

(day)’, but, though I agree with this interpretation of *b tš*<sup>c</sup>, the reading of the word-divider cannot be admitted for the surface of the tablet has disappeared where the word-divider would have been.

— p. 352 (§62.202.5), p. 353 (§62.25). My collation of the tablet confirms T.’s epigraphic<sup>756</sup> and grammatical observations that the restoration of *l* between the ‘5’ element and the ‘10’ element in the number noun ‘15’ in RIH 83/07+:8 (*CAT* 4.777)<sup>757</sup> was incorrect: the break is indeed too narrow to consider restoring *l* and the reading {ḥmš [.]} ‘šrh} is required (with or without the word-divider—I restored it here because it is used between the two elements of the number ‘13’ in line 5).

— p. 354 (§62.3). Two remarks are in order apropos T.’s assertion that the number ‘20’ in Ugaritic is the plural of ‘šr, ‘10’: (1) there is no internal proof that the form is not dual (‘20’ should in theory be the dual, not the plural, of ‘10’);<sup>758</sup> (2) if the form is indeed plural, the stem may be dissyllabic, not monosyllabic (e.g., /‘išarūma/, rather than /‘išrūma/). In favor of T.’s reconstruction in line with the other West-Semitic languages is the form *ttm*, ‘sixty’, which must be based on the monosyllabic stem /tīdt-/ rather than on /tīdat-/ . It is of interest that in the other West-Semitic languages, the number ‘20’ shows the unexpected plural ending (e.g., Hebrew /‘eśrīm/) whereas the other decades, if a /qatl/qitl/ base, show the unexpected singular/dual stem (/šib‘īm/, /tiš‘īm/). T. claims that the /-ā/ ending on ‘20’ in Akkadian, Old South Arabian, and Ethiopic is the feminine absolute rather than the dual, but provides no arguments to back up the assertion.

— p. 355 (§62.42b). Though the meaning of {ḥmšm . tlt . rkb . rtn} in RS 24.643:20 (*KTU* 1.148) is indubitably difficult,<sup>759</sup> T.’s interpretation of *rkb* as ‘wagon load’ is implausible, for thirty-eight wagon loads of whatever *rtn* is (perhaps some sort of textile<sup>760</sup>) is far out of proportion with the other offerings in this list.<sup>761</sup>

— p. 356 (§62.431a), p. 359 (§62.612), p. 360 (§62.632), p. 403 (§69.163.13), p. 408 (§69.211), p. 413 (§69.242b). Because the measure of the grains registered in RS 18.033 (*KTU* 4.345) is never stated, one can understand hesitation at indicating a hypothetical measure when translating this text. T. does not do so in the first three paragraphs cited but indicates it as the *dd* in the last three. The hypothesis of the *dd*-measure is plausible to the point of probability, and one wonders why T. has chosen to lead his readers to believe that the measure was uncertain in some cases while indicating it as certain in the others (§69.242b is devoted to administrative entries characterized by “Ellipse von *dd*”). A similar

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<sup>756</sup>See already Tropper, *AuOr* 13 (1995) 239.

<sup>757</sup>The restoration of *l* was indicated in the preliminary edition (Bordreuil, *Syria* 61 [1984] 2) and the reading was admitted by the authors of *CAT*.

<sup>758</sup>T. recognizes this implicitly below, p. 414 (§69.311), by including “20” under both plural and dual forms, each time with a question mark.

<sup>759</sup>Pardee, *Syria* 69 (1992) 164; idem, *Les textes rituels* (2000) 787, 794.

<sup>760</sup>References, idem, *Les textes rituels* (2000) 794 n. 67.

<sup>761</sup>The items are listed, *ibid.*, pp. 787, 957. I do not believe that the unit-measure of ‘wool’ in the following entry was the talent; cf. *ibid.*, pp. 24, 918 n. 5, 923 n. 20.

situation may be observed in the case of *tt māt rqh* in RS 11.795:4-5 (*KTU* 4.91), another text where the measure is not indicated explicitly: on p. 405 (§69.173.2) T. translates by “600 (Mass) Duftöl,” on p. 413 (§69.242b) by “600 (*kd*) Parfümöl.”

— p. 360 (§62.633), p. 362 (§62.813), p. 407 (§69.192.2b). T. translates *šmn nh* as “Schweinefett,” but does not give his reasons.<sup>762</sup> In the commentary on the passage that he cites on p. 360 (RS 92.2057:4 [RSO XIV 37]), the editors suggest a sort of ghee.<sup>763</sup> The phrase *šmn nh* was previously attested in RS 11.795:3-4 (*KTU* 4.91),<sup>764</sup> there followed by *šmn rqh*, ‘perfumed oil’. In RS 92.2057, the designation of the preceding commodity has disappeared from the tablet while the following is *ztn*, ‘olives’.

— p. 364 (§63.111). T. cites Akkadian, Arabic, and Ethiopic as a basis for reconstructing the ordinal numbers as formed on a /qātīl/ base, then concludes “Das Ug. unterscheidet sich somit ... vom kan. und aram. Befund ...” This positive presentation does not adequately convey to the reader the simple fact that we have no way of knowing whether Ugaritic had the more broadly attested form or the Northwest Semitic /qatīl/ form, or, for that matter, yet another. There can be no doubt, as T. demonstrates, that the Ugaritic base was dissyllabic (this is shown by {*tdt*}, ‘sixth’, in contrast with {*tt*}, ‘six’ = /*tittu*/ ← /*tidtu*/); and one might add that, since none of the forms shows any assimilatory phenomena, one of the vowels may be long, for a long vowel in either syllable would have impeded syncope and the subsequent assimilation, whole or partial, of one of the consonants. But that is all that one can say until one of the forms happens to be attested in syllabic script. As T. remarks in passing, the Ugaritic form certainly does not bear the *nisbe* that was a part of the ordinal numbers in Aramaic (/āy-/), Hebrew, and Phoenician (/iyy-/),<sup>765</sup> for this morpheme had retained the consonantal element {y} in Ugaritic; if one believes that the /qātīl/ base and this morpheme went part and parcel with each other, this fact constitutes an argument in favor of another base, and at that point the most widely attested, viz. /qātīl/, becomes the most likely candidate. T. does not, however, make this argument in so many words, perhaps because there is no reason, other than the fact of attestation in the other languages, to expect a link between the /qātīl/ base and the *nisbe* morpheme. Another datum not exploited by T. that

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<sup>762</sup>In 1995, T. identified Ugaritic *nh* with Akkadian *nāḫu*, “(Schweine-)Schmalz” (*ZA* 85, p. 64). If *nh* already represented a sort of fat, however, one would not expect it to be preceded by *šmn*. The latter word, when it designates an animal fat, is followed by the name of the animal: *šmn ūz*, ‘goose-fat’ (RS 16.399:22 [*KTU* 4.247]) and *šmn ālpm*, ‘fat of bovids’ (RS 94.2405:1—the reason for the plural *ālpm* is unclear though it may reflect the usage in the text itself, viz., the heading of a list of quantities of fat brought in from various villages).

<sup>763</sup>Bordreuil and Pardee, RSO XIV (2001) 353 n. 9.

<sup>764</sup>The reading was Herdner’s in *CTA* 141; in the *editio princeps*, *Syria* 21 (1940) 274, Virolleaud read {m(?)nh}, but his copy of the {n} shows only two wedges; what he took as two signs is in fact a four-wedged {n} of which the first is somewhat damaged.

<sup>765</sup>For this reconstruction of the *nisbe* ending in Hebrew (and Phoenician), see above, remarks to p. 197 (§33.322.42c) and to pp. 273-74 (§51.46h-k).

may favor his reconstruction is the ordinal ‘eighth’, which is written {ṭmn} and which one might have expected to have appeared as {ṭmny} if the base form were /ṭamānīyu/.<sup>766</sup>

— p. 364 (§63.112), p. 366 (§63.124), p. 380 (§65.22), p. 700 (§76.426). T. has properly attached a question mark to his listing on p. 364 of *tnt*, the feminine form of the ordinal number ‘second’, as appearing in RS 2.[003]<sup>+</sup> i 15 (*KTU* 1.14), and he refers on p. 366 to the reading of the passage as “umstritten.” The re-reading of the line as containing *tnt ūn*, “eine zweite wurde ihm zu(m Anlaß) einer Totenklage,” is, however, T.’s alone<sup>767</sup> and goes against the reading of all preceding editors of the text, some of whom have worked on the tablet itself, as {ṭāṛ ūṣm}.<sup>768</sup> So shaky a basis for the attestation of a form indicates that its mention should have been relegated, at most, to an *Anmerkung*. In fact, none of T.’s examples of {ṭnt} as an ordinal number or an iterative adverb stands up to examination. The reading {ṭnth} in RS 1.005:13 (*KTU* 1.43) must be rejected (see above, remark to p. 321 [§54.315.1], etc.), while {ṭnt} in RIH 77/18:18' (*CAT* 1.175), a medical text that prescribes drinking on the part of the sick person (see below, remark to p. 646 [§75.522], etc.), may mean ‘urine’. The proposal to find the ordinal in lines 2 and 4 of the administrative text RS 17.370[D] (*KTU* 4.305) deserves no place in a serious grammar: this word is the only one preserved in each of these lines and the “preferred” interpretation as ‘scarlet dyed stuff’ is really the only plausible one as that word is common in these texts while this form of ‘second’ has yet to see its first certain attestation.

— p. 365 (§63.122), p. 689 (§76.342), p. 698 (§76.421a). T. accepts with no sign of doubt the reading of RS 2.[004] ii 44' (*KTU* 1.17) as “*yrḥ yrḥ ṭn yṣi*” in spite of the fact that most of

<sup>766</sup>T. assumes p. 368 (§63.18) that the Ugaritic form, like the corresponding form in Arabic, showed no trace of the fourth root consonant, i.e., that it was vocalized /ṭāminu/, not /ṭāminû/; because the root was clearly quadriconsonantal, however, a more likely reconstruction for Ugaritic would show the contraction. My vocalization of the only certainly attested form, {ṭmn} in RS 24.250<sup>+</sup>:18 (*KTU* 1.106), as /ṭamīnû/ was in error: assuming the Northwest-Semitic base as I was, the form should have been /ṭamānî/ (← /ṭamānīyi/ [genitive case]). For another possible case of *ṭmn* functioning as an ordinal number (RS 24.248:7), see above, remark to p. 261 (§51.43d), etc.

<sup>767</sup>First proposed in *UF* 27 (1995) 530-31, then again in *AfO* 42-43 (1995-96) 269.

<sup>768</sup>Dietrich and Loretz, after reading {ṭāṛ ūṣm} in both *KTU* (p. 38) and *CAT* (p. 36), have more recently (*UF* 31 [1999] 149) muddled the waters by reading {ṭt rūm} and declaring that to be Virolleaud’s original reading as recorded on his copy which would have been in contradiction with his transcription of the second sign as {ā} (the reading has recently been accepted by G. Mazzini, “A New Suggestion to *KTU* 1.14 I 15,” *UF* 34 [2002] 560-575). The assertion is, however, simply false: examination of Virolleaud’s copy shows a first clear head of a horizontal wedge, followed by a tick on the upper part of the continuation of the sign which one must interpret, given that he read {ā} (pp. 34 and 52 with a question mark; on p. 55 the reading is described as “peu distincte”), as his way of indicating a damaged head of a second horizontal wedge, i.e., {ā}: *La légende de Keret roi des Sidoniens publiée d’après une tablette de Ras-Shamra* (Mission de Ras Shamra 2; Bibliothèque Archéologique et Historique 22; Paris: Geuthner, 1936), pl. I; copy reproduced by Herdner in her re-edition of the text (*CTA* [1963], fig. 36), who also followed the original editor’s transcription (p. 62, with note 1 affirming the correctness of the reading). When this basic error of representing the editor’s reading is followed by a highly dubious etymology for the word {rūm} (p. 150: Akkadian “*ru*” ‘*umu* D etwa ‘ab, wegschlagen”), one loses all faith in their objectivity here. Furthermore, I was able to collate the tablet in June of 2003, and Virolleaud’s original reading appears to me to be the only possible one (I have even dropped the half-brackets in my transliterations of all five signs—see now Bordreuil and Pardee, *Manuel* [2004] II 20).

the signs were marked in *KTU* as uncertain while each of the last three was followed by a question mark (in *CAT*, the only sign of doubt is the use of Roman script). The most likely reading of the line is {yrḥ . yr<sup>l</sup>-l[-(-)]<sup>l</sup>-l . ḥ<sup>l</sup>lh<sup>l</sup>}, where the sixth letter cannot be {ḥ}, the one to the right of the break is probably not {n}, and the last three signs can only with great difficulty be read {yṣi}.

— p. 365 (§63.123), p. 409 (§69.222). The ordinal adjective *tn*, ‘second’, is not present in RS 24.266:18' (*KTU* 1.119) because the reading may not be admitted.<sup>769</sup> Moreover, the word *tn* is, in my estimation, more plausibly interpreted as the cardinal number ‘two’ in RS 1.005:14 (*KTU* 1.43).<sup>770</sup> T. objects to this interpretation because the ‘two shekels’ is usually expressed by the dual of *tql* without the number noun ‘two’. Because *tn* is used with other nouns and because *tql* is in the construct here, with the resultant loss of the {-m} of the dual, that difficulty appears less important to me than the one that is raised by attempting to determine who ‘the second *Gataru*’ would be in this passage.<sup>771</sup>

— p. 366 (§63.123). By the standard rules of West-Semitic morpho-syntax, <š>d *tn nḥlh* (RS 18.046:10 [*KTU* 4.356]) cannot mean “ein zweites/weiteres <Fe>ld seines Erben” (an adjective cannot be placed between two nouns in a construct chain). Because the line is not treated under syntax in this grammar, there is no way of knowing how T. would analyze the line in terms of Ugaritic syntax. I see two possibilities: the phrase in fact means either (1) ‘the field of his two heirs’ (/šadû ṭinê naḥlēhu/), or (2) ‘a field, the second one of his heir’ (/šadû ṭanû naḥlihu/). The latter may indeed have essentially the same meaning as T.’s translation, i.e., the phrase would not mean that the heir himself owns two fields, but that the principal party has two fields, one of which is exploited by his heir.

— p. 366 (§63.123). Because the sacrifice of ‘two male bovids’ is attested elsewhere, T. is probably correct in interpreting *tn ālpm* in RS 24.255:14 (*KTU* 1.111:15) as having that meaning. There was certainly another sign before {tn}, however, and the interpretation of these three signs cannot, therefore, be considered certain.<sup>772</sup> T. is certainly correct, on the other hand, in rejecting the interpretation of *tn* here as the ordinal number ‘second’ and as expressing the ‘second day’ of a sacrificial sequence because the latter concept is expressed by *ʿlm* in line 13.

— p. 366 (§63.13). Because the word for ‘donkey’ as the object of a sacrifice is *ʿr* in the ritual texts (RS 1.002:24', 36', 42' [*KTU* 1.40]; perhaps in RS 24.266:16 [*KTU* 1.119]) and because the other word for ‘donkey’, *ḥmr*, is nowhere else attested in the ritual texts, the restoration of {ḥ[...] } as {ḥ[mr]} in RS 24.255:15 (*KTU* 1.111:16) cannot be considered at all likely.<sup>773</sup>

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<sup>769</sup>Pardee, *Les textes rituels* (2000) 663.

<sup>770</sup>Ibid., pp. 218, 248; cf. *Rituel and Cult* (2002) 71.

<sup>771</sup>*Textes rituels*, pp. 240-41, 248-49.

<sup>772</sup>Ibid., pp. 619, 621. T. asserts that “Vor *tn* sind allerdings keine Spuren eines {b} zu erkennen,” but does not say explicitly that there are traces there of a sign but they are too poorly preserved to permit a reading.

<sup>773</sup>Cf. Pardee, *Les textes rituels* (2000) 626.

— p. 367 (§63.14), p. 409 (§69.222). T. is surely correct to resist the restoration {*tn* . [*dd* . *šmn*]} in RS 1.003:45 (*KTU* 1.41), which requires an emendation in the parallel text RS 18.056:50 (*KTU* 1.87), where {*l* . *šmn*} is perfectly preserved. He remarks that the *dd*-measure is never used for oil.<sup>774</sup>

— pp. 368-69 (§63.211). Because above, pp. 364-65, T. has not listed an ordinal number ‘first’,<sup>775</sup> it is a bit strange to see *āhd*, ‘one’, listed here as an example of a cardinal number used as an ordinal (“... in der Funktion von Ordinalia”). However that may be, the grammatical categorization is hardly a useful one as applied to the only text cited, {*lbš* . *āhd* | *b* . ‘*šrt*’} (RS 15.035:1-2 [*KTU* 4.146]), for the line translates straight-forwardly as ‘one *lbš*-garment for ten (shekels)’. Indeed, the whole basis for the analysis appears to be the following entry (*w* . *tn* . *b* . *hmšt*) which T. interprets as meaning ‘and a second for five shekels’, criticizing Ribichini and Xella for the translation “e due (vesti) per 5 (sicli d’argento).”<sup>776</sup> Granting that T.’s interpretation of *tn* is correct,<sup>777</sup> the English translation offered above is still perfectly comprehensible, and one gets the feeling that this example was provided because the word for ‘one’ is often used in lists that otherwise consist of ordinals (as in the Biblical-Hebrew creation story: ‘day one’, ‘the second day’, etc.).

— p. 369 (§63.212), p. 396 (§69.133.32b), p. 398 (§69.143.11). With regard to the interpretation of *tmnt* in RS 24.256:11 (*KTU* 1.112) as meaning ‘on the eighth day’, not only is the use of a cardinal number in place of an ordinal “bemerkenswert” (p. 369), it is unknown elsewhere in the ritual texts in this type of formula and for that reason highly questionable. On pp. 395-396, T. explains the peculiarity as owing to the ellipsis of *ym*, ‘day’, and as analogical to the regular use of a feminine form of the number in economic texts when *ṭql*, ‘shekel’, is omitted. None of this is convincing because the normal use in the ritual texts is to use the masculine ordinal number when the word *ym* is omitted. Because the following word is damaged and of uncertain interpretation, I did not dare, in my treatment of the text, to emend {*b tmnt* . *iy<sup>l</sup>-lm*} to {*b tmn tiy<sup>n</sup>lm*}, ‘on the eighth (day), figs ...’, but I was tempted to do so.<sup>778</sup>

— p. 369 (§63.213), p. 389 (§69.115). Given the regular usage just described of ordinal numbers to designate the days of the month in the rituals texts, T.’s diffident suggestion (p.

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<sup>774</sup>For other epigraphic and philological reasons, see Pardee, *ibid.*, p. 198 with n. 219.

<sup>775</sup>Below, p. 370 (§63.311), under the heading “Andere Lexeme in der Funktion von Ordinalia,” T. observes that “Das Ug. kennt keine spezifische Ordinalzahl für ‘erster,’” then goes on to cite the single prose text where *b ym pr<sup>c</sup>* is the first entry and is followed by designations of ‘the following day’ and then by days ‘three’ through ‘five’ (RS 19.156 [*KTU* 4.279]).

<sup>776</sup>*La terminologia dei tessili nei testi di Ugarit* (Collezioni di Studi Fenici 20; Roma: Consiglio Nazionale delle Ricerche, 1985) 76.

<sup>777</sup>Because the price of *lbš*-garments varies considerably, from one-and-a-half to ten shekels per garment (Stieglitz, *JAOS* 99 [1979] 19; cf. Pardee, *Syria* 77 [2000] 52), it is impossible to say whether the price of five shekels or two and a half shekels is the more likely. The strongest indication is from line 8 of RS 15.035, where the word-order in the phrase *w lbš tn* shows that there, at least, *tn* is probably the ordinal, for one would expect the cardinal number to have been placed before the noun it modifies (see Tropper and Vita, *UF* 30 [1998] 679-80).

<sup>778</sup>*Les textes rituels* (2000) 639.

369) that the seven cardinal numbers in lines 27-30 of the funerary ritual RS 34.126 (*KTU* 1.161) were “Kardinalia für Tageszählung” cannot be taken seriously. It is far more likely that the number nouns had an adverbial function in this text<sup>779</sup> and that they designated the repetition of some act associated with the offering expressed by the verb  $\text{T}^{\text{c}}\text{Y}$ , which is repeated seven times in this passage. In my latest commentary on this text, I have suggested that the act in question was the performance of a sevenfold descent (the point of reference would be the verb YRD in the preceding passage) of the deceased king into the bowels of the earth for the purpose of enabling him to join his ancestors.<sup>780</sup>

— p. 373 (§64.23), p. 420 (§69.54b). It is difficult to see how {[b .] šb<sup>c</sup>[t . w .] nšp . ksp} (RS 18.024:27 [*KTU* 4.337]) can be thought to qualify as a “n[eue] L[esung]” when Márquez Rowe proposed it in 1992 and T. himself did so in 1995.<sup>781</sup> The reading is only “new” with respect to *CAT*, the authors of which failed to pick up Márquez Rowe’s correct restoration of {šb<sup>c</sup>[t]} in place of {šb<sup>c</sup>[m]}, the restoration that was proposed in *KTU*. It is the correct reading of the total indicated in the last line of this text, also achieved by Márquez Rowe, that permitted the correct restoration of line 27.<sup>782</sup>

— p. 374 (§§64.42, 64.51). It would be better to avoid the word “Schekelmünzen” in referring to Late Bronze weights; the uninformed reader might take the word in the narrow meaning of ‘shekel coins’.

— p. 376 (§65.12), p. 383 (§67.11), p. 691 (§76.343b). T.’s confident presentation of RS 2.002:56-57 (*KTU* 1.23) *yṭbn yspr l ḥmš* as containing a /YQTLØ/-form (here with energetic ending!) followed by a /YQTLu/-form expressing a purpose clause (pp. 383, 691) is based at least in part on the reading (taken from *KTU/CAT*, against Herdner’s reading in *CTA*) of the first word as bearing a {-n} which, normally, would mean that it belongs to the imperfective category. The text is, however, broken immediately after {yṭb}, viz., there is no trace whatever of a {n} on the tablet, and it is equally possible to restore something along the lines of *yṭb [il] yspr l ḥmš*, ‘*Ilū* sits down, he counts ...’.<sup>783</sup>

— p. 376 (§65.12). T.’s translation of *yspr l ḥmš* in the text cited in the previous note as meaning “Er (der Priester) rezitiert (dies) noch fünfmal” could have been tempered by three considerations: (1) first and of most relevance in this section of the grammar, this would be the only iterative expressed by the preposition *l* in Ugaritic, (2) the reading of the text after these words is not certain and the precise function of *yspr l ḥmš* may judged uncertain for that reason; (3) the structure of the text as laid out on this tablet speaks against this passage

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<sup>779</sup> As Bordreuil and I have consistently translated in our various presentations of this text (see bibliography in Pardee, *ibid.*, p. 816; more recently, Pardee, *Ritual and Cult* [2002] 88; Bordreuil and Pardee, *Manuel* [2004] II 64-65).

