approaching free variation.

— p. 321 (§54.315.2b). There is nothing special about *ārṣ* in RS 92.2014:12 (RSO XIV 52) being an adverbial accusative, as one might infer from T.’s creation of an “Anm[erkung]” for this passage alone in which it is asserted that this may be an example of “defektive Schreibung” (or, alternatively, of the accusative of direction).⁷¹⁰

— p. 322 (§54.315.2c), p. 325 (§54.323a). T. presents {š ḥmnh nkl} in RS 24.250+:14 (*KTU* 1.106) as a possible example of the locative morpheme occurring within a construct

⁷⁰⁶ In addition to the references cited by Pardee, *Les textes rituels* (2000) 247-48 and by T., p. 323, see, M. Dijkstra, “El, YHWH and their Asherah. On Continuity and Discontinuity in Canaanite and Ancient Israelite Religion,” pp. 43-73 in *Ugarit. Ein ostmediterranes Kulturzentrum im Alten Orient. Ergebnisse und Perspektiven der Forschung*. Band I: *Ugarit und seine altorientalische Umwelt* (Abhandlungen zur Literatur Alt-Syrien-Palästinas 7; Münster: Ugarit-Verlag, 1995) 51, n. 37; P. Merlo, *La dea Ašratum - Aṭiratu - Ašera. Un contributo alla storia della religione semitica del Nord* (Facultas S. Theologiae, Theses ad doctoratum in S. Theologiae; Mursia: Pontificia Università Lateranense, 1998) 197 n. 204.

⁷⁰⁷ Pardee, *Les textes rituels* (2000) 218, 219, 247-48.

⁷⁰⁸ Ibid., pp. 216, 1266.

⁷⁰⁹ J. Blau and S. Loewenstamm, “Zur Frage der scriptio plena im Ugaritischen und Verwandtes,” *UF* 2 (1970) 19-33.

⁷¹⁰ Pardee, *Les textes rituels* (2000) 830; Bordreuil and Pardee, RSO XIV (2001) 387-88; idem, *Manuel* (2004) II 70.

chain ('a ram in the ḤMN-sanctuary of Nikkal'), but rejects that syntactic analysis in favor of seeing a syntactic break after {ḥmnh} (on p. 325, he cites this and the following phrase without including *nkl* in either). Though the former analysis cannot be deemed certain, the regularity of use of {-h} in construct chains in old Hebrew (both in inscriptions and in Biblical Hebrew) and the paucity of cases of the absence of {-h} in Ugaritic where one might have expected it (the parallelism of *ʿlmh* with *dr dr* in the *ʿAqhatu* text is cited) certainly make it plausible.⁷¹¹

— p. 321 (§54.315.2), p. 324 (§54.323a). It must be deemed improbable, on the other hand, that {tgrh ršp} in RS 12.061:3-4 (*KTU* 1.78) contains the adverbial morpheme. There the {-h} is a pronominal suffix referring back to *špš*, viz., 'her gate-keeper (being) *Rašap*'.⁷¹²

— p. 325 (§54.323a), p. 568 (§74.422), p. 670 (§75.537e). There is certainly nothing wrong with T.'s proposal to analyze {tpnn} in RS 24.248:16 (*KTU* 1.104) as a D-passive, /YQTL/ 3 m.pl. (subj. *npšm*, 'outfits'), but he might at least have considered the possibility of taking it as D-active, energetic, 2 m.s.⁷¹³ But of course, as we have seen above (remark to p. 211 [§41.12], etc.) and shall see below, T. does not believe that any such forms are to be found in the prescriptive ritual texts.

— p. 325 (§54.323a), p. 372 (§64.22). Several remarks may be offered with respect to T.'s attempt to explain *ḥšt* in RS 1.001:10 (*KTU* 1.39) as an "Ortsangabe" rather than a common noun meaning 'half'. (1) As demonstrated above (remark to p. 123 [§32.146.23a]), *mlth* cannot mean 'half' and thus one of T.'s apparent motivations to discredit that meaning for *ḥšt* disappears. (2) The interpretation of *ḥšth* as 'half of it (i.e., the preceding grain offering)' is not so "kontextuell unwahrscheinlich" as T. would have it: generations of scholars have so interpreted the passage⁷¹⁴ and I had no difficulty accepting that meaning in my recent commentary of the text.⁷¹⁵ (3) T.'s attempt to find the word {ḥšt} in another passage, RS 24.296A:11' (*KTU* 1.136:11), must be rejected: as has been registered in *CAT*, the reading of the line is {lilzšt[...] }⁷¹⁶ and if one wishes to read {ḥ} for {z}, that constitutes an

⁷¹¹Pardee, *Les textes rituels* (2000) 590, 591.

⁷¹²Ibid., pp. 416-21, with previous bibliography.

⁷¹³*Les textes rituels* (2000) 567, 570. My hesitant proposal on p. 570 and in the glossary (p. 1198) to see in {tpnn} an L-stem form is not supported by any data gathered here by T.: all such forms are from hollow or geminate roots.