<sup>780</sup> For the details of the proposal, see *Les textes rituels*, pp. 823-24; more briefly, *Ritual and Cult* (2002) 86-87 and note 128 (pp. 114-15).

<sup>781</sup> Márquez Rowe, *UF* 24, p. 260, n. 7; Tropper, *AuOr* 13, p. 138.

<sup>782</sup> For a discussion of these matters, see Pardee, *Syria* 77 (2000) 43, 55.

<sup>783</sup> Bordreuil and Pardee, *Manuel* (2004), text 5 in the *Choix de textes*.

consisting of a ritual command.<sup>784</sup> Therefore, the interpretation ‘He ( *ʾIlū* ?) sits, he counts to five (months)’ makes more sense in context.<sup>785</sup>

— p. 377 (§65.131b), p. 514 (§74.222.3), p. 650 (§75.527a). In all these sections, T. either translates or else explicitly parses *tštn* in RS 24.248:20 (*KTU* 1.104) as G-passive of ŠT, ‘to put’: “sie werden ... hingestellt/niedergelegt” (p. 377). This is another case where the analysis as a 2 m.s. imperfective form, here expressing an indirect volitive, is just as plausible (cf. above, remark to p. 211 [§41.12], etc.).<sup>786</sup>

— p. 377 (§§65.132-133). Among other arguments<sup>787</sup> against taking *mlū ksm tltm* in RS 1.003:18-19 (*KTU* 1.41)//RS 18.056:20-21 (*KTU* 1.87)<sup>788</sup> as meaning “die Becher(?)<sup>789</sup> [werden] dreimal(?) [gefüllt]” are the facts that (1) the ‘precativ perfect’ is unknown in the ritual texts as well as in Ugaritic prose in general<sup>790</sup> and (2) the corresponding number is *ḥmš* ‘šrh, ‘fifteen’, in the similar passage in RS 1.001:9-10 (*KTU* 1.39), where the form in question is *mlūn*. The number there is obviously half of ‘thirty’, an indication that *tltm* should be taken as a plural, not as singular + enclitic {-m}; interpreting RS 1.001 along the lines of T.’s view of the other two texts would require that the former prescribe five times as many repetitions as the latter (T. makes no attempt to compare RS 1.001 with the other two texts: below, p. 397 [§69.142.2b] and p. 399 [§69.143.32b], he translates “15 *mlun*”).

— pp. 377-78 (§§65.142-143). In his discussion of the etymology of the Ugaritic iterative morpheme *-id* that is attached to a number noun (see above, remark to p. 150 [§33.116.2]), T. reaches the conclusion based on comparison with South Arabian *s<sup>2</sup>ltt’id* that the iterative morpheme was historically a noun meaning “Zeitpunkt, Moment.” The comparison proposed with Hebrew *ʾāz*, Aramaic *ʿedayin*, and the Ugaritic adverb *id*, all adverbs meaning ‘then, at that time’, and with similar forms in other languages, allows one to surmise, however, that all these forms were historically expansions of the demonstrative element /-d-/ and hence that fundamentally they were particles, rather than substantives. Whatever the origin of the Ugaritic iterative morpheme may have been, its only function is as an enclitic particle, and

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<sup>784</sup>I have discussed these matters in *Context I* (1997) 282 n. 60; *BASOR* 320 (2000) 59.

<sup>785</sup>Below, p. 383 (§67.11), T. indicates this interpretation; neither section contains a cross-reference to the other.

<sup>786</sup>Pardee, *Les textes rituels* (2000) 567.

<sup>787</sup>Cf. Pardee, *ibid.*, pp. 174-75 (cf. pp. 61-63).

<sup>788</sup>T. refers explicitly to RS 1.003:19 and to RS 18.056 as reading *ksm tltm* [*mlū*], but in the latter passage the word *mlū* is extant at the head of the phrase whereas in the former that is the only plausible restoration (*ibid.*, pp. 146, 174-75).

<sup>789</sup>On p. 401 (§69.152.2) T. observes that *ksm* in this passage “dürfte kaum ‘30 Becher’ meinen” because the plural of *ks*, ‘cup’, is elsewhere attested as *kst*. The fact is equally valid as a counter-argument to the translation proposed on p. 377. Cf. p. 408 (§69.212), where it is simply asserted that *ksm* is the singular of *ksmm* in RS 1.003 as well as in RS 1.001.

<sup>790</sup>Below, p. 726 (§77.34), T. remarks that the “Suffixkonjugation ... mit volitivischer Nuance” is rare and attested only in poetry. No examples from prose are cited on the following page in the section devoted to “statische Variante” of this usage (§77.35).

references in the presentation of the morpho-syntax of the numbers to its supposed substantival origin are misplaced (see below, remark to p. 390 [§§69.123, 69.124.1]).

— p. 378 (§65.147b), p. 538 (§74.333), p. 747 (§81.25). T. has basically adopted the interpretation proposed by Dietrich and Loretz<sup>791</sup> of RIH 78/14:12' (*CAT* 1.163:5) [*hm*] *tl̄t id ynphy yr̄h b yr̄h āhr̄m*, viz., “wenn es dreimal hintereinander Monat für Monat erscheint” (p. 378, with variants in the other two references). Neither this division of the line nor the interpretation of both tokens of *yr̄h* as meaning ‘month’ may, however, be said to be plausible.<sup>792</sup>

— p. 379 (§65.147d), p. 470 (§73.353), p. 617 (§75.224). T.’s confident presentation of the last word preserved in RS 18.113A+B:12 as “*likt*” and his equally confident analysis of the resultant form as a 1 c. sing. perfective, ‘I have sent’,<sup>793</sup> do not take into account the damaged state of the tablet. Because only the upper left corner of the {t1} is extant,<sup>794</sup> that ‘reading’ may only be judged a plausible reconstruction; even if the reconstruction is correct, the analysis as 1 c. sing. is plausible but not certain.<sup>795</sup>

— p. 379 (§65.21). When T. asserts here that an iterative adverb *tnm* with the nuance ‘do for a second time’ is only attested in RS 3.322+ iv 61 (*KTU* 1.19), he forgets that two pages above (§65.132) he has proposed that meaning as an alternative to ‘twice’ in RS 24.248:20 (*KTU* 1.104).

— p. 379 (§65.151). The only thing “new” about the reading of *pāmt tn l šrm* in RS [Varia 20]:20-21 is that it does not reflect a typographical error in *CAT*, where the {t} of *pāmt* was omitted.<sup>796</sup> This word was correctly read in the *editio princeps*.<sup>797</sup>

— p. 381 (§66). T. gives two interpretations of the indication of the price in RS 15.062:5 (*KTU* 4.158) that reads *tl̄t l šrm ksphm*: the total would either be twenty-three shekels or forty-six, i.e., twenty-three for each of the two kinds of saplings named in the preceding lines. The structure of this text requires the former.<sup>798</sup>

— p. 384 (67.22). Information regarding the words *tl̄tm* and *hmštm*, which to date have appeared only in RS 94.2184+ and of which the meaning is unclear because the tablet is damaged, did not come “aus RSOu 14,” where these forms are not mentioned, but via e-mail from the author of these remarks.

— p. 389 (§69.114), p. 394 (§69.133.22b). In the first paragraph cited, {ātt ādrt} in RS 11.857:09 (*KTU* 4.102) is translated “eine alte Frau”; in the second, {tl̄t ātt ādrt} in line 16 of the same text becomes “drei vornehme Damen.” One would expect *ātt* in an administrative

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<sup>791</sup>References in Pardee, *Les textes rituels* (2000) 869, n. 75.

<sup>792</sup>The details of the argument may be found *ibid.*, pp. 869-70.

<sup>793</sup>Following Dietrich and Loretz, *UF* 5 (1973) 77, n. 32, and their subsequent presentation of the text in their collections of Ugaritic texts.

<sup>794</sup>Virolleaud’s hand-copy represented very well what remains on the tablet (*PRU* V [1965], p. 14).

<sup>795</sup>Pardee, *UF* 19 (1987) 205.

<sup>796</sup>See the corrigendum in Dietrich and Loretz, *Word-List* (1996) 225.

<sup>797</sup>Bordreuil and Pardee, *Semitica* 41-42 (1991-92) 42-53.

<sup>798</sup>Pardee, *Syria* 77 (2000) 27-32.

text such as this to have a technical meaning and T.'s inconsistent translations constitutes, therefore, another case of the indecision to which reference was made in the introduction.

— p. 390 (§§69.123, 69.124.1). RS 24.291:9 (*KTU* 1.132) does not contain an example of a singular noun used after the number noun 'two': {*tt*} there is the Hurrian dative plural morpheme attached to the preceding noun, not the Ugaritic number noun.<sup>799</sup> Nor are three examples of the phenomenon in question to be found in RS 16.396:26-28 (*KTU* 4.244): {*tn krm*} there is each time in construct with the following personal name.<sup>800</sup> Yet another example is to be discounted on T.'s own testimony: on p. 69 (§21.412g) he observes that in RS 2.[032]:1-2 (*KTU* 4.4) the signs {*pḡn.dr(2)m*} constitute a single word. If that is the case, the entry is *tn pḡndrm* and the final *-m* is the dual morpheme of a single word.<sup>801</sup> This leaves only *tt tint*, 'two (pieces of) scarlet (tissue)',<sup>802</sup> as a valid example of the phenomenon and hence perhaps a simple scribal error. As a last example in §69.123, T. invites the reader to compare *tnid*, which he here translates "zwei Male" instead of the usual "zweimal," the comparison is meaningless in terms of Ugaritic grammar: *id* is invariable when attached to a number noun and hence clearly functions as an uninflectible enclitic particle, whether or not its origin was nominal.<sup>803</sup> A fifth putative example is adduced in §69.124.1: in *tt kwt yn* (RS 20.010:6 [*KTU* 4.691]) *kwt* is said to be in the singular—but, if *kwt* exists as a singular as T. believes, then it must be parsed in RS 20.010 as a dual in the construct state (*\*kwtm* → *kwt* in construct).<sup>804</sup>

— p. 392 (§69.131). Since the top of RS 10.043 (*KTU* 4.47) has disappeared, there is no way of knowing whether the numbers in this text refer to members of the corporations named at the head of each line or to some other entity for which each corporation is somehow responsible (reception or contribution).

— p. 395 (§69.133.32a), p. 847 (§91.321b). T.'s ambivalence regarding the correct translation of RIH 78/02:3-4 (*KTU* 4.771) on p. 395 ("Fünf (Schekel [Silber]) von/für Färberröte") is unexplainable given his correct explanation and translation on p. 847 (the *d* in the second paragraph of this text reflects the genitival construction in the first: "und fünf (Schekel [Silber für]) Krappwurzel").

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<sup>799</sup>Pardee in *Le bilinguisme* (1996) 78; idem, *Les textes rituels* (2000) 738-740, esp. p. 739 n. 5.

<sup>800</sup>See recently J.-A. Zamora, *La vid y el vino en Ugarit* (Banco de Datos Filológicos Semíticos Noroccidentales, Monografías 6; Madrid; Consejo Superior de Investigaciones Científicas; 2000) 86.

<sup>801</sup>According to various authors, including T. himself, *pḡndr* would be a Hurrian word denoting some kind of garment: cf. Watson, *UF* 28 (1996) 703 + n. 12; Tropper, *UF* 29 (1997) 665.

<sup>802</sup>In explaining the meaning of *tnt* as "*tnt*-Gewänder" in §69.124.1, T. refers to Akkadian *šinatena* (Alalah) and *šinītu*. Since Ugaritic is a West-Semitic language, it might not have been out of place to refer to Hebrew *šānī*<sup>Y</sup> as well.

<sup>803</sup>Below, p. 392, the same comparison is made with the other iterative number adverbs (*tltid*, etc.), and it is there asserted that *-id* is in the singular (cf. p. 393 [§69.133.21a], where *-id* is said to be of masculine gender). That may or not be true historically, but is in any case irrelevant for Ugaritic (see above, remark to pp. 377-78 [§§65.142-143]).

<sup>804</sup>In dispute is the question of the relationship between this word and *kw* in RS 18.148:20 (*KTU* 2.47:17)—which appears originally to have been written {*k.w*} (see provisionally RSO XIV [2001] 382-83).

— p. 399 (§69.143.21b), p. 776 (§82.412). On p. 399, the thirteen ‘openings’ (*pth*) mentioned in RS 15.184:7 (*KTU* 4.195) are said to be doors; on p. 776, they are said to be windows.

— p. 399 (§69.143.21c). It cannot be considered likely that the use of {*tn* ‘*šr*’} (instead of the more common form with ‘*šrh*’) in RS 17.124:1 (*KTU* 4.274) indicates a measure other than *kd* for the wine in question because line 7 contains the entry *kdm*, ‘two *kd*- measures (of wine)’.

— p. 405 (§69.181.1b). T. here assumes the alternative readings {*w* [. *tš*]‘*m*} or {*w* [. *šb*]‘*m*}<sup>805</sup> for RS 16.395:8 (*KTU* 4.243) though he had also earlier claimed that the {*b*} of the latter reading was partially visible.<sup>806</sup> My collation of the tablet has confirmed that it is necessary to leave the options open: I found no trace of a sign to the left of {‘*m*}.

— p. 406 (§69.191.1). Two considerations render unlikely T.’s suggestion that *spr irgmn tlt* in RS 15.106:1-2 (*KTU* 4.181) would be the title of the text {“Tributliste (in Form) von Kupfer”}: (1) there was sufficient room to write {*tlt*} in line 1 as part of the title of the text; (2) every other entry in the text includes the word {*tlt*}, which would not be the case of the first entry in lines 2-3 if *tlt* is detached therefrom.

— p. 412 (§69.231). The broken passage RS 17.370[C]:03-04/05 (*KTU* 4.304) is included twice in this section.

— p. 416 (§69.312.23). It is difficult to believe that *šb‘ māt š‘rt [kbd] iqnū* (RS 15.115:19-20 [*KTU* 4.182]) tells us anything about the regular syntactic relationship between the number noun ‘*šrt*’ and the numbered noun for here *kbd* is situated between the two words, making a genitive relationship impossible.

— p. 418 (§69.321). Since T. himself refers the reader back to §62.121 (pp. 345-46), where one reads that the number noun ‘two’ does not show mimation in the absolute case, one wonders how he could allow himself to say that *tn* and *tt* followed by a numbered noun “weisen somit die Form des St. cs. auf.” Since neither the absolute nor the construct shows mimation, the absence of mimation says nothing about the state of the noun.

— p. 419 (§69.43), pp. 841-42 (§91.24). In the first section cited, T. admits only two cases of ordinal numbers preceding the noun they modify, the first in the common poetic motif of ‘a day, a second; a third, a fourth day’ (*tlt rb‘ ym*), the second in a related phrase that is attested only once, ‘*d šb‘t šnt*’ (RS 3.322<sup>+</sup> iv 14-15 [*KTU* 1.19]). In the second, he places these examples from the number system in a broader context by treating them alongside common adjectives. Above, I have explained the number formulae and several similar ones as consisting of a substantivized adjective in construct with a plural noun (p. 261 [§51.43d], etc., in particular note 584).<sup>807</sup> Such an explanation will not account for the examples adduced in these two sections, however, for the modified noun is in the singular. Instead of simply saying that the ordinal precedes the noun however and functions as an attributive

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<sup>805</sup> As previously proposed in *AuOr* 13 (1995) 237.

<sup>806</sup> *AuOr* 16 (1998) 293.

<sup>807</sup> This explanation was adopted in *Les textes para-mythologiques* (1988) 116-17 to explain the phrase *w n‘mt šnt il*, ‘(for) the goodly years of El’ (lit. ‘the good ones of the years of El’), which functions as a superlative.

adjective, a feature of adjectival syntax that is extremely rare in the old West-Semitic languages (to my knowledge, it first begins appearing with anything approaching regularity in fifth-century Aramaic, where it is limited to a few expressions), I wonder if the syntax might be that of the adverbial accusative. The adjectives would here also be substantivized, with the noun standing as an adverbial accusative: ‘for a third, for a fourth, with respect to (the category) day’. The explanation would, however, probably remain largely theoretical even if we had access to the ancient vocalization, for all three words would have been in the adverbial accusative (/ʔalīʔa rabī‘a yōma) and there would be nothing but the word-order to tell the hearer that the adjectives were not functioning attributively.

— p. 425 (§73.121.1a), p. 720 (§77.312b). Only on p. 720 does T. translate RS 18.075:16'-17' (*KTU* 2.41:15-16) where he indicates the reading “iʔrš ‘my\mnm irštk,” which is rendered “Wünsche von mir, was immer du willst!” (on p. 425 *irš* is parsed as a G-stem imperative but the context is not translated). On p. 720 he also identifies {irš} as a ‘new reading’. This treatment requires several comments: (1) it is debatable whether {irš} is properly termed a new reading, since Gordon presented the reading as a restoration in 1965<sup>808</sup> and several scholars have accepted the restoration in their interpretation of the text — it is, therefore, only new as a ‘reading’ in the sense that the trace taken by the authors of *KTU/CAT* as belonging to {y} is taken by T. as belonging to {i}; (2) the ‘reading’ {i}, I have concluded after collating the text,<sup>809</sup> is difficult to accept, for the {i} would have to have been significantly less wide than the {h} just above it in line 14’;<sup>810</sup> (3) virtually no one, including T., has attempted to interpret lines 16'-17' as sequential to line 15' with the reading {irš} in line 16'—with good reason, for line 15', which begins with the deictic particle *hnm*<sup>811</sup> and cannot, therefore, be the end of a more complete sentence, makes no sense on its own;<sup>812</sup> (4) none of the Akkadian parallels cited for lines 17'-19' includes a formula similar to *irš* ‘mybefore the indefinite pronoun.’<sup>813</sup> The only alternative that I have

<sup>808</sup> *UT*, p. 19\*, 367 (§19.379)

<sup>809</sup> In my dissertation, I accepted Gordon’s reconstruction: *UF* 7 (1975) 341; *UF* 8 (1976) 215.

<sup>810</sup> See photo and copy in my *Les documents épistolaires* (in preparation).

<sup>811</sup> The word may be read as {hʔnm} or as {iʔnm} (the crucial lower left corner of the sign has entirely disappeared) and the ‘reading’ *inm* preferred by some (including *KTU* but not *CAT*) is as much a reconstruction as *hnm*. I prefer the latter because, though otherwise unattested, it has a good chance of being a Ugaritic word (the deictic particle *hn* + enclitic *-m*). Some of those who accept the reconstruction of *inm* see in it the Akkadian word *enūma* (Astour, *AJA* 69 [1965] 256; Márquez-Rowe, *AuOr* 10 [1992] 153; Watson, *AuOr* 12 [1994] 98; del Olmo Lete et Sanmartín, *Diccionario* I [1996] 39; idem, *Diccionario* II [2000] 395), unlikely in my opinion given the general rarity of Akkadian loan-words in the Ugaritic letters.

<sup>812</sup> The only attempt of which I am aware to interpret lines 15'-17' so is that of S. Ahl (*Epistolary Texts from Ugarit: Structural and Lexical Correspondences in Epistles in Akkadian and Ugaritic* [thesis, Brandeis; Ann Arbor: University Microfilms, 1973] 445), who reads *inm* ‘bdk hwt and translates “Is it not (a fact) that I am your servant?” The interpretation is not acceptable because the negative particle *in* would appear in a single sentence with a finite verb and because the verb ‘to be’ is not HW(Y/W) in Ugaritic prose but K(W)N. For the latter reason, reading the first word as *hnm* provides no solution.

<sup>813</sup> Parker, *Studies in the Grammar of Ugaritic Prose Texts* (1967) 59, 67-68 (n. 32), 70; Rainey, *UF* 3 (1971) 160.

discovered is to read {ʿylrš} and to take lines 15'-16' as a syntactic unit, followed by lines 17'-19', another related unit. The entire passage may be read and translated {ʿhlnm . ʿbdk . hwt (16') ʿylrš . ʿmy (17') mnm . ʾrštk (18') d [.] ḥsrt . w . ʾnk (19') ʾštn .. l . ʾhy} 'So now your servant shall be empowered to speak (lit. shall possess word) to me (viz., on your behalf): Whatever your request (may be concerning anything) that you lack, I will send it to my brother'.<sup>814</sup>

— p. 425 (§73.121.1a), p. 426 (§73.121.1c), p. 547 (§74.412.21), pp. 557-58 (§74.413.2), p. 614 (§75.212.2). T. comes to RS 17.117:12 (*KTU* 5.11) with the idea that {ʾrš} must be an imperative and, because the imperative of ʾRŠ is several times written {ʾrš}, the result is some mental gymnastics to account for the {ʾ}: the preferred interpretation (p. 426) is that the orthography shows anaptyxis (/waʾrVš/ → /waʾarVš/ — on the reconstruction of proto-Ugaritic as having zero vowel in the first syllable, see remark below to p. 426 [§73.121.2]), a phenomenon of which this would be the only example. T. points out, apparently as a basis for the irregularity in his preferred analysis, that this text shows other irregular uses of the {ʾ}-signs (these are limited, however, to two cases of {ʾ}) for expected {ʾ}) and adds in parentheses “Schultext”—on this classification of the text and its value as an explanatory device, see above, remark to p. 179 (§33.231.22), etc. Morphologically speaking, the analysis as a D-stem imperative is far the more plausible (pp. 426, 557, 614). Contextually speaking, however, the only analysis that makes sense to me in RS 17.117 is as a /QTLa/ form.<sup>815</sup>

— p. 425 (§73.121.1a). T. interprets {isp} in the second of the serpent-banishment texts (RS 24.251 [*KTU* 1.107])<sup>816</sup> as showing “Anaptyxe /i/ (bzw. Murmelvokal /ə/),” which he represents as “/ʾispī/ (Vokalsynkope) ← \*ʾ(V)supī.” He bases this complicated development, for which there is no clear parallel, on the fact that the verb ʾSP in Hebrew has a /yaqtul/ imperfect, but without considering the possibility that the Ugaritic form may have been /yaqtil/ as in Akkadian. (It is dangerous to extrapolate too much from Hebrew forms that do not show a /yaqtil/ base, for that base practically disappeared already in proto-Hebrew except in I-y roots.)