⁷¹⁴The situation is complicated by the fact that the word was originally read as {ḥš'}. The first to have suggested the reading with {t} was, as nearly as I can determine, Ginsberg (*Kitve Ugarit* [Jerusalem: Bialik Foundation, 1936] 112), but without a supporting note. Since that date, all the translators and commentators whom I have consulted (see bibliography in *Les textes rituels* [2000] 16) have taken the word as meaning 'half'.

⁷¹⁵Ibid., pp. 19, 64; in English, *Ritual and Cult* (2002) 68.

⁷¹⁶T. remarks that this reading is new in *CAT* without indicating that it was also present in the transcriptions of Ugaritic texts by Bordreuil and myself that we made available to him for the purpose of including the data therefrom in this grammar.

emendation.⁷¹⁷ (4) T. offers no better etymology for the word than the obvious comparison with Hebrew *ḤṢY*.

— p. 325 (§54.323c). Perhaps {ḥdṭh} in RS 24.248:18 (*KTU* 1.104) does not mean “bei Neumond” but ‘anew, again’.⁷¹⁸ For a more detailed remark on this passage, see above on p. 211 (§41.12), etc.

— p. 325 (§54.323c), p. 332 (54.423c). {ḥdṭm} in RS 19.015:13 (*KTU* 1.91) probably does not mean “zu Neumond” and is not linked to the next line because in lines 3-20 of this text each line is devoted to a separate festival unless there be an epigraphic indication of the contrary (l. 14). Rather, the {-m} marks the plural and refers to a series of new-moon festivals.⁷¹⁹ The basic form of the entries in lines 3-20, where the sacrificial festivals are cited by a name or catch-phrase, also speaks against seeing {ḥdṭm} as the equivalent of {b ḥdṭ} (p. 332) in the standard ritual texts: only one festival-name is introduced by *b* in this text, and there the meaning is local (l. 15); the only names that incorporate a temporal element are two that begin *k* + ‘RB, ‘when such-and-such a deity enters’ (ll. 10, 11), and they refer to liturgies that are not defined in terms of a day of the month. Because none of the other temporal expressions that consist of *b* + day of the month occurs in this text, that is probably not the function of *ḥdṭm*. Finally, as is clear from a statistical analysis of the ritual texts, the offerings at the new moon, though undoubtedly ideologically important, were not particularly numerous,⁷²⁰ and grouping these festivals for administrative purposes, as here, may be considered a reasonable procedure.

— pp. 326-35 (§54.4). T. devotes these ten pages to a presentation of examples of a locative case for which the internal proofs are limited to two: {šbù špš}, ‘at the rising of the sun’ (RS 1.003:47, 53, paralleled elsewhere by {šbà špš}⁷²¹) and {spûy}, ‘as a source of devouring’ (RS 2.[009]⁺ vi 11 [*KTU* 1.6]). At the beginning of the proofs (p. 326 [§54.412]) are also listed the four cases of the infinitive used ‘absolutely’ from III-’ verbs and which show {û}, hence an ending in a /u/-vowel. There is no obvious reason why the latter should be ‘locative’,⁷²² and even less that the narrative use of the infinitive, of which he lists three examples with /’/ that show {û}, should be ‘locative’ (below, p. 335 in an “Anm[erkung]” to §54.423g, T. suggests that the locative may be the regular case of the narrative infinitive). Moreover, neither of the first two examples is “locative” in the strict sense of the term (the first is temporal, the second the apparent equivalent of a purpose clause [*ytn b’l ḥḫym spûy*, ‘Ba’lu has given my own brothers that I should devour (them)’]). The question must,

⁷¹⁷Pardee, *Les textes rituels* (2000) 755.

⁷¹⁸Ibid., pp. 567, 570-71.

⁷¹⁹Ibid., pp. 491, 408-9.

⁷²⁰Ibid., pp. 925-27, 1052-66.

⁷²¹Ibid., pp. 199-202.

⁷²²T. asserts below (p. 329 [§54.422]) that in the comparable Akkadian construction the *parāsu(ma)* forms are locative, but provides no proof to that effect, not even to the extent of citing an Akkadian grammar. Even admitting the correctness of the identification of the Akkadian forms, however, that analysis may be correct for Ugaritic only if the locative case (still) existed there.

therefore, remain open as to whether Ugaritic had an adverbial case in *-ũ/* or whether the infinitive used ‘absolutely’ was in the nominative case and the first two cases cited represent nothing but this ‘absolute’ use of the infinitive. T. lays out his methodology explicitly on p. 328 (§54.421): because the locative case is difficult to distinguish from the adverbial accusative, the most likely Ugaritic examples will be those that are similar to locative formulae in Akkadian, where the writing system allows it to be isolated. The methodology would be acceptable if the existence of the case itself were more solidly established as a productive grammatical category: the basic problem is that all of T.’s examples except those already discussed in this remark could be adverbial accusatives. T. recognizes the questionable nature of the methodology (“methodologisch fragwürdig”) but prefers setting up a long list of questionable Ugaritic examples over simply saying that the case may exist but that there is no way of determining whether that is in fact the case nor how widespread its use may have been. It must be said that, because of the linguistic separation between Akkadian and Ugaritic and because of the dubiety regarding the very existence of the case in Ugaritic (beyond, perhaps, a few frozen forms), the Ugaritic examples cited have nothing but heuristic value and should not have been cited in a grammar of the Ugaritic language. In his conclusion to this section (p. 335 [§54.424]), T. asserts that, because the locative is not exclusively used with prepositional forms (on the difficulties of such forms that T. claims to exist, see below), the case must be “zumindest bedingt produktiv.” I would say that the two cases of nouns with {ũ} are at the inner limit of being sufficient to prove the existence of the case itself, let alone its productive nature. As an illustration of the problematic nature of the methodology, one may think of what could be done with the dual in Biblical Hebrew if the language were written entirely defectively, had no tradition of vocalization, and were as poorly attested in texts as poorly preserved as is the case of Ugaritic: working as does T., one could easily come to the conclusion that the dual was a productive grammatical number in Hebrew whereas, in spite of provable dual forms being far more numerous than are even debatable examples of the Ugaritic locative case, the dual number is indubitably only a grammatical relic in Hebrew.

— p. 326 (§54.412), p. 482 (§73.513.5). As we have just seen, in §54.412 T. presents the */-u/* ending of the infinitive in the ‘absolute’ or paronomastic construction (e.g., {gmũ gmit}, ‘are you indeed thirsty’, in RS 2.[008]⁺ iv 33’ [KTU 1.4]) as representing the locative case; that analysis is the only one indicated there. In §73.513.5, on the other hand, this morpheme is identified as “Lok[ativ] oder Nom[inativ].”

— p. 326 (§54.412). RS 16.394 is too badly damaged to allow for a certain analysis of {yşũ} in {[...][w] yşũ . ʾn[k] [...]} (l. 40’ = KTU 2.31:36) as an infinitive: it could be a participle.

— p. 327 (§54.415), p. 333 (§54.423d), p. 771 (§82.39), p. 874 (§93.422.3), p. 892 (§96.22b). There is nothing in RS 1.005 (KTU 1.43) to lead one to believe that *p’nm*, ‘on foot’, in lines 24 and 25 means “barfuß” (the interpretation preferred pp. 327, 333, over “zu Fuß”; on pp. 874, 892, the translation is “barfuß” with no indication of an alternative).⁷²³

⁷²³One may doubt that *b p’nh* (// *b ħrz’h*) in RS 2.[012] i 40’ (KTU 1.12) means simply ‘on foot’, as T. claims (p. 333).

The point of the expression is that the king, who would normally, when on outings, have ridden or been carried, must participate in this procession on foot.⁷²⁴ Nor does it appear likely to me that the form is in the locative case and that the *-m* is enclitic (both preferred explanations): the king has to walk on both feet, not on only one, so the form is more plausibly simply the dual, and the case may be the oblique (= accusative), rather than the poorly substantiated locative, supposedly an instrumental locative (p. 333). Finally, we have another case of indecision here, for on p. 771 T. translates “zu Fuß” with no indication of the interpretation preferred above and below.

— p. 328 (§54.416). It cannot be admitted that “Eine Hilfe bei Identifikation [des Lokativs] ist das häufig als Erweiterung des Lok. bezeugte enklitische *-m*.” The enclitic *-m* is attached to all parts of speech, and it provides no help whatever in distinguishing between an adverbial accusative and a putative locative.

— p. 328 (§54.417). Akkadian usage of *ina* + noun in locative case is insufficient basis for identifying the same syntax in Ugaritic. Not only is the very existence of the case itself as a productive grammatical category dubious, but there is no reason to doubt that *b* would have been followed by the genitive in all cases. Claiming that such is the case because one gets the interchange of forms with and without *b* in a given passage is insufficient; these may simply represent the interchange of adverbials expressed by the preposition and by the case system (accusative or locative).

— p. 328 (§54.417), p. 332 (§54.423c), p. 832 (§89.27b). T.’s analysis of {b‘lm} in RS 24.643:3 and 11 (*KTU* 1.148) represents one of the more flagrant and regrettable errors of the book. Because these sacrificial sequences correspond directly to the deity lists in which the multiple hypostases of *Ba‘lu* are written {b‘lm}, because that sequence corresponds formally to {dIM 2} through {dIM 7} in the Akkadian versions of this deity list, that sequence of signs in RS 24.643:3-4 and 11-12 can only mean ‘(for) another *Ba‘lu*’, not “am nächsten Tag.”⁷²⁵ ‘*lm* alone means ‘on the next day’ (see below, remark to p. 331 [§54.423a]), and it is unthinkable that the otherwise unattested formula *b ‘lm* would occur multiple times, one after the other, in this text (T.’s analysis requires that the text read ‘in on the next day, in on the next day, in on the next day’, etc., with no intervening liturgical act of any kind). To the extent that T.’s analysis is based on *KTU/CAT*’s unacceptable reconstruction of line 3 with only two tokens of {b‘lm}, by which a discrepancy is created between this text and the corresponding deity list, it must be observed that the epigraphic situation was clarified in detail as early as 1992⁷²⁶ and had been described more briefly four

⁷²⁴For this interpretation and bibliography, see Pardee, *Les textes rituels* (2000) 259-60.