<sup>814</sup>Márquez Rowe proposed to see in *hwt yrš* the literal translation of an Akkadian idiom, *awata(m) rašû*, “to have cause to complain” (*CAD R*, p. 422), which he translated in context “... your servant has got the (following) cause for complaint: (We agreed that whenever you wrote) to me any wish of yours ...” (*AuOr* 10 [1992] 153). The absence in the Ugaritic, however, of the crucial words placed in parentheses, which are intended to show that the speech refers to previous correspondence between the two principal parties, a fact that is usually stated explicitly in Ugaritic letters, renders the interpretation dubious, as do the general absence of Akkadisms in the Ugaritic letters and the fact that the Ugaritic verbal form is an imperfective (the usual practice is to use the ‘epistolary perfect’ when referring to acts from the writer’s perspective).

<sup>815</sup>For this interpretation, see Pardee, *AfO Beiheft* 19 (1982) 45; idem, *Context* III (2002) 109; idem, *Les textes épistolaires* (in preparation).

<sup>816</sup>For a new set of reconstructions of the passage of this text where the forms of the verb ʾSP appear, see Pardee, *Ritual and Cult* (2002) 181-84, 190-91 nn. 54-57—the new reconstructions have been judged necessary because in *Les textes para-mythologiques* (1988), ch. 8, I did not adequately take into account the size of the lacunae.

— p. 426 (§73.121.2). T. reconstructs the proto-Ugaritic G-stem imperative as having zero vowel in the first syllable (/qtul/, etc.) and the Ugaritic form as having a murmured vowel (he represents the forms as /r<sup>u</sup>gum/, etc.). I see no reason to deny full status to this vowel in Ugaritic (T. mentions both Akkadian /purus/ and Canaanite /qutul/ but for some reason considers the parallels inapplicable to Ugaritic); moreover, it is debatable at what stage, if ever, Semitic had a true /qtul/ base, i.e., one with a consonantal cluster at the beginning of the word.<sup>817</sup> (Does Arabic /ʔuqtul/ represent one way of resolving a proto-West-Semitic form or is it secondary?) In any case, to the extent that the theories of proto-zero vowel and murmured vowel in Ugaritic have influenced T.'s thinking on matters of anaptyxis and preformative /ʔ/, they would better have been left aside (see two preceding remarks and remark to p. 170 [§33.211.2], etc.). There is at least one /qatal/-imperative in Ugaritic of which the first syllable was represented by {ā}. I refer to *ār*k in RS 2.002:34 (*KTU* 1.23), where the combination of form and context indicate the presence of a volitive form.<sup>818</sup> Parsing that form as an infinitive (p. 484 [§73.513.6], p. 711 [§76.524.44]) or, a truly desperate solution since the subject is feminine, as a perfective (p. 614 [§75.212.3]) may thus be rejected and the writing may be taken as indicative of the quality of the first vowel in a /qatal/-imperative.

— p. 426 (§73.122). On the implausibility of T.'s proposed examples of G-stem imperatives with prosthetic ʔ*alif*, see remarks above to p. 170 (§33.211.2), etc., p. 195 (§33.322.2c), etc., p. 202 (§33.432a), etc., and below to p. 540 (§74.342). In addition to these examples, T. suggests below, p. 449 (§73.243.21), that {iḥdl} in RS 5.195:4 (*KTU* 7.51) might be another. There the context is entirely broken, and, since the grammatical category is questionable at best and most likely non-existent in Ugaritic, there is no reason to consider this example even as a possibility.

— p. 427 (§73.131), p. 443 (§73.233.1), p. 451 (§73.243.22b), p. 654 (§75.531b), p. 659 (§75.532). In the first section cited here, in the presentation of the G-imperative, one encounters the first of the verbal forms of the root BNY/W, 'to build', for which a base form with III-w is reconstructed ("b<sup>u</sup>nû/ < \*bnuw"). Three general remarks are in order: (1) Because, with an extremely small number of exceptions, only nominal forms with III-w are attested in Ugaritic while virtually all attested verbal forms with an extant third consonant show {y}—in keeping with the general Ugaritic pattern—, one may posit with equal plausibility that proto-Ugaritic III-w roots had, as in the other Northwest-Semitic languages, generally become III-y (T.'s basis for not adopting this hypothesis is that, by his rules for monophthongization of triphthongs, several forms should show consonantal {y} but do not [see remark below to pp. 653-54, §75.531b]). T.'s reconstructions thus are based on a root III-w when the proto-form would have included the triphthong /uwū/ or the diphthong /uw/ (though T. cannot make up his mind on such forms—see below my point no. 2), everywhere else on a root III-y. The reconstruction of proto-Ugaritic BNY/W as having a /yaqtul/ imperfect is, however, belied by the fact that Akkadian already shows the preterit *ibni* ←

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<sup>817</sup>For the few possible cases of isolated nouns, see Testen, *JNES* 44 (1985) 143-46.

<sup>818</sup>Pardee, *Context I* (1997) 280; Bordreuil and Pardee, *Manuel* (2004), text 5 in the *Choix de textes*.

/ʔabniy/ and the imperative *bini*.<sup>819</sup> (2) T. has trouble keeping his reconstructions straight. {ʔbn}, ‘I build’, is reconstructed as both /ʔabnî/ ← BNY (p. 451 [§74.243.21b]) and /ʔabnû ← BNW, i.e., /ʔabnuw/ (p. 659 [§75.532])—the same text is cited as source for the form in both paragraphs. On p. 427 (§73.132), adjacent reconstructions of proto-forms of 2 f.s. imperatives from putative III-w roots are different (DʔW and LʔW are shown as having identical final forms and identical proto-forms, but the stem vowel in an intermediate state is shown as known in one case, unknown in the other: “dʔVyī” vs. “lʔuyī”). Here and on p. 663 (§75.533), the latter form is vocalized /liʔī/ whereas on p. 617 the vocalization is /luʔī/. On p. 443 (§73.233.1), T. explicitly indicates “/tabnû/” as deriving from /tabnuw/—whatever the merits of the reconstruction /tabnuw/ may be, /tabnî/ cannot derive therefrom and would have to be a secondary form by paradigm pressure. Perhaps for that reason, T. indicates only /tabnû/ for the same form in the same text on p. 659 (§75.532). On the other hand, just below “/tabnû/” on p. 443 is indicated “/ʔahđî/” but here as derived from either /ʔahđiy/ or /ʔahđuw/ (ditto p. 451 [§73.243.22c]); on p. 660 only /ʔahđî/ ← /ʔahđiy/ is indicated—again all three times for the same form in the same text. (3) T.’s triphthong theory requires him to reconstruct other verbs as having /YQTL/ with stem-vowel /u/ (/yaqtul/) from a III-w root when the internal and comparative evidence for both aspects of the hypothesis are weak. For example: on pp. 654 (§75.531b) and 661 (§75.532), the /YQTL/ forms of the intransitive verb ‘LY, ‘to ascend’, are reconstructed on the base /yaʔluw/, though the verb shows consonantal {y} in the /QTLa/, the participle, and the infinitive. The basis for the choice of /yaʔluw/ as the Ugaritic base form is triphthong behavior; the justifications are that Arabic shows the root form ‘LW (note that this root in Arabic shows two forms: /qatala/yaqtul-/ and /qatila/yiqtal-/) and that intransitive but fientive verbs may be /yaqtul/ in Semitic (p. 453 [§73.245.1]). In the case of ‘LY, as we have already seen to be true of BNW/Y, Akkadian shows the /yaqtil-/ pattern. The basic problem, as stated above in the seventh general introductory remark, is that a grammar of Ugaritic, a language for which the data are few and not unequivocal, is not the place to attempt to solve such complicated problems of comparative Semitics. What T. is essentially proposing is that Ugaritic, unlike Hebrew and Aramaic where virtually all III-weak forms have in the G-stem collapsed into a single paradigm, was at a transitional stage, with some forms retaining their older III-w pattern, while others have assimilated to a younger III-y vocalism, while still other forms have already developed a tertiary form based on the III-y secondary form (e.g., p. 427 [§73.132]: “/liʔî/ < \*lʔuyī < \*lʔuwī”<sup>820</sup>). Are the data available from the Ugaritic texts sufficient and sufficiently unequivocal to make such a reconstruction of the system plausible? I have my doubts, but at the very least the reconstructions should be presented in a consistent manner, both internally to the grammar itself and as reflections of the Ugaritic data.

— p. 427 (§73.132), p. 448 (§73.243.1), p. 617 (§75.223), p. 660 (§75.532). The emendation of {tlûnn} in RS 02.[003]<sup>+</sup> i 33 (KTU 1.14) to {tlûnn} was proposed above, p. 57 (§21.352.1), but the form is for the first time parsed as G-stem /yaqtulu/ in the first section

<sup>819</sup>See W. G. Lambert, *NABU* 2001, p. 39, on OB *ibni*.

<sup>820</sup>I confess that I do not see quite why the intermediate form is necessary (on p. 200 [§33.323.6], in the relevant section on phonology, that step was put in parentheses).

cited here and that parsing is reflected in the other sections cited; he derives it from his verb L<sup>1</sup>W<sub>1</sub> “siegen” (p. 617). He analyzes *tlû* in RS 24.244:68 also as /yaqtulu/ but from the root L<sup>1</sup>W<sub>2</sub> “schwach sein” (p. 617). Whatever the ultimate solution may be to the appearance in Ugaritic of (a) root(s) containing the elements L and <sup>1</sup> from which nominal and verbal forms are derived with the opposite meanings of WEAK and STRONG (see further below, remark to p. 468 §73.333.1], etc.), a case can be made for the identification of *tlûnn*, if the emendation from {tlûnn} be accepted, as the D-stem of the verb attested in RS 24.244 which in this derived stem would have the meaning ‘make weak’<sup>821</sup>—the line is a thin one between ‘overpowering’ and ‘rendering weak’.

— p. 428 (§73.132). Assuming T.’s view of the nature of the vowel in the first syllable of the G-stem imperative as /Ø/ (see above, remark to p. 426 [§73.121.2]), the notation “/kir(i)yī/ < \*k<sup>i</sup>riyī” hardly makes sense. It can only be understood as some kind of shorthand for /kiryī//kiryī/ ← /k<sup>i</sup>riyī/ ← /kriyī/, where the possibility of the secondary murmured vowel in the first syllable having taken on full syllabic value when syncope of the vowel in the second syllable is registered. Apparently, hesitation on the occurrence of syncope has lead T. to confuse the two issues of syncope and the presence of a vowel in the first syllable of the imperative.<sup>822</sup> T.’s treatment of this particular form brings to mind that he does not bring up anywhere the question of the accent on the 2 f.s. imperative: final, as in Hebrew, or penultimate, as in Aramaic? He appears to assume that the middle syllable in this form is not accented, otherwise syncope would be impossible.

— p. 428 (§73.134), p. 492 (§73.532). T.’s claim that “Imp.-Formen f.pl. sind bislang nicht belegt” is technically incorrect, since one such form is attested, {šm<sup>c</sup>} in RS 5.194:11 (*KTU* 1.24). T. finds this form troublesome because the corresponding form in both Hebrew and Arabic shows an ending with consonantal {n}, though he cites Akkadian, Ethiopic, and some Aramaic dialects as having a form without the {-n}. He comes up with three other possible explanations: (1) a masculine imperative form in place of the feminine (as sometimes occurs in Biblical Hebrew); (2) an infinitive functioning as an imperative; (3) re-interpret the passage as containing a /QTLa/ form. While keeping in mind that one swallow does not a summer make, the comparative evidence appears sufficient to make the existence of a form without {-n} in Ugaritic at least plausible.

— p. 428 (§73.134). Though the comparative evidence supports T.’s confident statement that the final vowel of the dual was identical in the two grammatical genders, only the masculine form is proven to have /-ā/ by inner-Ugaritic evidence.

— pp. 428-29 (§73.14; §73.15), pp. 455-56 (§73.26). In his discussion of the /YQTLa/ form (pp. 455-56), T. opines that the final vowel may be short or long, but after this observation he vocalizes consistently in this section as /a/ (elsewhere in the grammar, he usually indicates /ā/). In his treatment of the extended form of the imperative (i.e., the imperative stem + /a/), he correctly identifies the /-a/ as identical to that of the /YQTLa/ form, but here

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<sup>821</sup> Bordreuil and Pardee, *Manuel* (2004) II 20-22 (the form is mis-identified in the glossary, p. 177, as from the root denoting strength).

<sup>822</sup> The hesitation continues below: on p. 429 (§73.142), the expanded m.s. imperative {mḥy} is vocalized without the option of syncope (“/m<sup>i</sup>ḥiyā”).

in his vocalization of all forms he indicates /ǎ/. I pointed out above that in T.'s discussion of {šǎ} as perhaps showing quiesced /ʾ/, he vocalizes the lengthened imperative option as /šaʾǎ/ (remark to pp. 37-38 [§21.322.5], etc.). Two points must be made here: (1) consistency of notation should have been observed; (2) the reasons for his uncertainty as to the vowel length should have been indicated. (I hold that since the corresponding morpheme in proto-Hebrew was short, i.e., it is represented by *qameṣ* in the Massoretic vocalization of Biblical Hebrew, it was probably short in Ugaritic as well.)

— p. 429 (§73.142), p. 621 (§75.233). Though the beginning of col. I of RS 3.322+ (*KTU* 1.19) is badly damaged, it is difficult to admit, judging from the preserved signs, that {...}šǎ in line 6 represents the m.s. impv. of NŠʾ, 'to lift', for the surrounding text appears to be couched in the 3rd person.

— p. 429 (§73.142), p. 475 (§73.426), p. 517 (§74.223.2). In the first reference cited, {mḥy} in RS 24.272:14 (*KTU* 1.124) is parsed as an emphatic imperative (to use the traditional term, i.e., the G-imperative followed by the vowel /-a/); here the optional interpretation of the {-y} as the enclitic particle is mentioned, with a reference to §89.3, though this text is not cited at the paragraph in question. In the second reference cited, the same form is cited as an example of the G-passive participle, though the interpretation as a G-passive /QTLa/ is classed as "Wahrscheinlicher." In the third reference cited, the form is cited among the "Weniger gesicherte Belege der Gp-SK." This form provides yet another example of the multiple interpretations encountered in this grammar of a given word in a given passage, here, as often, without adequate cross-referencing to other interpretations. (On the preferability of choosing one interpretation and stating the reasons for that choice in a given place, see above, third general remark.)

— p. 429 (§73.143), pp. 455-56 (§73.26). T. considers the /YQTLa/ form to be an emphatic form of the jussive, i.e., of /YQTLØ/, as the /QTLa/ volitive form is an expanded form of the imperative, i.e., of /QTLØ/. Does that mean that /YQTLu/ is an indicative form of the jussive? It appears more productive to consider these three forms as independent forms in Ugaritic, whatever their origins may have been. T. argues that this is not the case by attempting to limit the use of the /YQTLa/ form to the first person, as in Biblical Hebrew (where it is known as the "cohortative"), but in order to do so he has to explain away some forms that have {ǎ} as the final sign<sup>823</sup> and III-y forms that may be /YQTLa/. He offers as another proof the existence of the 1 c.s. jussive, claiming that this shows that the /YQTLa/ form "steht auch funktional nicht in Opposition zum unerweiterten Jussiv" (p. 456), an argument that makes no sense to me, since the two forms are morphologically distinct. The difficulty that we may have in translating the two forms as semantically/functionally distinct

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<sup>823</sup> Claiming that 3rd and 2nd person forms with final {ǎ} may be *plene* writing for quiesced /ʾ/ (p. 456) appears to me to be a rather obvious case of special pleading. For the case of *yqrǎ* in RS 2.[008]<sup>+</sup> vii 47 (*KTU* 1.4), see remark below to p. 456 (§73.263); for that of *ṯḥtǎ* in RIH 78/20:5 (*CAT* 1.169), see remark to p. 456 (§73.264). Nowhere in this grammar does T. treat {[t]mlǎh}, 'she FILL it' (RS 3.322<sup>+</sup> iv 61 [*KTU* 1.19]), a rather surprising omission in an otherwise so comprehensive a grammar—if he considers the reading/restoration to be incorrect, he should have stated this; if not, he should have incorporated the form into his system.

is not a basis for denying the distinction.<sup>824</sup> The best proof of their independence is that the volitive negative particle *ʾal* is never, to my knowledge, used with the /YQTLa/ form (below, p. 816 [§87.22], T. claims, in keeping with his theory, that the opposite may be the case, but he cites no orthographically distinct /YQTLa/ form in support of the assertion).<sup>825</sup> Various uses of /YQTLa/ forms other than as simple volitives are attested in Hebrew and in Arabic, and the various Ugaritic forms that T. has difficulty explaining as volitives make it more plausible to allow for a broader range of meaning there also than is allowed by the simple equation with the jussive.<sup>826</sup> For these reasons, and to avoid confusion with the Hebrew “cohortative,” which is limited to the first person, singular and plural, I propose that it would be preferable in describing Ugaritic grammar to use another term that expresses a form of volitivity, for example ‘optative’, or /YQTLa/-volitive, or simply /YQTLa/. Finally, whatever the status and function of the /YQTLa/ form may have been in Ugaritic, the data do not support the hypothesis that its use was limited to subordinate clauses.<sup>827</sup>

— p. 430 (§73.162). Is it really necessary to prove to anyone likely to use this grammar that the imperative can take an accusative complement?

— p. 430 (§73.162a). Two examples are cited, one after the other, of asyndetic imperatives; in both cases the second is *kbd*, ‘honor (someone)’. In the first, T. accepts the asyndesis, in the second he proposes that the text be emended by adding the conjunction *w* between the two verbs. Why?

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<sup>824</sup>T. claims also (p. 429) that the use of the so-called emphatic imperative in Hebrew (i.e., /kotbā/ as opposed to the simple imperative /k’tōb/) is “rein euphonisch” (p. 429). I realize that “emphatic” is not a popular term nowadays, but “purely euphonic” does not appear to solve the problem any more convincingly. What would T. need to establish that the two forms are “functionally” distinct? Is “emphasis,” or “euphony,” not a “function”? In English, grades of imperativity are expressed by all kinds of markers, from stress to contraction to added lexical items (e.g., ‘gét out’, ‘get out’, ‘get out of here’, ‘get outta here’, ‘get the hell outta here’, etc.) and any native speaker recognizes the function of each distinct form. What are the criteria for denying similar functionality to a Hebrew or Ugaritic morpheme?

<sup>825</sup>Perhaps the best possibility is in RS 2.[022]<sup>+</sup> iii 11 (*KTU* 1.5), where one finds *āl āst*, ‘I surely will not place,’ followed in the next line, after a break, by *āhpkk*, ‘I will overturn you’. The latter is probably a /YQTLa/ form because the {k} is written twice, and the former could be as well. It is well known, however, that the West-Semitic languages permit different volitive forms to appear in sequential utterances and parsing *āšt* as jussive, rather than as /YQTLa/, is, therefore, perfectly plausible.

<sup>826</sup>Though he does not do so here in detail (cf. p. 457), T. has in the past explicitly rejected the hypothesis according to which the Ugaritic /YQTLa/ form functioned regularly, as in Arabic, as a subjunctive in subordinate clauses (*UF* 23 [1991] 341-52; “Auf dem Weg zu einer ugaritischen Grammatik,” *Mesopotamica—Ugaritica—Biblica. Festschrift für Kurt Bergerhof zur Vollendung seines 70. Lebensjahres am 7. Mai 1992* [Alter Orient und Altes Testament 232; eds. M. Dietrich and O. Loretz; Kevelaer: Neukirchen-Vluyn; Butzon & Bercker: Neukirchener Verlag] 471-80, esp. p. 473-74) and I have done the same (*JNES* 52 [1993] 314-17; “Ugaritic,” *The Cambridge Encyclopedia of the World’s Ancient Languages* [ed. R. Woodard; Cambridge: University Press, 2004] 288-318, esp. 305; Bordreuil and Pardee, *Manuel* [2004] I 64). But the absence of an identical function in Ugaritic does not mean that we cannot take the particular development in Arabic as a sign of a difference in function between the /YQTLØ/ and the /YQTLa/ forms in the earlier languages.

<sup>827</sup>Pardee, *JNES* 52 (1993) 314-17.



— p. 438 (§73.223.41.2), p. 460 (§73.273.7), p. 661 (§75.532), p. 691 (§76.343b), p. 699 (§76.423), p. 887 (§95.231), p. 893 (§96.241b). T. accepts without question the reading {y‘n} that is proposed in *KTU/CAT* as the first word in RS 2.[014]<sup>+</sup> iv 5 (*KTU* 1.3) in spite of Herdner’s refusal to propose a reading for the badly damaged first sign<sup>832</sup> and in spite of the marking of the {y} as uncertain in both *KTU* and *CAT*. As Smith reported in 1997,<sup>833</sup> my collation of the tablet has shown only part of the lower section of a vertical wedge. This wedge was placed a bit to the right of the left margin and I conclude that the best reading here is {lml‘n} and that the poetic line contains a *figura etymologica*: {lml‘n . ḡlmm . y‘nyn}, ‘With a response did the two lads respond ...’.<sup>834</sup> The parallel with RS 2.[022]<sup>+</sup> i 11-12 (*KTU* 1.5) that T. sees here is incomplete, for in the latter text one finds only {y‘n}, not {y‘n ... y‘nyn}, viz., the text reads {w y‘n l gpn . w ūgr}, not {y‘n . ḡlmm . y‘nyn}, the reading of RS 2.[014]<sup>+</sup> iv 5 indicated in *KTU/CAT*. In RS 2.[022]<sup>+</sup> there is certainly, therefore, a /YQTLØ/ form and it may be considered an open question whether it is singular (i.e., *gpn-w-ūgr* are expressed as a single entity, vocalization /ya‘ni/) or dual (i.e., *gpn-w-ūgr* are expressed as a duality, vocalization /ya‘nâ/ ← /ya‘niyā/); the latter analysis is, however, to be preferred on the basis of the preceding phrase *tb‘ w l ytb ilm* (the analysis of {ilm} as a dual is to be preferred, viz., ‘the two gods depart they do not remain’, though it could be singular + enclitic-{m}). In the former text, on the other hand, the third root consonant is retained, and the analysis as a dual /YQTLu/ form (vocalization /ya‘niyāna/) appears to be required by the dual form of the common noun *ḡlmm* which is the subject of the verb.