⁷²⁵*Ibid.*, commentaries of RS 1.017, RS 24.264, and RS 24.643, where the alphabetic and syllabic data are outlined and discussed (see in particular the handy chart on p. 292). T. refers to the correct interpretation as an alternative on p. 332; on p. 832, he presents it with a question mark among “sonstige (mögliche) Belege” of enclitic {-m} and refers the reader back to §54.423c.

⁷²⁶Pardee, *Syria* 69 (1992) 160; cf. *Les textes rituels* (2000) 792; *BASOR* 320 (2000) 67. In this case as in so many others, T.’s data are not presented consistently: on p. 332, he says that {b‘lm} occurs five times in lines 3 and 4, six times in lines 11 and 12, whereas on p. 832 he says that the sign sequence occurs “jeweils sechsmal” (which is the correct description of the situation). Nowhere does he say that lines 3 and 4 are largely reconstructed.

years earlier.⁷²⁷ Many things are uncertain in Ugaritic, but choosing to ignore one of the comparatively few interpretations of a passage that are established by specific textual correlations and replacing that well-established understanding of the text with one that has no parallels and makes no sense can only discredit the grammarian. Furthermore, when this is done in the interest of establishing a dubious grammatical category (the locative case) and an even more dubious syntactic category (the use of the locative case with a preceding preposition), one's faith in the grammarian's objectivity is shaken.

— p. 328 (§54.417), p. 332 (§54.423c). *āḥr* is too well known as an adverb meaning 'thereafter' to give serious consideration to T.'s proposal to take it as a preposition followed by the noun in the locative case in RS 2.[003]⁺ iv 33, 46 (*KTU* 1.14).

— p. 328 (§54.417), p. 333 (§54.423d). The simple fact that the Ugaritic expression is *l bl* ('without') whereas the Akkadian expression cited as support for taking *bl* as in the locative case has *ina* should give the grammarian pause. As an example of the formula consisting of preposition + locative case it must be judged worthless.

— p. 330 (§54.423a), p. 739 (§81.11e), p. 799 (§83.231b), p. 853 (§92.232a), p. 885 (§95.11). T.'s treatments of *p* in the phrase *w yd ilm p* in RS 4.475:11-12 (*KTU* 2.10) is more ambivalent than is required by the text itself. In the first section cited he claims that the adverb *p*, which appears only in this passage, may have had the locative case attached to it, but the very existence of the case is in doubt, even more its unnecessary attachment to a lexical adverb. In the second, he identifies *p* as an adverb, but says that it may be a conjunction, provides a cross-reference to the section on the conjunction *p*, but, when one goes to that section, one finds no reference to this passage. T.'s basic interpretation of lines 11-13 (*w yd ilm p k mtm 'z mīd*) is: "Une die 'Hand'/'Hände' der Götter ist/sind hier(?) sehr stark, (so stark) wie Menschen / wie der Tod" (pp. 739, 799); his ambivalence regarding the status of *p* is expressed by replacing *hier* by ellipsis points on pp. 853, 885. Only once, on p. 885, does he attempt to explain why the predicate adjective '*z* does not agree with the noun it is supposed to be modifying, viz., *yd*, a feminine noun. There he proposes two explanations: (1) "Genusinkongruenz"; (2) *yd* would be "Pl.cs." and '*z* a /QTLa/ verbal form rather than an adjective. The first would involve a simple scribal error, while the second is out of the question as stated: the plural of *yd* is in all probability *ydt*,⁷²⁸ as in Hebrew, and, however that may be, it is not the plural that is used to express two paired body parts when

⁷²⁷Pardee, "An Evaluation of the Proper Names from Ebla from a West Semitic Perspective: Pantheon Distribution According to Genre," *Eblaite Personal Names and Semitic Name-Giving. Papers of a Symposium Held in Rome July 15-17, 1985* (Archivi Reali di Ebla Studi, vol. 1; Rome: Missione Archeologica Italiana in Siria, 1988) 119-51, esp. p. 138 n. 84.

⁷²⁸A word *ydt* of which the meaning is unclear appears in two economic texts (RS 18.024:9 [*KTU* 4.158] and RS 94.2600:8 [preliminary presentation in Bordreuil and Pardee, *Manuel* (2004), text 48 in the *Choix de textes*]), in the first case as the designation of a commodity going for ten shekels per unit (see Pardee, *Syria* 77 [2000] 27), in the second apparently preceding the commodity in question and hence perhaps meaning 'portions'. The latter meaning appears to be attested for *ydt* in poetry (RS 2.[022]⁺ i 21 [*KTU* 1.5], RS 24.293:11 [*KTU* 1.133]); T. analyzes this form as a plural (p. 284 [§52.5c]). The problem with this interpretation of the word in RS 94.2600 is that one would expect a standardized measure in an economic text, and there is as yet no evidence that *ydt* would have functions as the designation of a standard measure.

referring to many possessors but the dual. So stated, the proposal is morphologically plausible (/wa yadā ʾilīma pā ka môtima ‘azzā maʾda/ ‘the two hands of the gods are here, like death they have proven to be very strong’). However, as a consultation of the comparative literature shows, the word for ‘hand’ in the idiom ‘hand (of a deity)’ = ‘disease’ is normally singular in both Akkadian and Hebrew.⁷²⁹ All the more remarkable, then, that T. does not even mention a syntactic analysis of the passage that goes back at least to 1934⁷³⁰ and which solves the apparent problem of absence of gender agreement by taking *mtm*, understood as singular + enclitic *-m*, as the subject of the nominal phrase. This entails taking *k* as the conjunction rather than the preposition: /wa yadu ʾilīma pā kī môtuma ‘azzu maʾda/ ‘Moreover, the hand of the god is here, for death is very strong’.⁷³¹

— p. 330 (§54.423a), p. 739 (§81.12c), p. 740 (§81.12d). T. has garbled the data regarding the word for ‘there’ in RS 18.040:15 (*KTU* 2.40): in the first two sections cited, the form is given as {*tmn*} (the reading in *KTU*) and that form is said to appear in any context, while, in the third section cited, the form cited is {*tmny*} (this form appeared in the transcription that I made available both to the authors of *CAT* and to T.) and that form is said to appear only in the second part of the double formula of well-being (see remark above to p. 197 [§33.322.42a], etc.). RS 18.040:15 does indeed appear to contain {*tmny*}, but, as is clear from the citation on p. 739 (*w tmny¹ ydbh*, ‘and there he is sacrificing’), T. must have been aware that the passage does not correspond to the request for news addressed to one’s correspondent.

— p. 331 (§54.423a), p. 332 (§54.423c), p. 746 (§81.22i). Having proposed on p. 328 (§54.417—see remark *ad loc.*) that *b ʾlm* would be a doubly marked adverbial which would mean “am nächsten Tag,” T. proposes on p. 331 that *ʾlm* alone would mean “obendrein, ferner,” then on pp. 332 and 746, that it would mean “am folgenden/nächsten Tag” (he is not referring to distinct usages, for the same texts are cited for both meanings). The latter is indubitably the correct analysis: *ʾlm*, without *b*, was clearly how one said ‘on the next day’ in Ugaritic. The analysis of the use of the term in the ritual texts led me to that conclusion,⁷³² which is now confirmed by an epistolary example.⁷³³ Of the two proofs cited by T. for the meaning ‘on the next day’, one is valid, the other is not: (1) *ʾlm* does indeed precede *b ʾlt*, ‘on the third day’;⁷³⁴ (2) the sequence {*ym ʾlm*} in RS 1.003:8 (*KTU* 1.41)//RS 18.056:9

⁷²⁹Pardee, *Pope* (1987) 67-68.

⁷³⁰Albright, *BASOR* 54, p. 26; other references are cited on p. 68 of my article cited in the previous note.

⁷³¹{*ilm*} is vocalized and translated as singular + enclitic *-m*, just like *mtm*, because the reference in the Akkadian and Hebrew texts is normally to a single deity. Whether that deity is identified as Mot in the following phrase or *yd ilm* is generic in nature (like *qāt ili* in Akkadian) and *mtm* is a common noun cannot be determined. For this interpretation of the sentence in context, see Pardee, *Context* III (2002) 107-8; Bordreuil and Pardee, *Manuel* (2004) II 81.

⁷³²Idem, *Les textes rituels* (2000) 168-69.

⁷³³Cf. idem, *BASOR* 320 (2000) 70; *JAOS* 121 (2001) 136; the text in question, RS 94.2406, has now appeared in preliminary form in Bordreuil and Pardee, *Manuel* (2004), text 31 in the *Choix de textes*.

⁷³⁴T. cites RS 24.291:13, 22 (*KTU* 1.132); the same phenomenon occurs in RS 24.255: 13, 15 (*KTU* 1.111:14, 16).

(*KTU* 1.87), which T. translates “am Tag, am folgenden Tag,” is of dubious value because it is never repeated; moreover, the context of this occurrence is broken and there is simply no way of knowing the meaning or the syntactic function of {ym}.⁷³⁵ Finally, it is a bit ungenerous to say that the meaning “dann, ferner” is “traditionell” when the meaning accepted by T. was first proposed by de Moor in 1970⁷³⁶—though there is no doubt that the majority of scholars have preferred the meaning ‘next’.

— p. 331 (§54.423b). The only plausible “alternative” to taking the epistolary term *mrḥqtm*, ‘(from) afar’, as a locative is not that of seeing the {m-} as the preposition *mn* (pp. 762-63 [§82.22]), the existence of which in Ugaritic has not been proven, but that of taking it as an adverbial accusative.