— p. 438 (§73.223.41.2), p. 661 (§75.532), p. 887 (§95.231). T.’s own ambivalence regarding his own rules for monophthongization is evident in these three sections: in all three, y‘n in the phrase y‘n *gpn w ūgr* (RS 2.[022]<sup>+</sup> i 11 [*KTU* 1.5]) is analyzed as a singular verb followed by a dual subject; in the third, this passage is presented as a certain case of number discord between verb and subject (on this topic, see remark below to this page); whereas, in the second, it is admitted that {y‘n} may represent monophthongization of the triphthong (“ya‘niyā” → “ya‘nā”).

— p. 438 (§73.223.41.4), p. 442 (§73.223.5), p. 446 (§73.233.8), p. 460 (§73.273.8), p. 534 (§74.32), p. 594 (§74.622.3), p. 621 (§75.232), p. 645 (§75.522), p. 651 (§75.527g), p. 690 (§76.342), p. 749 (§81.4a), p. 801 (§83.24a), p. 872 (§93.422.1). Though *tšdn* in RS 24.258:23 (*KTU* 1.114) is certainly 3rd person fem. dual,<sup>835</sup> it is possible that *tttb* in line 27’

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w dg b ym .                      /wa dagu bi yammi/                      and fish in the sea.

<sup>832</sup> CTA (1963) 17 with n. 6.

<sup>833</sup> *Apud* Parker, ed., *Ugaritic Narrative Poetry*, p. 168 n. 74.

<sup>834</sup> Cf. my translation in *Context I* (1997) 252: “The lads answer up: ...”; Bordreuil and Pardee, *Manuel* (2004) II 16: “Les messagers se mettent à donner leur réponse: ...”.

<sup>835</sup> As is *t‘dbn*, repeated in lines 12 and 13 of this text where it has as subject the same goddesses as in line 23’ (Pardee, *Les textes para-mythologiques* [1988] 21, 22, 51). On T.’s treatment of these lines, see remark below to p. 663 (§75.533), etc., on *pn* in line 12.

and *trpā* in line 28' are not (‘*Anatu* and ‘*Attartu* may be presented as working separately).<sup>836</sup>

— p. 438 (§73.223.41.5), p. 441 (§73.223.42.12), p. 460 (§73.273.7), p. 646 (§75.522). One of the more striking usages of “n[eue] L[esung]” (see above, ninth preliminary remark) is encountered here: the reading “**y[ql]\*n** *alpm*,” ‘two bovids shall fall’, i.e., without the word *tn*, ‘two’, inserted between the verb and the noun (RS 1.009:11 [*KTU* 1.46]), is so described in spite of the fact that in 1996<sup>837</sup> T. declared himself in agreement with my reading of the verb as {y<sup>l</sup>ql<sup>l</sup>n} and without *tn*.<sup>838</sup> As I point out in my more detailed bibliographical treatment in *Les textes rituels*,<sup>839</sup> the proposal that the word *tn*, ‘two’, was not on the tablet goes back at least to 1970,<sup>840</sup> but is still present in the text of *CAT*. As regards the reading of the verb, T. again indicates {y[ql]\*n} on p. 441, but {y[ql]n} on p. 460, and {yq[ln]}, the reading in *CAT*, on p. 646. Finally, T.’s alternative parsing of {y<sup>l</sup>ql<sup>l</sup>n} as a G-passive, indicated only on p. 460, instead of the analysis as active, can hardly be given serious consideration, for QL is an intransitive verb expressing a form of motion.

— p. 439 (§73.223.41.6), p. 441 (§73.223.43), p. 442 (§73.223.5). Though I have in *Ritual and Cult* modified several details of the restorations of RS 24.251:32'-37' (*KTU* 1.107) that were proposed in *Les textes para-mythologiques*,<sup>841</sup> such is not the case of the restoration {[t<sup>i</sup>]sp} in line 40', a 3 f.du. form that is reconstructed as such to stand in contrast to the masculine form {y<sup>i</sup>sp} that occurs elsewhere in this passage (the subject of the verb is ‘*nt w*’ ‘*ttrt*, the goddesses ‘*Anatu* and ‘*Attartu*). On p. 439, T. argues forcefully for the restoration {[t<sup>i</sup>]sp} but with no reference to previous work on the passage.

— p. 439 (§73.223.41.8), p. 495 (§73.534b), p. 565 (§74.416.3), p. 589 (§74.622.3), p. 790 (§83.122g). T.’s attempt to interpret *ymgy* in RS 29.093:25 (*KTU* 2.70) and *yšb* ‘I in line 27 of the same text as 3 m. dual forms<sup>842</sup> founders on the form *b* ‘ly in line 28, which clearly bears the 1 c.s. suffix, not the 1 c.du. suffix, which would be {-ny}. T. mentions the possibility of emending {b ‘ly} to {b ‘l<sup>ny</sup>} (pp. 439, 589, 790), as has been proposed in *CAT*, but offers as his first translation “Als(?) deine beiden Diener zu dir gekommen sind wegen der Zahlung(?), liessen sie da nicht(?) einen Mantel für meinen ... Herrn anfertigen” (the

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<sup>836</sup>Pardee, *ibid.*, p. 21-23, 67. By the logic of an alternative interpretation proposed there, I could have preferred the interpretation of *trpā* as a dual: ‘‘*Attartu* does [X], ‘*Anatu* brings (something) back, the two of them heal’. What led me to interpret *trpā* as singular was that *tšdn* bears the ‘indicative’ {-n} while *trpā* does not. If the only function of the /YQTLa/ form in Ugaritic is volitive, the analysis of *trpā* as 3 f.s. /YQTLa/ is more difficult, for the verb is in a marked subordinate clause. I have difficulty accepting that view of /YQTLa/, however: see above, remark to p. 429 (§73.143), etc., below, remark to p. 456 (§73.264), and cf. Pardee, *Ritual and Cult* (2001) 160 (interpretation of *thtā* in RIH 78/20:5[*CAT* 1.169]) and 169 (interpretation of *trpā* and *ttrb* in RS 24.258 [*KTU* 1.114]).

<sup>837</sup>*AfO* 42-43 (1995-96) 271.

<sup>838</sup>*BSOAS* 58 (1995) 230; in *AuOr* 16 (1998) 88, I comment on T.’s acceptance of the reading {y<sup>l</sup>ql<sup>l</sup>n}.

<sup>839</sup>P. 268, n. 14.

<sup>840</sup>De Moor, *UF* 2, p. 324.

<sup>841</sup>Pp. 248-51.

<sup>842</sup>On p. 660 (§75.532), T. analyzes *ymgy* as a 3 m.s. and only alternatively as a 3 m.du.

text is {w . k ymgy (26) ‘bdk . l šlm (27) ‘mk . p l . yšb‘l (28) ḥpn . l b‘ly}), which cannot be said, as regards the use of the pronoun ‘my’, to make any sense.<sup>843</sup> And the emendation can only be qualified as arbitrary, for the text makes perfectly good sense as it stands. Though there are not many letters from two persons, in fact only two (this text and RS 8.315 [KTU 2.11]), the play of singular and dual forms in expressing portions of the message which are expressed as addressed from both or from one or the other of the two writers is consistently indicated in both texts. This passage may be translated something along the lines of: “And when your servant comes to tender to you his formal greetings, he will be sure to have a *ḥipānu*-garment made for my master ...” (i.e., the male of the two writers is here speaking).<sup>844</sup>

— p. 439 (§73.223.41.8), p. 589 (§74.622.3), p. 605 (§74.632), p. 790 (§83.122g), p. 812 (§85.8b). T. refuses not only to come to terms with the text of RS 29.093:25-28 (KTU 2.70), as we have seen in the preceding remark, and with the aspectual value of the verbs in this passage, as we shall see in the following remark, but he clearly has no idea what to do with the particle *l* in line 27: he translates it as “nicht” (p. 438), as “fürwahr/nicht” (p. 589), as “fürwahr” (p. 790), and once refers to it as a possible example of what he calls volitive *l* (p. 890)—but nowhere does he translate the passage. If the text be respected and the /YQTL/ forms be taken as imperfectives, *l* can only be the emphatic particle (by whatever name it may go) and the passage must be translated along the lines of: “... he will be sure to have a *ḥipānu*-garment made for my master ...”

— p. 439 (§73.223.41.8), p. 565 (§74.416.3), p. 589 (§74.622.3). As there are no provable examples of /YQTL/ forms having perfective or preterital value in prose, T.’s translations of *ymgy* in RS 29.093:25 (KTU 2.70) that reflect such a value cannot be admitted: the translation on p. 439 is quoted in the second previous remark; a similar translation is given on p. 589; on p. 565 one reads “Als/Wenn deine beiden Diener kamen/kommen, um zu bezahlen(?)”.<sup>845</sup> Only the interpretation as 3 m.s. and as some form of imperfective is plausible; the spelling with {y} in final position indicates an indicative, as I have translated above.

— p. 440 (§73.223.41.12), p. 441 (§73.223.42.10), p. 888 (§95.234). The sequence *y‘db ... tšb‘n* in RS 2.002:63-64 (KTU 1.23) causes T. no end of bother, but he appears to be more concerned about the different preformatives than about the fact that one is /YQTL/ the other /YQTLn/. Though the first discrepancy might, as T. suggests, be stylistic variation made permissible by the use of both preformatives in the dual, the conjugation of the two makes one wonder if something else is not going on here. I would suggest that both verbs in the first two cola of this tricolon are 3 m.s. and that the purpose of the play on grammatical

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<sup>843</sup>Neither does it make any sense to me to interpret the last clause as promising *not* to make a mantel for their master (see following remark).

<sup>844</sup>See my translation with notes in *Context III* (2002) 111 ; more recently, Bordreuil and Pardee, *Manuel* (2004) II 89.

<sup>845</sup>On pp. 495 (§73.534b) and 790 (§83.122g) one finds only the translation “kommen.” Cf. below, remark to p. 448 (§73.243.1) where the interpretation of {tšāl} in line 23 of this text as a jussive or a preterit is criticized.

number is to emphasize the fact that Dawn and Dusk are positioned at opposite extremities of the horizon:

|                                 |                                |  |
|---------------------------------|--------------------------------|--|
| <i>w ndd gzʿl l &lt;g&gt;zr</i> | /wa nadāda gazara lê <ga>zari/ | Each stands at one extremity, <sup>846</sup> |
| <i>yʿdb ũ ymn ũ šmāl</i>        | /yaʿdubu ʾô yamīna ʾô šamʾala/ | each consumes on right, on left;             |
| <i>b phm w ʿl tšbʿn</i>         | /bi pīhumā wa lā tišbaʿāna/    | (but of what is) in their mouths             |
|                                 |                                | never are they satisfied.                    |

— p. 441 (§73.223.42.7), p. 448 (§73.243.1), p. 460 (§73.273.7), p. 511 (§74.222.1), p. 569 (§74.422), p. 619 (§75.228). T. everywhere translates *tlākn* in RS 2.[008]<sup>+</sup> v 42 (*KTU* 1.4) as a passive, usually vocalized as a G-passive (“*tulʾakânî*” [pp. 460, 511, 569, 619]), once as a D-passive (“*tulaʾʾakânî*” [p. 511]), but also once as a G-active (“*tilʾakânî*” [p. 448]). The last is probably to be understood as a simple error, since T. translates even there as a passive and recognizes that LʾK is a true transitive verb.

— p. 442 (§73.223.5), p. 460 (§73.273.8), p. 627 (§75.42), p. 644 (§75.522), p. 685 (§76.321). Granted that the phrase *tnhn ũdmm* (RS 3.343<sup>+</sup> i 7' [*KTU* 1.15]) is difficult, one could nonetheless have asked for consistency of presentation: the problem is presented concisely on p. 442 (either *ũdmm*, derived from the city name *ũdm*, is dual and the verbal form is 3 f.du. or else *ũdmm* expresses [irregularly] a gentilic, ‘the people of *ũdm*’ [normally such a gentilic would be {*ũdmym*}]), on p. 460 the *tnhn* is presented as a possible 3 f.du. with a back-reference to p. 442, but on p. 627 only the analysis as 3 f.du is indicated while on pp. 644 and 685 only the analysis as 3 m.pl. Because the city is explicitly presented as a duality in the text on the first tablet of the *Kirta* cycle (RS 2.[003]<sup>+</sup> iv 47-48 [*KTU* 1.14] *ymgy l ũdm rbt w ũdm trrt*, ‘he/they arrive at Great ʾUdmu, at Well-Watered ʾUdmu’), the analysis as a dual appears the more likely.<sup>847</sup> If one wishes the form to be gentilic, emendation is required for there are no convincing case of gentilics without {-y-}.

— p. 442 (§73.223.6), p. 656 (§75.531e), p. 663 (§75.532). T.’s decision to parse *nšt* (and *nlhm*) in RS 2.002:71-72 (*CTA* 1.23) as 1 c.pl. (G-/YQTL/), rather than as 1 c.du., in a context where there are several third-person dual forms that refer to the same actors as those speaking in lines 71-72, is based on his view that the expected form is “*ništayā*” and the triphthong /ayâ/ should not monophthongize (p. 442). But on p. 656, he parses the form as a 1 c. pl. /YQTLa/ “*ništā*” contracted from “*ništayā*”—leaving open the possibility that the final vowel of the /YQTLa/ was also long. (On the unlikelihood that this vowel was long, however, see above, remark to pp. 428-29 [§73.142], etc.) It appears necessary to deal with the hypothesis that several of the 1 c. pl. and 1 c. du. forms of III-weak roots may have been identical or differed only by length of the final vowel. The paradigm may have been something like: /ništû/ (1 c.pl. indicative < /ništayu/— on this vocalization of the indicative ending, see remark below to p. 656 [§75.531e]), /ništa/ (1 c. pl. jussive/perfective < /ništay/), /ništâ/ (1 c. pl. ‘cohortative’ < /ništaya/), /ništâna/ (1 c.du. indicative < /ništayâna/), and /ništâ/ (1 c.du. jussive/perfective < /ništayâ/).

<sup>846</sup>Lit. ‘(Each) stands at an extremity with respect to (the other) extremity’.

<sup>847</sup>Cf. Del Olmo Lete and Sanmartín, *Diccionario* I (1996) 9.

— pp. 444-45 (§§73.233.41-42), p. 446 (§73.233.6), p. 579 (§74.511b), p. 678 (§75.673), p. 700 (§76.427a), p. 900 (§97.121). None of the forms cited on pp. 444-45 in proof of the assertion that the /YQTLØ/ 3 f.pl. form, i.e., the jussive/perfective, would end in {-n} is capable of proving the point. Three forms are cited from poetic texts, where no one, including T., has defined the use of the /YQTLu/ and /YQTLØ/ forms as systematically predictable, and a fourth, where the {-n} is entirely reconstructed, as well as the preceding sign, and half of the one before it, from a prose text. It is a complete mystery why this last form, cited as “tʿr[bn]” from RS 18.113:36' (KTU 2.42:24), is included since, in addition to the epigraphic problem of the word being largely reconstructed, including the morpheme under discussion, it would be in a conditional clause (it is preceded by *hm*) and there is no reason to expect a jussive form there and every reason not to expect a /YQTLØ/ perfective form there whatever the type of phrase may be (/YQTLØ/ perfectives occur neither in prose nor in conditional clauses). As for the *hypothesis* that the 3 f.pl. would have identical /YQTLu/ and /YQTLØ/ forms, it depends on whether the form is considered to have ended in a vowel or not, i.e., whether it was /taqtulā-, which would permit short and long forms (/taqtulā/ vs. /taqtulāna/) or whether the feminine suffixal morpheme itself was expressed by /na/, which would allow for only one form (for comparative reasons, the existence of a short form /taqtul/ vs. a long form /taqtulna/ does not appear likely). The strongest internal indicator that the latter hypothesis is correct comes, not from 3 f.pl. forms, but from 2 f.pl. forms. On p. 446 (§73.233.6), T. observes that the single 2 m.pl. form in RS 1.002 (KTU 1.40), written without {-n} (l. 31' {tqtṭ}), has its parallel in 2 f.pl. forms with {-n}<sup>848</sup>. The writing with {i} of one of these forms indicates /YQTLna/ as the probable pronunciation (*thṭin* in l. 22', 23', and plausibly restored in l. 19' = /tiḥta'na/). Because 3 f.pl. and 2 f.pl. /YQTL/ forms are often identical in the Northwest-Semitic languages, the 2 f.pl. form may be used as *prima facie* evidence for the vocalization of the 3 f.pl. form. Such an argument would have been far stronger than the one that requires contorting the data provided by {tnšān} (see remark above to pp. 52 [§21.341.21b], etc). As regards the interpretation of the 2<sup>d</sup> person plural forms in RS 1.002, T. consistently takes them as perfectives and includes them in his paragraph on /YQTLØ/ perfectives in prose (p. 700). None of the forms cited in the latter paragraph is convincing, however, and it appears necessary to find another solution to the use of /YQTLØ/ forms in RS 1.002. In fact, only one masculine form without {-n} is extant in that text ({tqtṭ} in line 31'), and that fact is balanced out by the identical spelling of one feminine form, viz., without {-n}, in the parallel text RS 17.100A+B:38' (KTU 1.84:7). If one admits, however, the evidence of {tqtṭ}, it is not necessary to see it functioning as a perfective. In this text, similarly to the use of the /YQTLa/ form *thṭā* in RIH 78/20:5 (CAT 1.169), the jussive forms may have been used to express hypothetical situations. A similar structure is found in the Tell Fakharia Aramaic

<sup>848</sup>Unfortunately, the data are not unambiguous: RS 1.002 consistently uses {-n} forms for 2 f.pl., but in the parallel text RS 17.100A+B:38' (KTU 1.84:7), the only attested 2 f.pl. form is written {tqtṭ}. T. suggests either emending to {tqtṭ<n>} or assuming that the {-n} was written at the beginning of the next line (p. 60 [§21.354.1a], p. 204 [§33.441], p. 579 [§74.511b], p. 678 [§75.673]); the latter solution does not appear likely, for this line is not long enough to have required the scribe to divide it across two lines (see hand copy in Pardee, *Les textes rituels* [2000] 1274).

inscription: there a lexically marked precative form is used in the protasis of a conditional clause, with the clear translation value of ‘should X do Y, then Z will occur’.<sup>849</sup> This use of a volitive form to express a hypothetical situation seems a plausible way of explaining both RS 1.002 and RIH 78/20:5.<sup>850</sup> Other options for explaining RS 1.002 are: (1) {tqṭṭ} in RS 1.002:31' is a simple error for {tqṭṭ<n>}, as may also be the case in RS 17.100A+B:38'; (2) though this is a prose text, there is extensive quasi-poetic repetition, and {tqṭṭ} in RS 1.002:31' may be an attempt at imitating poetic diction. Since the entire demonstration of the use of /YQTLØ/ in this text (p. 700) depends on the one form in RS 1.002:31', and since the similar feminine form in RS 17.100A+B:38' is apparently written erroneously without the {-n}, the hypothesis of a scribal error appears just as plausible as any of the others.

— pp. 444-45 (§73.233.41-42), p. 611 (§75.212.11), p. 672 (§75.61b), p. 675 (§75.62c). My collation confirms T.'s assertion (p. 675) that {tiggn} in RS 15.134:43 (*KTU* 1.82) may not be read as {thggn}: the lower vertical wedge, though somewhat damaged, is certain.

— p. 444 (§73.233.41), p. 579 (§74.511b). Not only is the reading of {t} as the prefix of the verb derived from 'ZZ, 'to be strong', impossible in RS 24.247+:20 (*KTU* 1.103),<sup>851</sup> but that verb is the only word extant in this line and the reconstruction of *mrḥy mlk* as its subject is only a guess—and a poor one at that, given that the preformative consonant is probably {y-}.<sup>852</sup> Though not epigraphically impossible as in line 20, the same reconstruction in line 48' is equally fanciful since the only remaining trace of the entire word is the tip of a horizontal wedge of the last sign of whatever word it was.<sup>853</sup>

— p. 445 (§73.233.43). Hebrew /tiqtōlnā/ cannot be considered as evidence for the affirmative element of the f.pl. /YQTL/ forms being /-nā/ because the Canaanite shift was operative in proto-Hebrew and proto-Hebrew /taqtulnā/ would have become /tiqtōlnō/. (One could equally well argue that the 2 m.s. morpheme in the /QTLa/ form was /tā/ because Hebrew has /tā—see remark above to p. 207 [§41.1].) Nor can Akkadian /iprusā/ be used as a datum in favor of vocalizing the Ugaritic morpheme as /-nā/, for these are different morphemes.

— p. 446 (§73.233.9), p. 460 (§73.273.9), p. 499 (§73.611.2a), p. 512 (§74.222.2), p. 623 (§75.237a), p. 733 (§77.412.3d). T. consistently parses *ṭḥtān* in RS 2.[008]<sup>+</sup> viii 20 (*KTU* 1.4) as /YQTLØ/ + energetic ending, viz., since this is a dual form, as ending in /ā/ (“ā” for T.) + his Energetic I (/anna/). Wherever he vocalizes, he provides his more-or-less standard segmental vocalization, with a dash between the verb and the energetic morpheme (e.g., p. 446: “*tuḥta'ā-nna*”) thus refusing to come to terms with the fact that the marking of the dual by /ā/ would disappear (the form should become /tuḥtanna/ with the Energetic I morpheme—of

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<sup>849</sup> See Biggs and Pardee, *JNES* 43 (1984) 254-55.

<sup>850</sup> On the place of the /YQTLa/ form in the Ugaritic verbal system, see above, remark to pp. p. 429 (§73.143), etc.; on *ṭḥtā* in particular, see remark below to p. 456 (§73.264).