— pp. 331-32 (§54.423c). Though it is unclear why T. decided to put his comment on the semantics of *šbū* as meaning “Sonnenaufgang” here rather than on p. 261 (§51.43c), where the word is presented as a /qatīl/ form, his reasons for adopting the meaning of ‘sunrise’, not ‘sunset’, match my own⁷³⁷; the essence of the argument goes back to 1983.⁷³⁸

— p. 332 (§54.423c). Unless the presentative particle *hl* be etymologically distinct from the presentative/locative particle *hlny* (a distinction which T. makes no effort to draw below pp. 750-51 [§81.4b-3]), the former cannot be thought to bear the locative morpheme /u/, for the latter is written in syllabic script with /i/ in the second syllable and, moreover, shows /u/ in neither the second nor the third syllables.⁷³⁹ It must indeed be considered dubious that these primitive particles, as opposed to primitive substantives that developed a secondary function as particles, should be forced to fit regularly into the substantival case system. Finally, the comparison of this particle with Hebrew /h^alō/, which functions to introduce rhetorical questions expecting an affirmative answer, is misguided no matter how popular (as T. himself recognizes below, p. 750 [§81.4a]): both the semantics of the Hebrew form, as just described, and the morphology, viz., without geminated /l/, show it to be distinct from Ugaritic /halli-/ and Canaanite “*allū*” (as T. normalizes the Canaanite particle, following Rainey—the syllabic writing permits either /hallū/ or /^hallū/).

— p. 332 (§54.423c). ‘*Im* ‘*Im* in RS 24.253:32 (*KTU* 1.109) may mean ‘on the day after next’⁷⁴⁰ rather than “am folgenden (und) am folgenden (Tag).” The repetition of ‘*Im* is unattested elsewhere and its precise meaning is for that reason uncertain.

— p. 333 (§54.423d), p. 500 (§73.611.2d), p. 680 (§75.72a). Though the verb in the phrase *āmrmrn* ‘*š qdš* (RS 92.2014:2 [RSO XIV 52]) could contextually have the first meaning in

⁷³⁵On this text, see Pardee, *Les textes rituels* (2000) 150, 151. For additional arguments from the ritual texts for the meaning ‘on the next day’, see pp. 168-69. As noted above, the meaning ‘on the next day’ is now confirmed by an epistolary document (reference and citation are to be found in the publications cited in note 733).

⁷³⁶Bibliography in Pardee, *Les textes rituels* (2000) 168 n. 82.

⁷³⁷*Les textes rituels* (2000) 199-202.

⁷³⁸G. Garbini, “Note sui testi rituali ugaritici,” *OrAn* 22 (1983), p. 53-60, esp. p. 56.

⁷³⁹Nougayrol, *Ugaritica* V (1968), text 138 (RS 20.426B):05’ {al-li-ni-ya}, normalized as /halliniya/ by T. on p. 751; for the analysis of the polyglot entry, see Huehnergard, *Ugaritic Vocabulary* (1987) 68, 121.

⁷⁴⁰Pardee, *Les textes rituels* (2000) 604, 612-13.

the translation proposed on p. 500 (“ich reinige/banne (dich)”), I do not see how it could have the second, i.e., with an implied 2 m.s. object. Throughout this incantation, its beneficiary and he only is addressed in the second person and he is not, one would think, subject to banishment. ‘I purify (you with) sacred wood’ is, therefore possible in context, but ‘I banish (you) with sacred wood’ is not. T. does not even mention the interpretation that was proposed in the preliminary edition of the text, viz., ‘to move back and forth (transitively), to shake’.⁷⁴¹ That interpretation relies on the recognition of a verb MRR, ‘to pass (by)’ (it is highly unlikely that there was a geminate root in Ugaritic that meant “vertrieben” in the G-stem—see below, remarks to p. 600 [§74.626.2], etc., p. 605 [§74.632], etc., and to p. 673 [§75.62a], etc.). The “dich” is, in any case, misplaced because the second-person forms in this text refer to the person in whose favor the incantation is proclaimed, not the banished entity.⁷⁴² T. never explains to what root the meaning “reinigen” would be attached and I know of none (the only one that appears to come even close is MR, ‘to bless’, on which see the remarks just cited and, more particularly, to p. 540 [§74.35], etc.).⁷⁴³

— p. 334 (§54.423e), p. 366 (§63.123), p. 419 (§69.43). T. gives radically different interpretations of *w npš bt tn tlt māt* (RS 13.014:16 [KTU 4.123]): (1) “300 rote Madchenkleider im (Wert von) 300 (Schekeln)” (p. 334) and (2) “und die Ausstattung des zweiten/eines weiteren Hauses” (p. 366, translating only the first four words), “ein weiteres/zweites Haus” (p. 419, translating only *bt tn*). The semantic field of *npš* is very broad, from a ‘garment’ to the collection of items necessary for a function, including garments, vessels and tools; it thus corresponds reasonably closely to English ‘outfit’ which has a similar range of meaning (cf. British English ‘kit’ which can denote a set of clothing, as in ‘evening kit’, whereas in American English it tends to denote a collection of tools). This would be the only text where *npš* is used to express the ‘outfitting’ of a house. The function of *tlt māt* at the end of line 16 is certainly to number something in this line (i.e., this line is syntactically independent of the preceding and the following lines), but that something could be *npš*, *tn* or *bt tn*. In the last two cases *npš* could be a summary term for the list of following

⁷⁴¹Pardee, *Les textes rituels* (2000) 829-31; cf. now, *Ritual and Cult* (2002) 159; this was the interpretation indicated in the proofs of the edition of the texts from 1986-1992 that were made available to T. (“J’agiterai du bois sacré”) and which has now appeared in Bordreuil and Pardee, RSO XIV (2001) text 52. Ford has recently proposed that lines 1-5 (*ūzb ... ’qrb*) would be the words of the evil sorcerer (*UF* 34 [2002] 119-52), but that structural analysis is based in no small part on an analysis of *ūzb* that I cannot accept (*ū* would be an emphatic particle: see remarks to p. 196 [§33.322.3c], etc., and to p. 805 [§84.22]). Furthermore, Ford has no proposal for the meaning of *zb*, which constitutes a second major difficulty with this re-analysis of the passage. On the possible analysis of *zb* as a stative verbal adjective, rather than as a /QTLa/ perfective, see below, note 1220.

⁷⁴²See the complete text provided in the studies to which reference was made in the preceding note and add now Bordreuil and Pardee, *Manuel* (2004), text 18 in the *Choix de textes*.

⁷⁴³In his study of RS 92.2014 in *UF* 34 (2002) 119-52, Ford proposes that *āmrmr* would mean ‘to make bitter’, in context, ‘to produce venom’. I have no particular objection to this analysis of *āmrmr*, for it would be based on the root MRR ‘to be bitter’ that is well attested in Ugaritic (see remarks below to p. 601 [§74.626.3b], etc., and p. 673 [§75.62a], etc.). This structural analysis is, however, part and parcel of his overall interpretation of the passage of which the principal difficulties are described in note 741.

items and it would be used either absolutely (‘list of goods’) or in construct (‘the goods of the house/daughter’). In the first of these possibilities, *bt* could denote a type of textile⁷⁴⁴ (‘And the inventory: *bt*-textile dyed scarlet, 300 units ...’).⁷⁴⁵ The problem here is that nothing in the text indicates to whom this list of items belongs. According to the second of these possibilities, there is no indication of to whom the house or the daughter belongs whose goods are being inventoried, unless it be the name that occurs before the horizontal line that separates lines 16-23 from what precedes (*bn grgš*), but this does not appear plausible. Moreover, the mention of ‘wine’ in the last two lines renders dubious any hypothesis which sees *npš* as a summary term for the items named in lines 16-23; it appears preferable, therefore, to see line 16 as one entry among others. Since there is no obvious identification of *bt* taken as ‘daughter’ (if the phrase is generic, as T.’s first translation supposes, why would it not be *npš ātt*, ‘women’s garments?’) and since the meaning of the translation ‘the *npš* of the house’ would not be at all clear, it appears more plausible to take *npš bt tn* as a unit and to translate the line as ‘three hundred *npš*-garments of *bt*-textile (dyed) scarlet’.

— p. 334 (§54.423f), p. 671 (§75.538). T.’s decision to place *štm* in RS 2.[022]⁺ i 25 (*KTU* 1.5) among the “Nicht sicher deutbare Formen” on p. 671 appears to belie his confident analysis of the form on p. 334 as an infinitive in the locative case functioning as a purpose clause.

— p. 335 (§54.51). {MEŠ} in {DINGIR.ma-lik.MEŠ} (RS 20.024:32)⁷⁴⁶ does not designate the Ugaritic plural /ūma/, for there is no reason to believe that /malik/ is a Ugaritic word. Rather, the entire entry is the Akkadian translation of Ugaritic *mlkm*, of which the vocalization was plausibly /malakūma/, and {MEŠ} thus designates the Akkadian plural morpheme.⁷⁴⁷

— p. 347 (§62.132a), p. 374 (§64.32). T.’s first interpretation of *tltt w tltt ksph* in RS 15.062:13 (*KTU* 4.158) as “3 1/3 (Schekel) ist sein (Preis in) Silber” appears not to correspond as well to the structure of the text as his second, viz., “je drei (Schekel).”⁷⁴⁸

— p. 347 (§62.141). No reason is given for the reconstruction of the feminine form of the number noun {ārb‘t}, ‘four’, as only /‘arba‘at-/ with no indication of the possibility that it may have been /‘arba‘t-/ (on the general problem see above, remarks to pp. 182-85 [§§33.242-243], etc.)—in this case, proto-Hebrew had both with the distribution in Biblical Hebrew according to state, absolute and construct.

— p. 348 (§62.162a), p. 369 (§63.214), p. 385 (§67.32), p. 703 (§76.521.1). In order to explain the phrase {b tt ym ḥdt ḥyr} in RS 12.061:1-2, T. posits the existence of a noun

⁷⁴⁴J. Pasquali, “La terminologia semitica dei tessili nei testi di Ebla,” *Quaderni di Semitistica* 19 (1997) 217-270, esp. p. 257.