<sup>851</sup> See the hand copy of the tablet in Pardee, *Les textes rituels* (2000) 1278, and the textual remark on p. 542.

<sup>852</sup> On the form of *mrḥy*, ‘lance’, see above remarks to p. 52 (§21.341.21b), etc., and pp. 306-7 (§51.121.2a).

<sup>853</sup> Pardee, *Les textes rituels* (2000) 545, 1278.

course, to retain the vowel length, one need only invoke Energic II, which would give /tuḥta'āna/). He also presents the final vowel in three different forms: “*tuḥta'ā-nna*” (pp. 446, 623), “*tuḥta'ā-nni/a*” (p. 499), and “*tuḥta'ā-nnV*” (p. 512). But the real question is whether the form is to be analyzed as /YQTLØ/ + energic, as one of the independent energic moods, as /YQTLa/ + enclitic *-na*, as /YQTLØ/ + *-na*, or as /YQTLu/. On the first two alternatives, see remarks below to pp. 497-506 (§73.6), etc., where it is claimed that T.’s view of the energics is insufficiently established; on the third, see remark above to p. 429 (§73.143), etc., where it is claimed that T.’s view that the form /YQTLa/ form was restricted to the 1<sup>st</sup> person is insufficiently established; on the fourth, see also remarks to pp. 497-506 (§73.6), etc., where the problem of distinguishing enclitic *-na* from the energic morphemes is discussed; on the last, see remark below to p. 721 (§77.322a), etc., where it is claimed that T.’s view that purpose/result clauses were expressed by /YQTLØ/ forms does not take sufficiently into account the data from Biblical Hebrew. Standing in the way of the third analysis is the fact that *āl*, the negative adverb that appears earlier in this sentence and on which the verb *ṯḥtān* must depend,<sup>854</sup> is as yet unattested with /YQTLa/ forms in passages of which the interpretation may be considered certain; in the way of the last the fact that one would not expect a /YQTLu/ form to follow the negative adverb *āl* (but, since this verb is in a separate clause with a different subject, must that stricture hold?). Given the various limitations of the data available, it appears best to parse the form as Energic II (/tuḥta'āna/), depending loosely on the preceding *āl*, where that particle modified directly what was almost certainly a jussive (*āl y'bdkm*, /'al ya'budkumā/, ‘that he not set the two of you’)—though it could be /YQTLa/ if one admit the construction consisting of that form negativized by *āl*. It is immediately apparent, however, that the analysis as Energic II is highly theoretical, for the form as vocalized according to this parsing was most likely identical to /YQTLu/, to /YQTLØ/ + *-na*, and to /YQTLa/ + *-na*. At best, from T.’s perspective, this passage might be taken as the proof text that the energic endings could be attached to /YQTLØ/ forms, for normally *āl* is followed by /YQTLØ/.

— p. 447 (§73.242.2), p. 458 (§73.273.1), p. 459 (§73.273.3), p. 500 (§73.611.2e). In the first two of these sections, T. cites RS 25.423:13 {iḥ-ra-bu}<sup>855</sup> as evidence for the root ‘RB, ‘to enter’, having a /yiqtal/ form; on p. 459 *t'rbn* in RS 19.015:11 (*KTU* 1.91) is vocalized /ta'ribūna/; in the fourth section cited, the imperative is indicated as /<sup>c</sup>Vrub-/. Thus the waterfront is covered.

— p. 448 (§73.243.1), p. 493 (§73.532), p. 494 (§73.533), p. 613 (§75.212.14),<sup>856</sup> p. 615 (§75.212.5), p. 643 (§75.522), p. 649 (§75.526a). The passage *nrn āl tūd ād āt lhm ttm ksp* in RS 16.264:19-21 (*KTU* 2.26) has caused no end of difficulty to its interpreters. T. proposes two different syntactic analyses with two morphological possibilities for *ād*: “Von(?) PN sollst du (kein Geld) einfordern! Fordere du (stattdessen) von(?) ihnen(?) /

<sup>854</sup>T. correctly negativizes this clause in his translations on pp. 499, 512, and 733, but neglects to do so on p. 446.

<sup>855</sup>An unpublished text cited by van Soldt in *BiOr* 46 (1989) 650, 651.

<sup>856</sup>The *KTU* reference is mistakenly indicated as 2.16 on this page and is so indexed on p. 1026.

Fordere (du selbst) für sie ...” (where *ād* is taken to be an infinitive functioning as an imperative);<sup>857</sup> or “Von(?) PN sollst du überhaupt kein (Geld) einfordern” (where *ād* is taken as an emphasizing infinitive); on p. 649, the alternative analysis of *ād* as a participle is indicated, but not the syntactic analysis to which that morphological analysis would belong. Tellingly, T. includes neither syntactic analysis below in the section on syntax. I know of no case where the independent pronoun is expressed in a phrase in which the infinitive is used as a paronomastic adverbial, and that solution appears ruled out. Though the use of the infinitive in place of an imperative is attested, it is not particularly common in prose, and if another solution is available it may be preferred. I have proposed that the verbal phrases *tūd* and *ād* may be derived from different verbs and constitute a sort of pun.<sup>858</sup> *tūd*, as most scholars have thought, is from <sup>ʾ</sup>(W)D, ‘to burden (someone with something)’;<sup>859</sup> but *ād* from <sup>ʾ</sup>DY, attested in Arabic with the meaning ‘to pay’ in the D-stem. The translation would be: ‘Do not burden Nūrānu; pay for them yourself, (a total of ) sixty (shekels of) silver’, with the appropriate vocalization /nūrāna ʾal taʾud ʾaddi ʾatta lêhumu/.<sup>860</sup>

— p. 448 (§73.243.1), p. 617 (§75.222), p. 633 (§75.512). In these three passages, T. refers to the division of signs in RS 3.322+ iii 47 (*KTU* 1.19) as {grb til} rather than as the commonly accepted {gr bt il}, ‘alien in the house of a god (i.e., who has sought asylum in a temple)’. In the first, he cites the first interpretation with a question mark, in the second he cites it but expresses his preference for {gr bt il} (the basis being that this would be the only attested /YQTL/ form of a root II-<sup>ʾ</sup> in Ugaritic that would show /i/ as the stem vowel), in the third he cites only {til} as an example of the root W<sup>ʾ</sup>L. On pp. 448 and 633, he vocalizes the form as /taʾil/, on p. 617 as /tâʾil/—the latter is, of course, the expected form according to T.’s principles (see discussion above). Nowhere in the grammar does he indicate what his interpretation of {grb} would be if the word *til* is in fact present.

— p. 448 (§73.243.1), p. 618 (§75.227). In the first paragraph cited, *lāk* in RS 18.113A+B:39' (*KTU* 2.42:27) is parsed as an imperative, in the second, with a question mark, as an infinitive (it goes unmentioned in §75.223, the section devoted to G-stem imperatives of II-<sup>ʾ</sup> roots). Since the letter was written by a servant of the king of Ugarit to the latter seeking instruction regarding the purchase of boats in Cyprus, the last line of the

<sup>857</sup>This interpretation goes back to del Olmo Lete and Sanmartín, *AuOr* 6 (1988) 261.

<sup>858</sup>*Context* III (2002) 101 n. 106.

<sup>859</sup>T.’s translations and analyses reflect this derivation and semantic analysis everywhere except on p. 448, where the translation is “du sollst zurückerstatten.”

<sup>860</sup>T.’s vocalization of the jussive form as /taʾûd/ (pp. 448, 613) is another example of maintaining a long vowel in a closed syllable, where it was in all likelihood short (see above, seventh general remark); his vocalization on p. 643 as /taʾud/ is correct and is another example of inconsistency (see above, third general remark). Curiously enough, on p. 448, T. provides what he considers to be the first two stages of development /taʾûd/ ← /taʾuwd/ while, on p. 643, he gives the last two /taʾud/ ← /taʾûd/. It is likely, however, that the form /taʾûd/ never existed as such, for the basic rule is that vowels that for one reason or another would be expected to be long appear as short if the syllable is closed. So the indicative form /taʾûdu/ would have had the long vowel characteristic of hollow roots, but the jussive would never have shown it.

letter must be expected to have requested that the king send his decision: {w . mlk . ʾlālk . ʿmʾ-l[...] } ‘So, O king, send m[e (a message) ...]’.

— p. 448 (§73.243.1), p. 616 (§75.222), p. 810 (§85.7b). *tšāl* in RS 29.093:23 (*KTU* 2.70) is plausibly not a second-person form and is certainly not a perfective (“du sollst(?) fordern / hast gefordert”) because this is a prose document. The phrase is {w . k tšāl (24) bt . ʿbdk}, where *k* appears to be the adverb ‘thus’ rather than the conjunction ‘for/when’<sup>861</sup> and the subject of the verb appears to be *bt* ‘bdk, ‘the house(hold) of your two servants’. Since *bt* is a masculine noun, *tšāl* would be a plural form referring to the members of the household: ‘Moreover, thus must the (members of) the house(hold) of your two servants ask’ (/wa kā tišʾalū bētu ʿabdêka/). The reference is to the fact that the master of the two servants has neglected properly to provision their household and its members must request that with which they should be provided automatically.<sup>862</sup>

— p. 449 (§73.243.21). For *iph* in RS 16.196:4' (*KTU* 2.25), T. proposes one vocalization (/yiphâ/), two possible derivations (/yiphay/ and /yiphaya/), and three possible translations (“ich sehe/sah/will sehen”). The translation as a perfective is highly unlikely in this prose text and I do not see the basis in the derivations for the translation as a simple present. A derivation as a volitive is indeed to be preferred. As stated above (remark to p. 190 [§33.311.5], etc.), there is no reason to expect the final vowel to have been long in III-weak jussives, and there may, therefore, have been a marked difference between the jussive of these roots and the /YQTLa/-volitive (in the case of the verb in question, jussive /yipha/ vs. /YQTLa/-volitive /yiphâ/).<sup>863</sup>

— p. 449 (§73.243.21), p. 660 (§75.532). On p. 449, T. lists *ihd(n)* in RS 15.007:7, 10 (*KTU* 2.15) as possible examples of /yiqtal/ forms with the remark that the interpretation of the two forms is controversial; on p. 660, he lists them as certain examples of the verb ʾHDY, ‘to rejoice’, with no other possible interpretation indicated. Because one finds other examples of {i} for expected {â} in this text, it is legitimate to see here ʾHD, ‘to seize’ (cf. Hebrew *yeʿehōz*), an interpretation that goes back to the *editio princeps*.<sup>864</sup>

— p. 449 (§73.243.21), p. 522 (§74.232.21), p. 628 (§75.44), p. 671 (§75.538), p. 739 (§81.12a). Another form listed on p. 449 as a possible /yiqtal/ is *its* in RS 3.367 iv 4' (*KTU* 1.2), but T. himself refutes the derivation of the form from a hypothetical root NTS on p. 522, pointing out that the sequence {ts} is “praktisch nicht vorkommen” in the Semitic languages. Hence his reticence to accept the only plausible analysis, viz., as 1 c.s. Gt-/YQTL/ of NSY,

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<sup>861</sup> On p. 810, T. suggests hesitatingly that *k* is the emphatic particle and that ŠʾL here has the meaning of ‘to concern oneself with’: “Und du sollst dich fürwahr(?) kümmern(?) um das Haus deiner beiden Diener.” The interpretation is ingenious but does not reflect standard Ugaritic prose. The servants do not want their master to ‘ask about’ their welfare but to do something about it.

<sup>862</sup> Pardee, *Context III* (2002) 111.

<sup>863</sup> E. E. Knudsen, “The Mari Akkadian Shift *ia* ← *ê* and the Treatment of 𐤢𐤏 Formations in Biblical Hebrew,” *JNES* 41 (1982) 35-43, esp. pp. 40-41, proposes that Hebrew /yiqte<sup>h</sup>/ is from /yaqtiya/, in favor of which all other forms had disappeared in Biblical Hebrew. All the evidence goes against the hypothesis that such a systematization along the lines of a single form that we encounter in Hebrew would already have existed in Ugaritic.

<sup>864</sup> Virolleaud, *PRU II* (1957) 41.

‘to displace, expel’, appears to be owing more to ancillary considerations than to the simple analysis of the form at hand. These considerations are: (1) he believes that the parallel verb, *ānšq*, is from the root NŠQ, ‘to kiss’, rather than from NŠQ (← NŠQ), ‘to burn’ (see below, remark to p. 550 [§74.412.24]), which would require that *its* convey a positive notion rather than a negative one; (2) he is loath to accept the existence of 1 c.s. jussives, believing that the /YQTLa/ form is “offenbar zumeist ... gebraucht” (p. 721 [§77.321])—this would mean that the final triphthong of /\*<sup>ʔ</sup>intasiya/ has monophthongized, an irregular contraction (pp. 195-96 [§33.322.2]). But, if T.’s views regarding the distribution of the /YQTLØ/ and /YQTLa/ forms are too restrictive (see above, remark to p. 429 [§73.143], etc.), then all the elements of this passage fall coherently into place:

|  |   |  |
|--|---|--|
| (2') [-----]hy <sup>l</sup> -l[---] <i>l āšši . hm .</i> [...] | I will certainly force them out,              | [...] <i>la ʔašōši ʔhumu</i>                   |
| <i>āp . āmr [ ]</i> (3') [----]                                | yes, I will expel[...].                       | <i>ʔapa ʔamri[...]</i>                         |
| <i>w b ym . mnḥ l ābd .</i>                                    | In Yammu I will destroy the resting place,    | <i>wa bi yammi manūḥa la ʔabbid</i>            |
| <i>b ym . irtm . m[...]</i>                                    | in Yammu, at the heart (I'll destroy) M[...], | <i>bi yammi ʔirtama M[ ]</i>                   |
| (4') [ <i>b tṭiʔ</i> ] . <i>nhr . tlʿm .</i>                   | [in Ruler] Naharu, (I'll destroy) the neck.   | <i>bi tṭiṭi naḥari tala ʿama</i>               |
| <i>tm ḥrbm . its .</i>   | There, with the sword, I'll expel (him),      | <i>tamma ḥarbama ʔittasi</i>                   |
| <i>ānšq</i> (5') [ <i>b]htm .</i>                              | I'll burn (his) palace to the ground.         | <i>ʔanaššiq bahatīma</i>                       |
| <i>l ārs . ypl . ūlny .</i>                                    | The strong one will fall to the earth,        | <i>lê ʔarši yappulu ʔulāniyyu</i>              |
| <i>w l . ʿpr . ʿzmny</i>                                       | to the dust the mighty one.                   | <i>wa lê ʿapari ʿažūmāniyyu</i> <sup>865</sup> |

— p. 450 (§73.243.22a), p. 590 (§74.622.3). T. is loath to accept that {āšlw} in RS 2.[003]<sup>+</sup> iii 45 (KTU 1.14) be the correct reading (he would emend to {t<sup>ʔ</sup>šlw} and analyze the form as √LWY Š-stem). He has preferred, however, to ignore two important factors: (1) that the root ŠLW retains the third root consonant in a remarkable fashion in Biblical Hebrew as well as in Biblical Aramaic, (2) that the meaning ‘to rest, find repose’, i.e., that of the root ŠLW, fits the context remarkably well.

— p. 450 (§73.243.22b), p. 632 (§75.511g), pp. 633-35 (§75.512). In the first section cited, T. vocalizes all 1 c.s. /YQTL/ forms of I-y ← I-w roots with a short vowel (e.g., *abl* /ʔabil-/ , “Ich will/werde trage(n)”), whereas in the second he proclaims that “In dieser Grammatik wird der letztere MphT [yâ2i3] vorgezogen... .” Following the latter principle, most such forms in §75.512, where all such attested forms are cited but not all vocalized, are indicated as having /â/ in the first syllable. On my disagreement with this principle (which means that I actually prefer his vocalizations on p. 450), see remark below to pp. 630-32 (§75.511d-g). On the particular problem of YTN, ‘to give’, of which the 1 c.s. is here vocalized /ʔatin-/ , see remark below to p. 597 (§74.624), etc.

— p. 451 (§73.243.22b), p. 645 (§75.522). On p. 451, T. vocalizes the 1 c.s. /YQTL/ of ŠD, ‘to hunt’, as “*ʔašîd(u)*,” (with the remark that the root may be ŠWD (which means for him that the /YQTL/ would be “*yašûd-*”) and the same form of ŠḤ, ‘to cry out’, as “*ʔašîḥ-*”(with no remark as to a possible alternative origin); on p. 645, each is presented with alternative forms, “*yašû/îd-*” and “*yašî/ûḥ-*.” The hesitation apparently comes from the fact, though he does not say so, that the data from the other Semitic languages vary: in the first case, Hebrew shows /yašûd/, Arabic both /yašîd-/ and /yašâd-/ , in the second the imperfective is

<sup>865</sup> Bordreuil and Pardee, *Manuel* (2004) II 9, 11.

attested only once in Hebrew, then with a ‘strong’ formation (*yīṣwāḥū<sup>w</sup>*) while Arabic shows /*yaṣīḥ-*/. In neither case can the Ugaritic form be /*yiḳāl-*/ because the 1 c.s. form is attested for each and the writing with {ā} shows the vowel of the preformative to be /a/ (the Barth-Ginsberg law says that /*yaṣād-*/ would have become /*yīṣād-*/ in Northwest Semitic). Because T. has shown himself so willing to make more-or-less arbitrary decisions elsewhere and has indeed done so in his presentation of these verbs on p. 451, I see no reason why he should not have done the same here—there is no obvious reason why a grammar so given to reconstruction as this one should suddenly pull up short and refuse to make a decision on p. 645. My standard methodology is to favor the Northwest-Semitic data over the Arabic when they conflict (because Ugaritic shows more fundamental isoglosses with Northwest Semitic than with Arabic), unless there be an overpowering theoretical reason not to do so. In these cases, that means that ŠD should be /*yaṣūd-*/ (because that is the Hebrew form) and ŠḤ should be /*yaṣīḥ-*/ (because the Hebrew form is here irrelevant: /*yīṣwāḥ-*/ is formed like a strong root and simply follows the rule according to which III-guttural roots with an historically short stem vowel form the /YQTL/ on the /*yiḳtal*/ pattern). For a similar problem with GL, ‘to rejoice’, see below, remark to p. 500 (§73.611.2d). However one may view the problem of reconstruction, it is probably safe to say that most users of the grammar would at least have preferred consistency of presentation.

— p. 452 (§73.243.22e), p. 633 (§75.512). The epigraphic situation in RS 17.139:28 (*KTU* 2.34:30) is too uncertain<sup>866</sup> to lend any plausibility whatever to T.’s view that the signs {āḏ<sup>c</sup>} may be found there which would constitute an anomalous 1 c.s. form of YD<sup>c</sup>, ‘to know, which is elsewhere written, as expected, {īḏ<sup>c</sup>}.

— pp. 453-54 (§73.245.2), p. 868 (§93.361). In these sections, T. asserts that in Ugaritic, as in Arabic and Akkadian, “Durch Veränderung der Themavokale im Grundstamm kann ein verb in eine andere semantische Gruppe überführt und seine Valenz verändert werden” (p. 868). Though this may be the case, there is no proof available from II-’ roots that such is the case, viz. /QTLa/ forms written both {qāl} and {qīl}, nor does T. offer on pp. 453-54 any really convincing examples from usage that make the case any solidier.

— p. 456 (§73.263). In my translation of *yqrā* in RS 2.[008]<sup>+</sup> vii 47 (*KTU* 1.4), I expressed the /YQTLa/ as an iterative.<sup>867</sup> There is no real basis for such a grammatical analysis of that form, however, and seeing in the utterance an ironic volitive would be a better reflection of the morphology and context: “Let *Môtu* proclaim (all he wants) // Let the beloved one (of *’Ilu*) claim (to his heart’s content).” T. translates “er soll rufen,” but does not explain the meaning of the form in context. He translates *ymzā* in RS 2.[012] i 37’ (*KTU* 1.12) first as “er fand,” acknowledging however by his alternate translation “auf daß er finde” that the form may well express volitivity.<sup>868</sup>

— p. 456 (§73.263), p. 658 (§75.531f), p. 659 (§75.532). T.’s proposal to analyze {ybky} in RS 34.126:15 (*KTU* 1.161) as a /YQTLa/ volitive is certainly superior to Bordreuil’s and my

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<sup>866</sup>Pardee, *AfO* 31 (1984) 227; idem, *Les documents épistolaires* (in preparation).

<sup>867</sup>“*Môtu* is always proclaiming” (*Context I* [1997] 263).

<sup>868</sup>Cf. Parker *apud* Parker, ed., *Ugaritic Narrative Poetry* (1997) 189: “That he might meet the Tearers.”

/yabkiy/ (i.e., a jussive without monophthongization of the /iy/ diphthong),<sup>869</sup> probably also to the analysis as /YQTLu/.<sup>870</sup> There is no real reason to believe that the /y/ of the /yaqtiy/ jussive would have remained in so late a text, and the analysis of the {y} as a *mater lectionis* (T.'s third proposal on p. 456, his second on p. 659) is an even less desirable solution. It is unclear why, on p. 658, he refers to the final vowel according to the /YQTLa/ hypothesis as “ein ‘emphatisches’ /ā/”; the epithet would appear to reflect his dubiety about the existence of the /YQTLa/ volitive—on the other hand, he presents no case for an independent emphatic morpheme /-ā/. This is, however, once one rejects /yabkiy/ as highly implausible and /yabkiyu/ as on the borderline, one of the more convincing examples of the third-person /YQTLa/ volitive.<sup>871</sup> Finally, on p. 659, he proposes yet a fourth analysis, that of a G-passive.