⁷⁴⁵ It seems highly unlikely that *tn* is the ordinal number ‘second’ because no ‘first house’ appears earlier in the text. Whatever division of these words is preferred, therefore, the interpretation of this word as corresponding to the well-attested term for a scarlet dye produced from a land-based organism (cognate with Hebrew *šānī*)—as opposed to the murex-based dyes—appears necessary.

⁷⁴⁶Nougayrol, *Ugaritica* V (1968) text 18.

⁷⁴⁷Pardee, *Les textes rituels* (2000) 311-14.

⁷⁴⁸Pardee, *Syria* 77 (2000) 36-37.

distinct from the number noun /tittu/, ‘six’, which would mean “Sechszahl”; it is translated in context “an der Sechszahl der Tage (d.h. am sechsten Tag) der Neumondphase (des Monats) Ḫiyaru” (p. 385). He rejects the interpretation of the passage by Pardee and Swerdlow as “during the six days of the (rituals of) the new moon of Ḫiyyāru”⁷⁴⁹ on the basis of the “parallele Konstruktion” in RS 24.256:10 (*KTU* 1.112). That text, however, reads {[b] ṣḫb^ᶜ ym ḥdt}, without the following month name (which was almost certainly indicated in line 1 of this ritual text), and the phrase in context must mean ‘on the seventh day of the new-moon (festival)’. It is the latter formulation that is anomalous in the ritual texts, for the simple phrase {b ṣb^ᶜ}, ‘on the seventh (day)’, is expected.⁷⁵⁰ This text, then, plausibly contains a particular construction, which may be vocalized /bi ṣabī^ᶜi yamī ḥudṭi/ and translated ‘on the seventh of the days of the new-moon festival’.⁷⁵¹ This type of construction is well attested in the mythological texts, e.g., {b ṣb^ᶜ ymm}, ‘on the seventh of days’, in RS 2.[004] i 15’ (*KTU* 1.17), which is preceded by a series of ordinal numbers (that they are ordinals is proven graphically by {rb^ᶜ} in line 8 and {ṭdt} in line 11).⁷⁵² {ṭṭ} in RS 12.061:1 cannot, on the other hand, be an ordinal number, nor can it be the /qatūl/-noun of which he posits the existence elsewhere (see preceding note), and the simplest interpretation is, therefore, simply, ‘during the six days of the new-moon (festival), i.e., the six days extending after the festival celebrated on the day of the new moon’.⁷⁵³ There is, therefore, no reason to invent a new noun which would have the value of an ordinal number. — p. 348 (§62.181). How does T. know that the feminine form of the number noun ‘eight’ was /ṭamānīt-/ , rather than /ṭamānat-/ as in proto-Hebrew (/š³mōnāḥ/)? On the general problem, see above, remark to p. 184 (§33.243.13-14), etc. — p. 349 (§62.192a), p. 364 (§63.112), p. 368 (§63.19). It is difficult to admit {w b ṭš^ᶜ .} as a “N[eue] L[esung]” (p. 368) of RS 24.248:11 (*KTU* 1.104) when previous editors have read the {^ᶜ}⁷⁵⁴; the break in the tablet runs through the putative {^ᶜ}, however, and the reading is epigraphically uncertain.⁷⁵⁵ The claim to a new reading apparently refers to the word-divider, which would confirm T.’s interpretation of *b ṭš^ᶜ* as meaning ‘on the ninth

⁷⁴⁹ *Nature* 363 (1993) 406.

⁷⁵⁰ For the rare structure *b ym* + ordinal number, see above remark to p. 261 (§51.43d), etc.

⁷⁵¹ Pardee, *Les textes rituels* (2000) 634, 638-39. T. posits the existence of a noun {ṣb^ᶜ} distinct from the standard /ṣab^ᶜ-/ to explain this passage (p. 261, §51.43d], p. 386 [§67.43]), but such an explanation is no more plausible here than in the mythological texts (see continuation of this remark).

⁷⁵² T. explains ṣb^ᶜ in this sequence as a /qatūl/-noun (see *ibid.*, as well as p. 365 [§63.113]), accepting, however, that the other number words in the sequence, including *rb^ᶜ* and *ṭdt*, are ordinal numbers. But the morpho-syntax just proposed works equally well for the ordinal number ‘seventh’ and it avoids the problem of a noun meaning ‘seven’ functioning as an ordinal number—this is a real problem, for in most cases the sequence appears in the literary *topos* that consists of detailing the seven days or months over which a situation extends. Only in a passage in the *Kirta* text, *ḥmš ... ṭdt yrḥm* (RS 2.[003]⁺ ii 30-31 [*KTU* 1.14] is *ṭdt* taken to be a /qatūl/-noun (see above, remark to p. 261 [§51.43d], etc.).

⁷⁵³ In addition to the brief note by Pardee and Swerdlow cited above, see now the full commentary by Pardee, *Les textes rituels* (2000) 416-27.

⁷⁵⁴ Herdner, *Ugaritica* VII (1979) 39; *KTU/CAT*, *ad loc.*

⁷⁵⁵ Pardee, *Les textes rituels* (2000) 566, 1279.