— p. 456 (§73.264), p. 620 (§75.232). One may doubt two aspects of T.'s treatment of *thṭā* in RIH 78/20:5 (CAT 1.169): (1) that the {ā} represents quiesced {ʾ}, i.e., /tiḥṭā/, and (2) that the verb is to be taken as a perfective (on p. 456, T. translates: “Du hast dich versündigt”). The form is far more plausibly /YQTLa/ and the usage of this form may be explained as expressing an eventuality in the protasis of what is formally a conditional clause.<sup>872</sup>

— p. 463 (§73.313), p. 610 (§75.211), p. 614 (§75.212.3), p. 716 (§76.534), p. 718 (§76.55), p. 819-20 (§88.1). T. asserts that the particle of existence *it* functions as a finite verbal form in Ugaritic and concludes therefrom that in Ugaritic, as in Akkadian, substantives could assume a verbal conjugation. In the last paragraph cited, his reasoning takes another step: since verbal forms are attested, *it* itself may have to be parsed as 3 m.s. /QTLa/ (“Tatsächlich ist die Abgrenzung von *it* als nominaler Part[ikel] und *it* als verbaler S[uffix] K[onjugation]-Form 3.m.sg. (bzw. 3.pl.) bei zahlreichen Belegen problematisch.”) All of this speculation founders, however, on the fact that none of the three examples of verbal usage cited is certain, other interpretations having been offered for each.<sup>873</sup> No inkling of this uncertainty is provided by T. In particular, the presentation on p. 614 of these

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<sup>869</sup> Bordreuil and Pardee in *Une bibliothèque au sud de la ville* (1991) 154; Pardee, *Les textes rituels* (2000) 819

<sup>870</sup> This is T.'s second preference on p. 456, his first on p. 659 (it was his first in *UF* 29 [1997] 672, n. 8), and a possibility considered by myself in *Ritual and Cult* (2002) 114 n. 126, where the analysis as /yaqtila/ is preferred.

<sup>871</sup> This is the vocalization now proposed in Bordreuil and Pardee, *Manuel* (2004) II 64.

<sup>872</sup> For this way of looking at the text, see Pardee in *Verse in Ancient Near Eastern Prose* (1993) 212; idem, *Les textes rituels* (2000) 877; idem, *Ritual and Cult* (2002) 160; Bordreuil and Pardee, *Manuel* (2004) II 67, 68; and remark above to pp. 444-45 (§73.233.41-42), etc. In Ford's recent re-interpretation of this text, he accepts the analysis of *thṭā* as /YQTLa/ (*UF* 34 [2002] 155, 172-74). I am not convinced by his arguments for taking ḤṬ' as meaning 'to harm' (the meaning here would be different from that attested in RS 1.002 [KTU 1.40] because the texts belong to different literary genres), but that question is tied in to the overall interpretation of the incantation.

<sup>873</sup> The present reviewer has never accepted the interpretations of the forms written {it} as verbal forms, though he realizes that further data could prove the existence of the form (cf. *AfO* 31 [1984] 224-25, *Context* I [1997] 336).

presumed verbal forms as constituting an exception within the family of I-ʾ verbs, all the rest of which show {ā} in the /QTLa/, could at least have included an indication that not everyone accepts the existence of this particular anomaly. Here he holds that the root may be III-y and the forms which he takes to be /QTLa/ “etymologisch mit der Partikel *it* verwandt und möglicherweise von dieser abgeleitet sind.” For a verb to show an /i/-vowel in the first syllable, it would have to have developed directly from the particle (which would historically have been /ʾiṭa/), not from the root ʾT, which would have given /ʾaṭa/ or /ʾāṭa/; √ʾTY would also have given /ʾaṭa/ or /ʾaṭā/. The question turns around the comparison with Akkadian *išû*, ‘to have’, and Arabic *laysa*, ‘not to be’, the latter of which plausibly arose from a contraction of the negative particle *lā* and a particle related to Ugaritic *it* and Hebrew *yēš*. There being as yet, however, no proof that the Ugaritic forms were verbal, the question must, at the very least, be left open. Finally, though he cites the passages, T. does not deal with the implications of the one clear case of *it* modifying a feminine form and not itself showing gender agreement: in the double formula *hm it šmt hm it ʿzm*, ‘whether there be fat, whether there be bone’, in RS 3.322+ iii 4 and parallels (*KTU* 1.19), there can be no doubt that *it* is functioning as a quasi-verb but the form is identical before the feminine and masculine nouns. Reasoning from this case, one would have to refute T.’s general assertions that we really do not know whether *it* is nominal or a 3 m.s. (or pl.) verbal form by observing that there is no correspondingly clear evidence indicating that they may be verbal.

— p. 464 (§73.331.1). The inquiring mind would like to know how the syllabically written forms {ša-ma-TA} (RS 15.086:16 [*PRU* III, p. 51]) and {šú-um-mu-TA} (RS 16.174:12 [*PRU* III, p. 68]) could both be passives, i.e., both are translated “wurde ... übergeben”). The first form is cited as proof of the vocalization /qatala/ for the /QTLa/ of G-stem “fientisch-aktivisch” forms (p. 462), the second is cited under the heading “vgl.” but appears to be parsed nowhere in this grammar (RS 16.174 is totally absent from the index of syllabic texts on p. 1050).

— p. 464 (§73.331.1), p. 516 (§74.223.2), p. 623 (§75.237a), p. 838 (§91.11c). Though T.’s analysis of the form written {qrā} in RS 34.126:4-7, 11-12 (*KTU* 1.161) as G-passive, following the editors’ analysis,<sup>874</sup> is undoubtedly correct, his reconstruction of the subject of the verb in lines 4 and 5 as {rp[ā]} (p. 838) cannot be so readily admitted, for the subject of a passive verb should be in the nominative case and the noun in question should have been written {rpū}. The reconstruction of this noun with {ā} is not explained, but it would appear to be linked to T.’s analysis of the plural oblique form *rpim* in line 8 as having a nominative function (see above, remark to p. 307 [§54.121.2b], and below, remark to p. 465 [§73.332.1], etc.).

— p. 464 (§73.331.1), p. 888 (§95.233). In the first section cited, {qrā} in RS 34.126:6 (*KTU* 1.161) is included with no hesitation among 3 m.s. forms; in the second, it is observed that the form may be dual (the subject is a royal/divine name of the form X-w-Y.

<sup>874</sup>Bordreuil and Pardee, *Syria* 59 (1982) 123, 126; idem, in *Une bibliothèque* (1991) 154, 155, 156-57; Pardee, in *Verse in Ancient Near Eastern Prose* (1993) 209; idem, *Les textes rituels* (2000) 818-19, 821; idem, *Ritual and Cult* (2002) 87; Bordreuil and Pardee, *Manuel* (2004) II 63, 64.

Complements of such divine names are sometimes singular, sometimes dual [see remark below to p. 887, §95.231]).

— p. 465 (§73.331.3), p. 670 (§75.539). T. reconstructs the 2<sup>d</sup> person singular pronominal elements in the /QTLa/ as /-tā/ (m.) and /-tī/ (f.). While some ambiguity may be said to exist with regard to the length of the vowel in the 2 f.s. form, the comparative data for the 2 m.s. (Arabic, Akkadian, Hebrew, Aramaic ...) leave no doubt that Ugaritic would have stood alone in its area if it had /ā/ in the 2 m.s. Leaving the question entirely open, as T. does here, borders on the irresponsible in a grammar so dependent on comparative data for its reconstructions of forms (see remarks above to p. 143 [§33.115.11] and to p. 207 [§41.1]). One must also conclude that the vowel of the 2 f.s. in proto-West-Semitic, as well as in Akkadian,<sup>875</sup> was short, for it disappears in both Hebrew and Aramaic and is short in Arabic; sporadic cases of /ī/ represent secondary lengthening, therefore, not retention of an original long vowel. That being the case, there is no reason to expect Ugaritic to have shown /ī/ in this form.

— p. 465 (§73.331.3,4). In §73.331.3, under the heading of 2<sup>d</sup> person singular forms, T. cites *ḡmīt* (RS 2.[008]<sup>+</sup> iv 34' [KTU 1.4]) as indicating the absence of a vowel between the verbal stem and the pronominal element ({*ḡmīt*}, 'you are thirsty', must be /ḡami'ti/, not /ḡami'āti/). In §73.331.4, under the heading of the 1 c.s. /QTLa/ form, three forms are cited to prove the absence of a vowel between the verbal stem and the pronominal element, viz., that the form was /qataltu/ not /qatalātu/. One form shows only one {t} for the expected two {{smt} for /ṣamat + tu/ in RS 2.[014]<sup>+</sup> iii 44' [KTU 1.3]), the second shows assimilation of non-emphaticness ({mḥšt} for /maḥṣ + tu/ in the same text, col. iii, lines 38, 41, 43, 45), while the third shows assimilation of the third radical /n/ ({ytt} for /yatan + tu/ in RS 24.244:75 [KTU 1.100] and in RS 22.003:6 [KTU 4.710] a text of which the language is not certainly Ugaritic). Forms from the same root as the last are, however, attested in texts awaiting final publication that show the orthography with {n}: {ytnt}, 'I gave', in RS 94.2406:4,<sup>876</sup> RS 96.2036:9, and RS 94.2284:4, and the same writing for the 2 m.s. form 'you gave' in line 31 of this last text.<sup>877</sup> Similar writing is attested for the Š-stem of this root: {štnt} in RS 94.2479:21 meaning 'I have had delivered'<sup>878</sup> (the same writing is attested in a broken context in RS 94.2580:18). Similar data exist for hollow roots, where the endings /-ātu/-āta/ are very plausible (see below, remark to p. 642 [§75.521c]). Because of the mixed data for strong roots, one must await more examples before proposing an overall hypothesis—though YTN is the only triconsonantal root that behaves erratically according to presently known data, and it, of course, shows various characteristics of weak roots in Ugaritic and the other Semitic languages. The possibility must, in any case, remain open that forms with and without a vowel between the verbal stem and the pronominal element

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<sup>875</sup> Gelb, *Sequential Reconstruction* (1969) 64, 66.

<sup>876</sup> I have cited this passage in *JAOS* 121 (2001) 136; see now Bordreuil and Pardee, *Manuel* (2004), text 31 in the *Choix de textes*.

<sup>877</sup> The last two mentioned texts may also now be consulted in the *Manuel*, RS 96.2039 as text 33 and RS 94.2284 as text 34.

<sup>878</sup> Text 32 in the *Manuel*.

existed side by side in Ugaritic (so T., p. 642 [§75.521c]), as is the case with hollow-root Hiphils in Hebrew {*h<sup>a</sup>qīmōtā* ≈ *hēqamtā*}.

— p. 465 (§73.332.1), p. 516 (§74.223.2), p. 623 (§75.237), p. 892 (§96.22a). T.'s argument in *Anm.* 2 to §74.223.2 that *qrū* in RS 34.126:8 must be a G-passive /QTLa/ form because it agrees in person, gender, and number with its putative subject, viz., *rpīm*, ignores the fact that, in order to be the subject of the verb, that noun would have to be in the nominative case, i.e., be written {*rpūm*}, rather than {*rpīm*}. Hence the editors' decision to parse *qrū* as active rather than passive<sup>879</sup> (cf. above, remark to p. 307 [§54.121.2b]).

— p. 465 (§73.332.1), p. 622 (§75.234). T. makes no attempt to explain how *mlū* (RS 1.003:[18], RS 18.056:20 [KTU 1.41, 1.87]) could in context be a 3 m.pl. /QTLa/ form of the G-stem, 'they are full'. In both sections cited, T. cites RS 18.056, where {*mlū*} is extant; in the first, he declares the context to be unclear, in the second, to be broken. Though both RS 1.003 and RS 18.056 are in fact broken, comparison of the two permits a complete reconstruction of this passage; qualifying one or the other as 'broken' is, therefore, only superficially correct. Moreover, the context becomes considerably clearer if one does not attempt to analyze this word as a finite verbal form. Indeed T.'s alternative analysis, viz., as a nominal form, is the only one that I have found to be workable in context.<sup>880</sup>

— p. 467 (§73.332.3), p. 560 (§74.414.2), p. 670 (§75.537d). In the first section cited, T. states that *šltm* in RS 3.367 i 24' (KTU 1.2) may be D-stem or G-stem; in the other two sections cited, only the analysis as D-stem is admitted. Since the verb is clearly attested as intransitive,<sup>881</sup> surely this example, where the meaning must be transitive,<sup>882</sup> is better parsed as D-stem.

— p. 467 (§73.332.4), p. 597 (§74.624), p. 636 (§75.514), p. 664 (§75.534), p. 734 (§77.413), p. 778 (§82.421). The two epistolary texts RS 17.063 (KTU 5.10) and RS 17.117 (KTU 5.11)<sup>883</sup> contain a total of five /QTLa/ forms from four different roots all showing the ending {-tn}. On p. 467, T. proposes that all five are to be interpreted as 2 f.pl. forms, though he does so very hesitantly (viz., in the discussion he admits that "Alternative Deutungen sind möglich und wohl vorzuziehen"). On p. 597, he indicates first this analysis of *štnn* in RS 17.063:4 but adds that the form may be 1 c.s. or 2 f.s. On p. 636, he analyzes *ytn* in RS 17.117:5 as 1 c.s. but with the indication "Deutung unsicher." On p. 664, he considers that *hytn* may be a /QTLa/ form of HYY, but considers the grammatical person to be uncertain. On p. 734, he presents as possible interpretations of these various forms that they may be 1 c.s. /QTLa/ forms. Finally, on p. 778, he translates *ytn* in RS 17.117:4 as "halte ich(?) mich(?)." What I find astounding from a grammarian of T.'s expertise is that

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<sup>879</sup> See references in note 874.

<sup>880</sup> *Les textes rituels* (2000) 174-75. For a side-by-side presentation of the two texts and their reconstruction, see now *Ritual and Cult* (2002) 59-62.

<sup>881</sup> Del Olmo Lete and Sanmartín, *Diccionario I* (1996) 158.

<sup>882</sup> As the context requires; for recent translations, see Pardee, *Context I* (1997) 246; Smith *apud* Parker, ed., *Ugaritic Narrative Poetry* (1997) 99; Wyatt, *Religious Texts* (1998) 60.

<sup>883</sup> On this identification, see above, remark to p. 179 (§33.231.22), etc.

(a) he does not observe that, for epistolographic reasons, all five are most easily interpreted in context as 1 c.s. forms and (b) he does not bother to refute this latter analysis,<sup>884</sup> simply assuming it to have no more validity than the others.

— p. 467 (§73.332.5). T. comes to no conclusion regarding the vowel of the pronominal element of the 1 c.pl. /QTLa/ form, viz., whether it be /-nã/ or /-nũ/. He does not cite here, any more than above when he was dealing with the accusative/genitive pronominal suffix, Huehnergard's proposal<sup>885</sup> that this latter form may be attested syllabically as /-nũ/ (see remark above to p. 214 [§41.21]); if one accept that this was the form of the one suffix, it must be judged likely that the pronominal element in the verb was identical.

— p. 468 (§73.333.1). Here T. leaves open the question of whether MGY, 'to arrive', was /qatala/ or /qatila/ in the suffix conjugation; everywhere else he indicates /qatala/ (p. 196 [§33.322.3b], p. 655 [§75.531d—here he states that the 2 m.s. form may be /maġîta/ "im Einklang mit dem he. Befund" but does not indicate the historical derivation], p. 664 [§75.534]).

— p. 468 (§73.333.1), p. 516 (§74.223.2), p. 639 (§75.517a), p. 833 (§89.31). Against T.'s interpretation of *yldy* in RS 2.002:53 (*KTU* 1.23) as /QTLa/ G-stem 3 m.du. + enclitic -y speaks line 60, where, in an identical dialogic structure, the answer to the question 'What have they (the two women) borne?' is in the form of a nominal sentence (simply *ilmy n* 'my ..., 'the gracious gods'). On the basis of that comparison, the answer *yldy šhr w šlm* would not mean 'Šaḥru-wa-Šalimu are born', but '(They have borne) two offspring (namely) Šaḥru-wa-Šalimu'.<sup>886</sup>

— p. 468 (§73.333.1), p. 534 (§74.32), p. 626 (§75.42), p. 672 (§75.61c), p. 674 (§75.62b), p. 677 (§75.672). In 1988, Tropper and Verreet published an article on the Ugaritic roots NDY, YDY, HDY, NDD and D(W)D<sup>887</sup> in which they argued that the forms *ndd*, *ydd*, and *tdd* which show a meaning 'to take a position, stand, stand up' are N-stem forms of a hollow root rather than from a root NDD. Here T. classifies all such forms as from a root NDD and cites in support an article of his dating to 1997<sup>888</sup> in which he endeavored to show that Akkadian *izuzzu/uzuzzu* arose from a root NDD and cited as cognates Ugaritic NDD and Arabic *nadda*. I would not presume to speak to the Akkadian question,<sup>889</sup> but the Ugaritic question is open, the Arabic root cited does little to prove his case (*nadda* means 'to flee', not

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<sup>884</sup> Pardee, *AfO Beiheft* 19 (1982) 40-41, 45.

<sup>885</sup> *Syria* 74 (1997) 219.

<sup>886</sup> Pardee, *Context I* (1997) 281; Bordreuil and Pardee, *Manuel* (2004) II 30, 33 (though it appears to me now that the vocalization should have been /yaldêya šaḥra wa šalima/, i.e., with accusative case vowels on the names, rather than "yaldêya šaḥri wa šalimi"). Lewis *apud* Parker, ed., *Ugaritic Narrative Poetry* (1997) 212, seems to have adopted an analysis like T.'s, for he translates "a pair is born"; so Wyatt, *Religious Texts* (1998) 332 "have been born."

<sup>887</sup> *UF* 20, pp. 339-50.

<sup>888</sup> *AoF* 24 (1997) 189-210, esp. 204-8.

<sup>889</sup> See the detailed arguments of J. Huehnergard, "izuzzum and itūlum," *Riches Hidden in Secret Places: Ancient Near Eastern Studies in Memory of Thorkild Jacobsen* (ed. T. Abusch; Winona Lake: Eisenbrauns, 2002) 161-86.

‘to stand up’ [this root provides, therefore, an important etymological parallel for the Ugaritic root NDD that means ‘to flee’], and T.’s appeal to the III<sup>d</sup> Form, which means ‘to struggle’, is irrelevant for it does not mean ‘to stand up against’ but is probably little but a reflection of Form II meaning ‘to oppose someone in speech’; *naḏḏa* means ‘to urinate’—an anthropological linguist might make something of that!), and he ignores Hebrew *m<sup>2</sup>zūzā<sup>h</sup>*, ‘door-post’, exactly the form and meaning that one would expect from an original hollow root DD.<sup>890</sup>

— p. 468 (§73.333.1), p. 483 (§73.513.5b), p. 617 (§75.224). p. 618 (§75.227a), p. 666 (§75.536a), p. 888 (§95.234). In the first section cited, the word {lā} that appears three times in the phrase {lā šmm}, ‘the heavens are powerless’<sup>891</sup> (RS 2.[014]<sup>+</sup> v 18 [KTU 1.3], RS 2.[008]<sup>+</sup> viii 22 [KTU 1.4], RS 2.[009]<sup>+</sup> ii 25 [KTU 1.6]), is analyzed as a 3 m.s. /QTLa/ G-stem form, in the second and fourth as an infinitive, while in the third, fifth, and sixth both analyses are mentioned. The final /m/ shows *šmm* not to have been singular in Ugaritic, and a polyglot vocabulary shows the form to have been plural (/šamûma/, not /šamâma/), as T. recognizes on p. 468; one wonders, therefore, how the analysis as 3 m.s. would be explained in context—on p. 888, the phrase is cited as a possible example of number disagreement, but the analysis as an infinitive is preferred). Though I also have preferred the analysis of *lā* as the infinitive,<sup>892</sup> one might consider the possibility that some cosmological distinction was made between plural and dual ‘heavens’, with the latter used in this idiom. The problem with the analysis as an infinitive is that of the final vowel: T. vocalizes “*la ’ā*,” which he says is derived from “*la ’āwV*” (p. 618) or from “*la ’āyV*” (p. 666), thereby leaving aside his usual interest in triphthong contraction. On p. 485, examples are cited of III-’ infinitives used narratively and ending in /-u/; the one example claimed of the infinitive so used and ending with /i/ or Ø is better explained otherwise (see remark below to p. 485 [§73.514d]). Though none of the examples of the /-u/ ending is absolutely certain,<sup>893</sup> it nonetheless appears necessary to consider that this is the currently most likely hypothesis and hence to consider that the proto-form of the infinitive of L’Y would have been /la’āyu/. What reason is there to believe that G-infinitives behaved differently from other nouns as regards maintenance of the case vowel when a III-weak consonant elides? (The common position today, accepted by T., is that such nouns are triptotic, with a case vowel showing contraction, e.g., /šadû/šadâ/šadî/.) In his presentation of /qatāl/ nouns on p. 260 (§51.43a), T. cites no III-w/y forms; in his presentation of III-w/y infinitives on pp. 666-67 (§75.536a), he vocalizes all

<sup>890</sup>If this etymology be correct, Jewish Aramaic *m<sup>2</sup>zoztā’* would be a loan-word from Hebrew; note the absence of this word from Syriac.

<sup>891</sup>On pp. 468 and 666, T. identifies this root as denoting ‘strength’, on pp. 483, 617, 618, and 888 as denoting ‘weakness’.

<sup>892</sup>*Context I* (1997) 254 n. 107.

<sup>893</sup>On the basis of {yrû ... tt<sup>c</sup>} in RS 2.[009]<sup>+</sup> vi 30 (KTU 1.6), the anomalous writing of the first word in {yrâun ... tt<sup>c</sup> . nn} in RS 2.[022]<sup>+</sup> ii 6-7 (KTU 1.5) is probably best explained as a partially phonetic writing of /yarā’unnu/ ← /yarā’unhu/ or as a simple error, perhaps even for {yrû . nn}. T. prefers the explanation of this form as an infinitive (p. 38 [§21.323], p. 326 [§54.412], p. 481 [§73.513.2], p. 485 [§73.514c], p. 500 [§73.611.2g], p. 506 [§73.634d], p. 622 [§75.236], p. 638 [§75.516a]), but also entertains the analysis as a /QTLa/ form (p. 622 [§75.236]).

contracted forms with /-â/ but does not explain why that vowel should have predominated. In his presentation of triphthong contraction, only these infinitives are cited as examples of /āyV/ contraction (p. 197 [§33.322.41c]). On p. 482, the only place where the case of the phonology of III-w/y infinitives is discussed, he remarks only that the ending /-u/ may not have been used exclusively. I see two possible ways of cutting this version of the Gordian knot, both based on the assumption that *lā* is not the infinitive used narratively: (1) *lā* is 3 m.s. /QTLa/, used absolutely, and *šmm* is in fact an adverbial accusative (‘there is weakness in the heavens [which are] under the control of ’*Ilu*’s son Mot’); (2) *lā* is nominal and the entire phrase *lā šmm* is adverbial, modifying the previous phrase (‘Šapšu, luminary of the gods, glows hot / in the weak state of the heavens [which are] under the control of ’*Ilu*’s son Mot’). In the first case *lā* would be vocalized /la’a/ or /la’â/, depending on whether the 3 m.s. form showed contraction or not (see remark below to p. 655 [§75.531d], etc.), in the second /IV’â/, first vowel unknown because it would be unknown whether the noun in question is the /qatāl-/ infinitive or another de-verbal noun.

— p. 469 (§73.333.3), p. 647 (§75.524). The vocalization of {bštm}, ‘tarry, go slowly’ (RS 3.361 iii 18 [KTU 1.1] RS 2.[014]<sup>+</sup> iv 33 [KTU 1.3]), as “/bāštumâ/” must be criticized from two perspectives: (1) the unlikelihood that a long (contracted) vowel would have been preserved in a closed syllable (see above, seventh general remark), (2) T.’s own observation (p. 642 [§75.521c]) that not a single hollow-root form is attested that explicitly shows the absence of a vowel between the root and the pronominal element whereas several show the presence of such a vowel. The form may, therefore, more plausibly be vocalized /bāšātumā/.

— p. 469 (§73.333.4). I have cited above (remark to p. 214 [§41.21], etc., cf. on p. 467 [§73.332.5]) Huehnergard’s suggestion that the 1 c.pl. objective pronominal suffix may have been /-nū/. T. observes here, very correctly that the 1 c.du. pronominal suffix (subjective on a verb, genitive on nouns and particles [the objective form is not yet attested]) probably was not based on this form because the {y} is probably a consonantal glide (after /u/, one would expect the glide /w/). This observation requires one of two solutions: (1) either the 1 c.pl. suffix was /-nā/ or /nī/, forms compatible with the /y/-glide, or (2) the ‘person’ element of the 1 c.du. and the 1 c.pl. was not identical. Above, remark to p. 196 (§33.322.3b), etc., I have proposed that the latter explanation may be preferred.

— pp. 471-77 (§§73.4-73.427). Though there can be no doubt that all the ‘participles’ (active, passive, and stative) were in some sense in Ugaritic, as in Hebrew, verbal adjectives, one may doubt that the active participle functioned imperfectively, hence was a “Particip Präsens,” and the passive participle perfectively, hence was a “Particip Perfekt” (cf. remark below to pp. 682-718 [§76] with regard to similar remarks regarding the active participle and the infinitive). Judging from Biblical Hebrew, of which the verbal system was similar though not identical to the Ugaritic system, all three participles were unmarked for aspect and could be used to express complete or incomplete acts. T. appears to be confusing aspectual marking in the verbal system and the semantic field of the voices, viz., the fact of being the patient of a transitive act as expressed by the passive participle will more often than not denote an act that is complete, rather than one that is incomplete. If passivity in and of itself denoted completeness, there could be no imperfective of the finite passive verbal

stems. The very existence of /QTLa/ (perfective) and /YQTL/ (imperfective) passive verbal forms shows that passivity itself is not perfective.

— pp. 471-77 (§§73.4-73.427). In this treatment of “Verbaladjektive,” T. follows the Arabic system in proposing the existence of only active and passive verbal adjectives, rather than the Northwest-Semitic one, where one finds that stative verbs usually have neither an active nor a passive participle but only an adjective of which the stem is usually identical to that of the /QTLa/ (e.g., /kābēd/, which is either perfective or participial, but /kāb<sup>o</sup>dū/ perfective over against /k<sup>o</sup>bēdīm/ adjectival). T. admits the existence of these stative adjectives, but only as elements of the nominal system (pp. 258-259 [§§51.42c,d]) not as productive verbal adjectives. Given the many other similarities between the verbal system of Ugaritic and that of the other Northwest-Semitic languages, it must be considered certain that proto-West-Semitic had them and highly likely that the Ugaritic stative verbs had corresponding adjectival forms that were at least as productive, if not more so, as in the later Northwest-Semitic languages.<sup>894</sup> T.’s adoption of the Arabic paradigm as a pattern for Ugaritic will come particularly to the fore in his presentation of the G-participle of hollow roots, where no attempt is made to distinguish between active and stative verbal adjectives (see below, remarks to p. 642 [§75.521d], etc., and to p. 649 [§75.525a]), and in that of the geminate roots, where the failure to take the category into account appears to have been instrumental in allowing T. to categorize several /qall-/ forms as /QTLa/ when they may in fact have been stative adjectives (see various remarks below to pp. 672-76).

— p. 473 (§73.422), p. 474 (§73.424), pp. 474-75 (§73.425). Citing the Akkadian verbal adjectives /qatal/, /qatil/, and /qatul/ in support of the possibility that Ugaritic may have had more than one form that served as G-stem passive participles hardly appears apposite, for Ugaritic is fundamentally a West-Semitic language, not East Semitic. In the Northwest-Semitic languages, /qatal/, /qatil/, and /qatul/ serve as stative verbal adjectives while the explicit expression of passivity is reserved for /qatūl/ and /qatīl/ (see above, remark to p. 259 [§51.42c], etc.). The best argument for the existence in Ugaritic of the passive verbal adjective /qatūl-/ remains that provided by RS 15.098:11 (*KTU* 2.17:4), where {lūk} can, in spite of the difficulty of the following line, be nothing but a G-stem passive participle: {w ht . lūk ‘m ml[...] (12) pgsdb . šmlšn} most probably means ‘And now, PGSD B ŠMLŠN is hereby sent with (this) [my] mes[senger party].’<sup>895</sup> All the forms cited in favor of Ugaritic having a G-stem passive participle /qatīl-/ (p. 474 [§73.424]) may, even if correctly read and interpreted, be analyzed as /qatīl-/ adjectives. In Hebrew, where the /qatūl/ passive participle is productive, many /qatīl/ adjectives exist, most with a passive connotation. When writing a grammar, it is necessary to distinguish between accidental and productive forms. T. makes the correct qualification when he concludes that “Es folgt daraus nicht zwingend, daß {qatīl} im Ug. als gewöhnlicher MphT [Morphemtyp] des G-Ptz.pass. diene.” Immediately after this call for reason, however, T. falls into the all-too-familiar trap

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<sup>894</sup>Pardee, in *The Semitic Languages* (1997) 138; idem, *Encyclopedia of the World’s Ancient Languages* (2004) 307; Bordreuil and Pardee, *Manuel* (2004) I 71.

<sup>895</sup>The restoration of {ml[ākty]} appears far more likely than that of {ml[k]} assumed by T.’s translation “zum König(?).”

(pp. 474-75) of all-inclusivism by citing {lik} in RIH 83/07+:2 *et passim* (KTU 4.777) in favor of the /qatīl/ passive participle (or /qatīl/ as he indicates it here) without even considering other possibilities (see second following remark). It goes without saying that forms that do not include the information provided by one of the three { }-signs, i.e. {ḥlq} and {‘db} cited pp. 474-75, are useless for the purpose of determining whether the G-passive participle in Ugaritic was /qatūl/ or /qatīl/. In this respect, it appears equally uncertain to assign to Phoenician<sup>896</sup> the G-passive participle /qatīl/ when all the data are from proper names where the forms may represent frozen adjectival forms rather than the productive passive participle. In spite of his hesitation here, T. is apparently convinced that the Ugaritic form was /qatūl-/, for that is the only form indicated in reconstructions of the G-passive participle of weak roots, e.g., III-’ [p. 622, §75.235b], III-y [p. 666, §75.535b], or geminate [p. 676, §75.65b]—for the example of *lūk* in T.’s treatment of III-’ roots, one will not find it in a sub-section of participles, as in the three sections just cited, but on p. 619 under “abgeleitete Stämmen” [§75.228a]).

— p. 474 (§73.423), p. 827 (§89.231a), p. 863 (§93.33a). Contextual reasons have been provided for taking *ūzr* in RS 2.[004] i 2’ *et passim* (KTU 1.17) as a G-passive participle modifying the subject of the verb<sup>897</sup> rather than as the direct object of the verb (to T., the latter analysis appears more plausible than the former, though he provides no interpretation along those lines—see also remark below to p. 615 [§75.212.4]). The writing with {ū} in the first syllable would reflect vowel harmony with the following long vowel (/’azūr-/ → /’uzūr-/), for which good examples exist (see pp. 175-76 [§33.215.3]).

— p. 474 (§73.425), p. 617 (§75.224), p. 703 (§76.521.1). In the first section cited, T. asserts that the six-fold repeated *lik* in RIH 83/07+ (CAT 4.777) cannot be a 3 m.s. /QTLa/ form because the explicit subject is a town name which should be grammatically feminine; he goes on to interpret the form as a G-passive participle, explaining the singular form after a number phrase as owing to the absence of a “durchgehende Syntax” and translating “ON: soundsoviele (Personen): (bereits) geschickt.” On p. 617, he parses the form as 3 m.s. /QTLa/ and on p. 703 this example is included with a list of /QTLa/ forms, neither time with a cross-reference to his earlier refutation of that analysis. Cf. also p. 355 (§62.41b), where he translates l. 6 “(der Ort) *Rqd* hat 37 (Fronarbeiter) geschickt” and l. 9 “(der Ort) *M’rby* hat 42 (Fronarbeiter) geschickt,” and p. 356 (§62.431b), where he translates l. 2 “(der Ort) *Ubr’y* hat 87 (Fronarbeiter) geschickt,” but without an analysis of the verbal form.<sup>898</sup> Since there is explicit evidence for the G-passive participle being /qatūl/ and since the explicit subject of the verb in this text is each time a plural number referring to a number of men (which means that a participial form should show number agreement, viz., end in {-m}), the analysis as a passive participle must be considered out of the question. Even more astounding is the absence of even a mention of the following possibilities of analysis: {lik}

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<sup>896</sup>See also T., *UF* 31 (1999) 738.

<sup>897</sup>Pardee, *Context I* (1997) 343 n. 2.

<sup>898</sup>There is fairly general agreement today that *ḥrd* does not denote “Fronarbeiter” but persons in military service (cf. J.-P. Vita, *El Ejército de Ugarit* [Madrid: 1995] 136-44, 153, 180, 181, 182, 184; del Olmo Lete and Sanmartín, *Diccionario I* [1996] 197).

in this text may be (a) 3 m.s./pl. indefinite-subject active (‘one/they has/have sent’, i.e., /la’ika/ or /la’ikū/) or (b) 3 m.pl. G-passive /QTLa/ (/lu’ikū/ would be T.’s preferred vocalization of the latter form [p. 514, §74.223.1] and it is a mystery to me why that option is nowhere even mentioned).

— p. 474-75 (§73.425), p. 476 (§73.426), p. 552 (§74.412.27). One wonders how *hlq* in RS 19.018 ii 2 *et passim* (KTU 4.611) could be a G-passive participle when, as T. correctly observes on p. 552, the G-stem of this root is intransitive.

— p. 475 (§73.426), p. 668 (§75.537a). In the first section cited, devoted to the G-passive participle, the first word of *špy b hrš* in RIH 77/25:12’ (CAT 2.79:10) is cited as a point of comparison (“vgl. auch”); in the second, it is explicitly parsed as G-passive /QTLa/. In point of fact, the text is too broken for any one analysis to be certain. (The general meaning of ‘cover with gold’ is well established; what is unknown is the precise form of the verb in this passage.)

— p. 477 (§73.427). The identifications of *mtrḥt* in RS 2.[003]<sup>+</sup> i 13 (KTU 1.14) as a {m}-preformative noun or as a {m}-preformative G-stem passive participle are, respectively, implausible and highly unlikely. As T. shows in this section, the case for {m}-preformative G-stem passive participles is very weak. As regards the identification as a common noun, in support of which the Akkadian *marḥītu* is cited, the root TRḤ in Ugaritic had its own development independent of Akkadian *rēḥû*. Ugaritic *mtrḥt* is, therefore, to be parsed as a D-stem passive participle (see remark above to p. 269 [§51.45n]).

— p. 478 (§73.431c,d). T. recognizes here that the G-participle is often substantivized (i.e., does not function as a verbal adjective but as a noun) and lists examples, most from economic texts, where the category is often encountered, e.g., *nskm*, lit. ‘those who pour out, viz., pour out metal, hence, founders, metal-workers’. It might have been useful in the listings below of G-participles from the various root types to distinguish the adjectival forms from the nominal ones. For example, at III-weak roots, most forms cited function as adjectives, though there are exceptions, e.g. *āpym*, ‘bakers’ (the vocalization /’āpiyūma/ appears to be established by a syllabic spelling), or the divine titles *bny bnwt* (‘builder of offspring’, said of ’*Ilu*) and *qnyt ilm* (‘productress of gods’, said of ’*Aṭiratu*). On the other hand, two of the three examples of G-stem participles from geminate roots reflect substantivized adjectives (*gzzm*, ‘shearers’, and *dbbm*, ‘speakers [of evil words]’)—to the point that their categorization as /qātil-/ forms is in fact uncertain: *gzz* is apparently so classified for comparative reasons (cf. Hebrew *gōzēz*), though that is never stated anywhere, and T. himself recognizes that *dbb* may be /qattāl-/ rather than /qātil-/ (p. 676 [§75.65a]).

— p. 478 (§73.431d). Because of the paucity of stone tools found at Late-Bronze-Age Ugarit, it must be judged unlikely that the *psl ḥzm* were “Steinmetzen für Pfeilspitzen (aus Stein)” over against the *nsk ḥqm/ḥzm* who concerned themselves with casting metal arrowheads. It is more likely that this latter group did indeed cast the metal arrowheads while the former prepared the shaft and assembled the arrow.<sup>899</sup> That the primary meaning of PSL in Ugaritic was not ‘to carve stone’ is proven by the occupation title *psl qšt*, ‘bow-

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<sup>899</sup>Pardee, *Semitica* 49 (1999) 54-55.

maker’. While on the topic of founders, though the translation “Silbergiesser” for *nsk ksp* is certainly correct on the superficial level, the absence of a term for ‘pourer of gold’ means that in all likelihood the ‘silversmiths’ worked with both silver and gold, just as the *nsk tlt*, ‘pourer of copper’, worked with both copper and bronze.<sup>900</sup>

— p. 479 (§73.432.1a). T. correctly recognizes here the existence of the personal name *šnr*. I note that, where *šnr* exists, a by-form *šnrn* is plausible and T.’s proposal to emend the latter form to {s<sup>1</sup>nrn} in RS 11.858:8 (*KTU* 4.103)<sup>901</sup> is for that reason inherently dubious. This theoretical stance is confirmed by a second attestation of *šnrn* in RS 94.2290:9.

— p. 480 (§73.511). To the definition of the infinitive as a “verbal noun” with “a purely verbal character,” it should be added that the infinitive usually appears in a given form, with variations not only as per verbal stem but also as per root type (e.g., *k<sup>2</sup>tōb*, *šébet*, *b<sup>2</sup>nōt*, and *qūm* in Hebrew); non-paradigmatic forms (e.g., *’ah<sup>a</sup>bā<sup>h</sup>* in Hebrew) are comparatively rare. That T. realizes that the form of the infinitive is productive is clear from his presentation, where the infinitive of the G-stem is correctly identified as /qātāl/ while non-productive verbal nouns are treated as “andere Verbal substantive.”

— p. 480 (§73.512). In Biblical Hebrew, the so-called infinitive construct occurs not only in the construct and pronominal states, but also in the absolute state, as, for example, when followed by the definite direct object marker (phrases of the type *k<sup>2</sup>tōb ’et-hassēper*, ‘to write the document’).

— p. 482 (§73.513.5a), p. 487 (§73.523bβ), p. 667 (§75.536b) As an example of an uncontracted /qatāl-/ G-stem infinitive of a III-weak root, T. cites {’ly} in RS 92.2014:6 (RSO XIV 52), a text that was yet unpublished when this grammar was being prepared.<sup>902</sup> An equally certain example from this same root is to be found in RIH 78/14 (*CAT* 1.163), lines 2’ {b ‘l<sup>1</sup>[y]’h<sup>1</sup>}, 4’ {b ‘lyh}, and 6’ ({[b ‘l<sup>1</sup>]’lyh}, ‘in its rising (said of the moon)’.<sup>903</sup> On pp. 487 and 667, T. identifies this as a /qatl/, qitl/, or /qutl/ form, though without indicating his reasons. I have argued that, since the infinitive in the syntagmeme consisting of *b* + infinitive is attested as /qatāl-/ ({b šāl} in RS 2.[003]<sup>+</sup> i 38 [*KTU* 1.14]), such is the preferred vocalization of {’ly} in RIH 78/14:2, 4.<sup>904</sup>

— p. 483 (§73.513.5b), p. 667 (§75.536a). Because ‘*n* is followed by a pronoun expressing the object of the verb (*w ‘n hm*, ‘he answered them’) in RS 2.002:73 (*KTU* 1.23), rather than by the subject pronoun in the structure infinitive + independent pronoun that is attested several times in this section of the text, the analysis of ‘*n* as 3 m.s. /QTLa/ may be considered more likely than that as the infinitive, which is T.’s preference.

— pp. 483-84 (§73.513.6), p. 614 (§75.212.3), p. 711 (§76.524.44). As T. correctly observes on p. 484, *ark* in RS 2.002: 34 (*KTU* 1.23) “kann nicht S[uffix-]K[onjugation] sein

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<sup>900</sup>For a recent discussion with citation of a new text that explicitly identifies *tlt* as copper (RS 94.2519), see Pardee, *Syria* 77 (2000) 48-49 (“RS 94.2194” in note 104 is incorrect).

<sup>901</sup>Tropper, *AuOr* 16 (1998) 293.

<sup>902</sup>A preliminary publication of this text appeared in Pardee, *Les textes rituels* (2000) 829-33.

<sup>903</sup>A text first published by Bordreuil and Caquot, *Syria* 57 (1980) 352-53.

<sup>904</sup>Pardee, *Les textes rituels* (2000) 864 n. 19.

[*yd* = fem.].” But on p. 614, he explicitly parses the form as 3 m.s. /QTL<sub>a</sub>/. His analysis on pp. 483-84 as an infinitive used in replacement of a finite form is certainly morphologically possible, but taking it as a f.s. imperative (/’arakī/) addressed to ‘the hand’ provides a far better literary solution (see remark above to p. 426 [§73.121.2]), for it allows the speech of the two women whom *’Ilū* has just met on the shore of the sea to continue through line 35 after which *’Ilū*’s alternative is explicitly resolved.<sup>905</sup> If this latter analysis be correct, then it is not “offenbar” (p. 711) that an infinitive can follow a jussive in the command-response sequence, for this is the only example of that putative sequence cited by T.

— p. 485 (§73.514d), p. 622 (§75.236), p. 903 (§97.24). In these sections, T. claims or assumes the existence of one and only one example of an infinitive in ‘paronomastic’ usage that shows /i/ (or Ø) as the final vowel: *šm’ t ḥtī nḥtū* (RS 4.475:7-8 [KTU 2.10]). He fails to mention that, because of this morpho-syntactic anomaly, it has been proposed that *ḥtī* is in fact a common noun meaning ‘blow’, in the plural and in construct with the following verbal phrase.<sup>906</sup> The sentence may be translated somewhat literally: ‘I have heard of the blows with which they have been smitten’,<sup>907</sup> less literally, ‘... that they have suffered defeat.’<sup>908</sup> (On the verb *nḥtū*, see below, comment to p. 533 [§74.32], etc.)

— pp. 485-90 (§73.52). It is a bit of an exaggeration to say that Ugaritic shows alongside /qātal-/ “eine Reihe von anders gebildeten Verbalsubstantiven zum Grundstamm” (p. 485). The only other stem that is clearly attested, and that by a very small number of entries in the quadrilingual vocabularies, is /qitl/. The dividing line between ‘infinitive’ and ‘verbal noun’ is a fine one; but the ‘infinitive’ may be defined as the productive verbal noun for a given verbal stem, with productive variations according to root type, while ‘verbal noun’ may have two uses, a broad one (‘all nouns expressing abstractly the action of a corresponding verb’, viz., infinitives and other verbal nouns) and a narrow (‘non-productive nouns some of which may function for certain roots as the infinitive’, e.g., *’ah<sup>a</sup>bā<sup>h</sup>* in Biblical Hebrew). It is quite clear that the Ugaritic system is different from the Hebrew and Aramaic ones, though perhaps closer to the latter than to the former (in Hebrew the G-stem /qatāl/ form has lost infinitival value, to be replaced by /qutul/ in the strong root; in Aramaic all the derived-stem infinitives show /ā/ in the second syllable, though this is not the case of the G-stem, where the most prevalent pattern is /miqtal/). In this respect, Ugaritic is closer to Akkadian, where the productive verbal noun in the G-stem is /qatāl/, though it is impossible to determine whether Ugaritic may have had a larger number of verbal nouns on the model best known from Arabic. Of the many G-stem verbal nouns cited in this section, there are usually no orthographic criteria by which to determine whether a given form is /qatāl-/ , /qitl-/ , or yet another. For example, in the case of {hg} in RS 2.[003]<sup>+</sup> ii 91 (KTU 1.14 ii 38), the form is indicated as /higī/ ← “/higw/yi/” (p. 486, §73.523b<sub>a</sub>), though Arabic /hağw-/, Tigre /higyā/,

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<sup>905</sup> Idem, *Context I* (1997) 280.

<sup>906</sup> Pardee, *Pope* (1987) 67.

<sup>907</sup> Cf. *ibid.*, p. 66.

<sup>908</sup> *Context III* (2002) 108; French version in Bordreuil and Pardee, *Manuel* (2004) II 81: “... ils ont subi des échecs.”

and Medieval Hebrew /h<sup>a</sup>gāyā<sup>h</sup>/ are all cited as cognates. The writing without {y} shows a final biconsonantal form, but it does not determine which original triconsonantal form has contracted.

— p. 485 (§73.521). Because the use of the paronomastic infinitive is rare in prose, one may doubt that the first token of {šr} in the phrase *w šr yšr šr* in RS 24.250:15-16 (*KTU* 1.106) is a verbal noun in the narrow sense of the word. This phrase is more plausibly interpreted as meaning ‘and as for the singer, he shall sing a song’ (/wa šāru yašīru šīra/).<sup>909</sup>

— pp. 486-88 (§73.523). This long paragraph is devoted to putative examples of /qatl/qitl/qutl/ verbal nouns. It is prefaced with the statement that some or all of such forms may in fact be /qVtVI/ (p. 486). In other words, anything is possible. It is here that the question of whether the /qatāl/ base was or was not the productive G-stem verbal noun must be considered, for many of the forms listed here occur in a most characteristic Northwest-Semitic syntagmeme, that wherein the standard infinitive is preceded by a preposition to form a circumstantial clause, temporal or other, e.g., *b dm<sup>c</sup> n<sup>c</sup>mn ġlm il*, lit. ‘in the tear-shedding of the goodly lad of <sup>ʾ</sup>*Ilū*’ (RS 2.[003]<sup>+</sup> ii 8-9 [*KTU* 1.14]). Because one of these is indubitably /qatāl/ (*b šāl* /bi ša<sup>ʾ</sup>āli/, *ibid.* i 38—see above, remark to p. 482 [§73.513.5a], etc.), I see no reason to doubt that this was the form most commonly used. That other verbal substantives existed is certain, for one finds other verbal citation forms in the polyglot vocabularies (particularly /qitl/), two verbal nouns of II-<sup>ʾ</sup> roots spelled with {i} rather than {ā} (*šib* and *šil*), and various forms from weak roots. This situation is not, however, all that different from what one encounters in the other Northwest-Semitic languages, where a productive G-stem infinitive exists for strong roots (e.g., /\*qutul/ in Hebrew or /miqtal/ in many dialects of Aramaic) alongside other verbal substantives, which are more or less productive for the various root types (e.g., *benōt*, *rédet*, etc., in Hebrew, with appropriate variations of /qutul/ for certain weak roots, in particular *qūm* and *subb*-). Until contrary data appear, it is safest to assume a similar situation for Ugaritic. Because actual data prove that, for the verb Š<sup>ʾ</sup>L, /ša<sup>ʾ</sup>ālu/ existed alongside /ša<sup>ʾ</sup>lu/šī<sup>ʾ</sup>lu/šū<sup>ʾ</sup>lu/ and that the former was used in the *b* + infinitive syntagmeme,<sup>910</sup> it appears necessary to conclude—with present data—that such was the case for most if not all triconsonantal roots and that all such structures should be vocalized on the pattern /bi qatāli/. (On the specific case of III-y roots, see following remark.) Beyond that, however, nothing can be said with any confidence. A plausible distribution would be /qatālu/ forms in the paronomastic syntagmeme (a finite form modified adverbially by an infinitival form as in *lākm ilāk* /la<sup>ʾ</sup>ākuma ʾil<sup>ʾ</sup>aku/, ‘I will surely send’, RS 16.379:19-20 [*KTU* 2.30]) and in the preposition + infinitive syntagmeme just discussed,<sup>911</sup> but another verbal noun when the structure is accusative, e.g. *hlk aḥth b<sup>c</sup>l y<sup>c</sup>n*, ‘Ba<sup>c</sup>lu saw the going of his sister’ (RS 2.[014]<sup>+</sup> iv 39 [*KTU*

<sup>909</sup>Pardee, *Les textes rituels* (2000) 590, 591, 595-96; cf. *idem*, *Ritual and Cult* (2002) 55.

<sup>910</sup>One may doubt on this basis that the sequence {...} . t<sup>c</sup>rb . b šī in RS 3.361 v 26 (*KTU* 1.1) is to be reconstructed with {l} at the beginning of the next line, as T. proposes on p. 486 (§73.523a).

<sup>911</sup>That this usage was not universal is shown by the formula *tr b lkt* in RS 3.362<sup>+</sup> ii 28<sup>c</sup>, 29<sup>c</sup> (*KTU* 1.10), but the import of the idiom is clouded by the fact that HLK is ambivalent, appearing both as HLK and as though from Y/WLK (see remark below to p. 625 [§75.332]).

1.3] and RS 2.[008]<sup>+</sup> ii 13'-14' [KTU 1.4]). In this particular idiom, *hlk* is in parallel with a {t}-preformative verbal noun (*tdrq*) and one may suspect that *hlk* also is in a form other than /qatālu/. But whether that is in fact the case and, if so, whether the example may be extrapolated into a rule are questions awaiting new data for resolution.

— pp. 486-87 (§§73.523b $\alpha$ - $\beta$ ), pp. 666-67 (§§75.536a-b). Because T. himself recognizes the existence of /qatāl/ infinitives alongside /qitl/ verbal nouns, one is at a loss to understand why he classes the verbal nouns from III-y roots in §73.523b $\beta$  as /qatl/qitl/qutl/ forms when these are written with {-y}, in contrast with a group gathered in §73.523b $\alpha$  which are written without the {-y} and which are also said to belong to this general morphological category. (I can detect no principle governing the break-down in §75.523a and b according to /qatāl/ and other types—see further remark below to this section.) Because most of the examples in §73.523b $\beta$  consist of the syntagmeme consisting of preposition + infinitive, for which the example of {b šāl} proves that the infinitive may be of the /qatāl/ form (see preceding remark), one can only conclude that the more likely vocalization is the latter. T.'s classification of these forms is all the more puzzling given that he includes here examples of {‘ly}, from another text though orthographically identical to the form cited in §73.513.5a (p. 482), to illustrate the uncontracted /qatāl/ form. More important than speculating whether the forms cited in §73.523b $\beta$  are or are not /qatl/qitl/qutl/ (a classification that appears to border on the purely arbitrary) would be showing why those cited in §73.523b $\alpha$  may not be historical /qatāl/ forms that have undergone contraction.

— p. 488 (§73.523c). A specific objection must be lodged with regard to T.'s treatment of one of the verbal-noun constructions. He observes that {nšī} in the phrase *b nšī* 'nh, lit. 'in his/her lifting of his/her eyes', is "wohl /niš'i/." He goes on to claim that the Ugaritic form "entspricht dem he. Inf.cs. n<sup>o</sup>śo'." Though this is syntactically true, it is certainly not morphologically so: the Hebrew form corresponds to the standard G-stem infinitive of strong roots, viz., it is derived from a /qutul/ base (cf. alongside /n<sup>o</sup>śō'/ a suffixal form such as /noś'i/). So, if the comparison is to be made, why not identify the Ugaritic form as a /qutul/ or /qutl/ base rather than a /qitl/? But Ugaritic morpho-syntax not being identical to Hebrew morpho-syntax, why attempt to identify the Ugaritic and Hebrew *forms* at all? Why not simply say that the Ugaritic form may well have been the standard Ugaritic infinitive (i.e., /našā'i/), though another verbal noun cannot be ruled out?

— p. 488 (§73.524a), p. 493 (§73.532), p. 667 (§75.536b). T. consistently analyses *s't* in RS 2.[003]<sup>+</sup> iii 7, iv 51, and v 1 (KTU 1.14) as a verbal substantive. If so, this form would provide the only example of a verbal noun of a type other than /qatāl/ functioning narratively. Moreover, in spite of textual problems in two of these three passages, it appears extremely likely that the subject is each time feminine.<sup>912</sup> It appears necessary to conclude that the analysis as a participle, whether plural or singular (in which case the subject would each time be singular collective), is to be preferred. T. apparently shies away from this analysis because the feminine plural participle of III-y roots is attested with the /y/ retained in the orthography. Because, however, no contraction rule is without exception or

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<sup>912</sup>Pardee, *Context I* (1997) 335 n. 26.

because the form is singular, well attested without {y} (e.g., {bkt} ‘she who weeps’), this reticence appears misplaced in this case.

— p. 489 (§73.525). One must ask if T. has not allowed himself to be a bit too much influenced by Hebrew morpho-syntax in classifying the nominal type /til(a)t-/ as a G-stem verbal noun. In only two of the passages cited are such forms used with a preposition to form a temporal/circumstantial clause; in one, the preposition *k* introduces a comparison. In the others, the function of the form in question is that of a common noun. For example, T.’s translation of *trḥšnn b d‘t* (RS 03.325+ vi 10 [KTU 1.16]) as “Sie wusch ihn rein vom Schweiß” hardly bears out his classification of *d‘t*. If the function were verbal, might one not expect the noun to bear a pronominal suffix to mark the subject (‘She washes him as he sweats’)? Another example: *šnt* in the phrase *šnt tlūān* (RS 02.[003]+ i 33 [KTU 1.14]), appears to function as a common noun and is so translated by T. (‘Schlaf überwältigte ihn’). Even in Hebrew, where this nominal type functions as the standard G-stem infinitive for I-y roots, there is slippage between the functions of a verbal or a common noun, so that words such as *šébet* and *dá‘at* require two dictionary entries, one under the verb (‘to sit, sitting’, ‘to know, knowing’), the other as a common noun (‘seat, dwelling’, ‘knowledge’). Viewed from another perspective, these forms provide a clear backdrop for the Hebrew system, where the form has become far more specialized in function. Moreover, this basic similarity must be considered as an isogloss linking Ugaritic and Hebrew, even though the Ugaritic usage of the forms in question appears to be far less paradigmatic.

— p. 489 (§73.526), p. 637 (§75.514), p. 703 (§76.521.1). In the first section cited, T. translates the particle *l* in RIH 83/22:4 (CAT 4.779) as an emphatic particle, in agreement with the preliminary edition,<sup>913</sup> but with a question mark; he also suggests emending the *l* to *d*, viz., the relative pronoun. On p. 703, he translates the *l* as the negative particle, with no question mark and with no cross-reference. The passage is not cited below in either of the two sections in which the two particles are treated. In context, it appears necessary to prefer the editor’s interpretation: those who paid the total sum of 93.5 shekels paid it partially in silver, partially in cloth/garments (25 shekels’ worth of *iqnū*, 40 shekels’ worth of *ktn*).<sup>914</sup>

— p. 494 (§73.534a). Because of the space available, one understands T.’s desire to come up with a longer restoration at the beginning of RS 2.[004] i 40' (KTU 1.17) than {w . hr}, often adopted on the basis of RS 2.002:51 (KTU 1.23) where this reading is extant in a similar context. But T.’s proposal to restore {[w hrhr(t)]?} not only supposes a form that is previously unattested but one that is not particularly likely to have existed, for the root is HRY, ‘to become pregnant’ and reduplication of the first two consonants of triconsonantal roots is not a common pattern in Semitic (T. cites no such forms in the relevant section devoted to nominal patterns). Either {hrt}, which is attested as a deverbal noun from this

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<sup>913</sup>Bordreuil, CRAI 1984, p. 433: “Ils ont réellement donné leur argent” (*l ytn ksphm*).

<sup>914</sup>Bordreuil, *ibid.*, took *iqnū* as referring to lapis-lazuli, *ktn* to the stone designated in Akkadian as *katinnu*. Since *ktn* commonly designates a garment in economic texts and *iqnū* commonly designates purple-dyed wool in the same body of texts, an interpretation along the latter lines appears more plausible (see Bordreuil and Pardee, *Manuel* [2004], text 52 in the *Choix de textes*).

root, or {hry}, unattested for this root but well known for other III-y roots, is more plausible as a restoration here.<sup>915</sup>

— pp. 497-506 (§73.6), p. 730 (§77.411). Several points may be made with regard to the so-called ‘energetic’ endings:

(1) If one may judge from Arabic, the two Ugaritic /YQTL/ forms termed ‘energetic’ that showed the forms /-an/ and /-anna/ contain distinct morphemes, not allomorphs.

(2) As far as I can tell, T.’s classification of any given absolute form (i.e., one that contains only the energetic morpheme, not that morpheme followed by a pronominal suffix) written with a single {-n} as Energetic I (/anna/) or Energetic II (his Energetic III, viz. /-an/) is purely arbitrary. He, in any case, provides no criteria for independent classification of these forms in Ugaritic, where the purely consonantal script does not allow the reader to perceive immediately whether the morpheme is /-(a)nna/ or /-(a)n/. The conclusion that he reaches with regard to Energetic I, viz., “Zusammenfassend ist festzuhalten, daß der Energ. I in absoluter Position in der Poesie sehr produktiv ist” (p. 500) rests, as far as I can tell, on no further foundation than his subjective reconstructions. Going along with T.’s hypothesis regarding the nature of the energetic endings (see below, remark 7) for the moment, a most basic hypothesis would seem to say that /-(a)nna/ would have been used after verbal forms ending in /-Ø/ or a short vowel whereas /-na/ would have appeared after verbal forms ending in /-Ø/ or a long vowel. Only thus may be avoided the problem, to which allusion is made repeatedly below (see here remark 4), of attaching /-nna/ to a form of which the grammatical number is expressed by vowel length and of the resultant shortening of that vowel when the syllable in which it occurs is closed by /-nna/ (e.g., /tQTLū-nna/ would have become /tQTLunna/ and the marking of plurality would thereby have been lost).

(3) As he considers the forms to be allomorphs, there can be for him no semantic distinction. Herein lies one of the basic problems of these forms: what was their function within the verbal system? If a definable function were discernible, one could begin to discuss a distinction between the two. If that function is purely one of ‘emphasis’, i.e., the only function of the additional morpheme is to add weight to the verbal expression by extending it,<sup>916</sup> then the two forms of the extension certainly have morphemic value because one is longer, hence more emphatic, than the other.

(4) A further comment is required in this matter of identifying {-n} with one or the other of the two possible morphemes: T.’s ambivalence regarding the shortening of long vowels in syllables that have become secondarily closed must be mentioned here again (cf. above, eighth general remark). If {tšūn}, for example, a 3 m. pl. form, does indeed contain the Energetic I morpheme (the one with geminated /n/), then it can only be vocalized /tiššaʾunna/

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<sup>915</sup>The space available at the beginning of this line indicates that the total restoration should count about five signs (that is the number of signs that occupy the same space in the preceding line), and the restoration of only three signs and a word-divider by Bordreuil and Pardee, *Manuel* (2004) II 24 (i.e., the restoration {w hr .}) is thus probably too short—in the following line we restored four signs and a word-divider in about the same space.

<sup>916</sup>T. describes this as a true modal function that may be defined as “eine besondere Betonung der zugrundeliegenden Verbalform” (p. 730), but he does not distinguish between the forms of emphasis expressed by each of the forms of which he posits the existence.

← /*\*tišsaʾūnna*/, which makes it formally identical with singular indicative forms plus Energic I, i.e., /*tQTLu + nna*/ (2 m.s. and 3 f.s.).

(5) If a pronominal suffix is attached to the verb in the form {-nh}, one can be sure that the energic element ended in a vowel (T., p. 501).

(6) T.'s posited third energic form (his Energic II) may not be considered a likely form (or allomorph): he takes the morph that is written {-nn} to be related to the Akkadian ventive morpheme *-nim*, but there is no reason to consider the hypothesis of a /n/ - /m/ interchange to be plausible (see above, remark to pp. 222-23 [§41.221.52c], etc.). Because the Ugaritic consonantal orthography records only the consonantal skeleton, one must fall back on comparative considerations in order to reconstruct a plausible set of forms. Given that both Arabic (/an/ and -anna/) and Hebrew (/yiqṭlennū/ [← YQTLanhu] and /yiqṭlenhū/ [← /YQTLannahu/]) show two forms only, it may be considered likely that Ugaritic also had only two basic forms. The form written {-nn} may more plausibly be considered, therefore, to correspond, not to a third energic morpheme *per se*, but to a combination of energic and suffixal morphemes.<sup>917</sup> T. recognizes the necessity of dealing with this combination of morphemes, but prefers to posit the existence of a third energic over envisaging possibilities of recombination of well-attested West-Semitic morphemes. In this respect, his remark that “Der Sequenz /-nVn-/ des ug. Energ. II folgt mit Sicherheit kein Vokal” holds true, of course, only if one accepts the existence of the morpheme itself. If one considers, as do I, that the second {n} of the {-nn} graphemic unit itself consists historically of an energic morpheme and a pronominal suffix (i.e., /-n + hV/ → /-nnV/, itself reanalyzed as a pronominal suffix and attached to the Energic I morpheme, i.e., /-nna + nnV/), T.'s view of the phonetic make-up of {-nn} has no relevance. The strongest argument based on the Ugaritic data against the existence of this third energic form is that it is unattested independently, viz., only {-n} is attested as the energic ending without associated pronominal suffix in Ugaritic. In order to argue for the existence of a morpheme in a language, it is good to have at least one token of the morpheme. The fact that {-nn} occurs only as a pronominal morpheme must be considered an argument, along with the others cited above, against the origin of that morpheme in a third energic morpheme unattested elsewhere as such in Northwest Semitic.<sup>918</sup>

(7) Two verbal forms are clearly attested with three tokens of {n} affixed to the verb RS 1.026+:11 {tšknenn}, ‘(someone) shall establish it/him/her’, and RS 15.174:17 {ttnn ṯ.ṯ nn}, ‘(someone) shall give it/him/her’. One interpretation of these forms is as energics + a suffix {-nn},<sup>919</sup> rather than as plural indicatives (/tQTL+ ūna/ + {nn}), as T. takes them (see above, note 483). If such be the correct interpretation, then it is highly unlikely that one energic would have been attached to another.

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<sup>917</sup>Pardee, *JNES* 43 (1984) 244-45 n. 14.

<sup>918</sup>This stance should not be taken as denying any historical relationship between the Akkadian ventive and the West-Semitic energics, but as a caution against seeing one of the Akkadian morphemes surviving partially intact in Ugaritic alongside two others that show very close affinities with the energic morphemes in Hebrew and Arabic.

<sup>919</sup>Pardee, *JNES* 43 (1984) 244-45 n. 14.

(8) Most fundamentally, T. takes the energetic endings not as constituting modal endings on a par with /-Ø/, /-u/, and /-a/, but as separable morphemes that may be added to either of the first two forms.<sup>920</sup> This is a pure reconstruction in the sense that there are no explicit Ugaritic data to support it (of /YQTL/ forms from III-ʾ roots that do not themselves end in a vowel, no form spelled {yQTûn}, i.e., /yiqtaʾun(na)/, is attested to date—granted, there are very few forms attested<sup>921</sup>; nor are there any spelled {yQTîṇ}, i.e., /yiqtaʾn(a)/<sup>922</sup>). Moreover, it goes against the Arabic evidence, where /YQTLanna/ and /YQTLan/ function as distinct moods (cf. p. 730 [§77.411]<sup>923</sup>). On the other hand, the data from fourteenth-century Canaanite appear clearly to favor T.’s hypothesis (cf. pp. 501, 730).<sup>924</sup> Closer to home, also in favor of T.’s hypothesis is the fact that verbal forms ending in {-n} appear in Ugaritic poetry in all the morpho-syntactic slots of the /YQTL/ forms without affirmative {-n(n)}, i.e., as perfectives, imperfectives, and all the volitive forms. Because there is yet no certain example of a spelling {yQTûn}, the frequent use of enclitic {-n} in Ugaritic means that the Ugaritic verbal system could be identical to the Arabic one, with forms that do not correspond to this system explained by the enclitic {-n}.<sup>925</sup> The corresponding Hebrew forms favor T.’s hypothesis that /-an(na)/ could be attached to /YQTLØ/ forms, i.e. there is no difference between perfective and imperfective forms ending with {-n}; the morpheme is, however, attested only before pronominal suffixes (/yiqṭʾlennû/ = ‘he will kill him’, /wayyiqṭʾlennû/ = ‘he killed him’) and these are, therefore, frozen forms that may only with great circumspection be exploited for determining the proto-Hebrew distribution of the energetic forms. If T.’s reconstruction is correct, the energetic morphemes probably arose in early West Semitic from the use there of enclitic /-n/; the longer of the two would simply represent the West-Semitic proclivity for particle accretion, in this case the accretion of particles with an identical consonant (/n(V)/ + /nV/); cf. p. 502, where T. hypothesizes that /-nVnV/ would have become /-nnV/. This raises the question, however, of the distinction between the energetic morpheme and the /-na/ morpheme attached

<sup>920</sup>On T.’s view of /YQTLa/ forms, see above, remark to p. 429 (§73.143), etc.

<sup>921</sup>In T.’s listing on pp. 620-21, there is only one, a 1 c.s. form that would correspond to a ‘cohortative’ in Hebrew: {iqrân} /ʾiqraʾan(n)a/. To this form is perhaps to be added {yṣân} in RIH 77/2+:3ʾ as a 3<sup>d</sup> person /YQTLa/ form, though the {â} here may represent the dual morpheme (see remark below to p. 500 [§73.611.2h], etc.).

<sup>922</sup>On p. 500 (§73.611.2h), T. cites *KTU* 2.54:2 for the form {yṣîn}, but the reading is uncertain and the interpretation even more so (see remark to p. 500 [§73.611.2h], etc.).

<sup>923</sup>Here T. describes the situation in Arabic as permitting the energetic endings only to be attached, formally, to the /YQTLØ/ form, i.e., the /a/ vowel is that of the energetic ending itself, not that of the /YQTLa/ form.

<sup>924</sup>See also my comparison of the Canaanite system with the Ugaritic one, *JNES* 58 (1999) 314-16. In an otherwise scathing review of A. Rainey’s study of *Canaanite in the Amarna Tablets: A Linguistic Analysis of the Mixed Dialect Used by the Scribes from Canaan* (Handbuch der Orientalistik. Erste Abteilung: Der Nahe und Mittlere Osten 25/1-4; Leiden: Brill, 1996), E. Von Dassow accepts both Rainey’s analysis of the cuneiform data as representing Canaanite morphology and T.’s extrapolation of these conclusions to Ugaritic (*EI* 53 [2003] 196-217, esp. pp. 213-15).

<sup>925</sup>Thus T.’s assertion that /YQTLunna/ is “häufig belegt” alongside /YQTLanna/ (*UF* 33 [2001] 729) is based on his reconstruction of the system, not on any explicit data from Ugaritic.

to /YQTL/ forms ending in a long vowel to express the indicative over against the jussive/perfective (i.e., /YQTLūna/ vs. /YQTLū/). T. regularly proposes that /-nna/ would have been attached to such jussive/perfective forms but does not explicitly take into consideration the resultant shortening of the vowel by which plurality is expressed (/tQTLū+nna/ → /tQTLunna/, which would thereby have become identical to a singular form beginning with *t-*). On the other hand, if one posits an energetic morpheme /-na/, the plural indicative form and the jussive form followed by this energetic morpheme would have been identical (/tQTLūna/ = /tQTLū + na/).<sup>926</sup> All in all, the one factor that leads me to believe that T.'s basic hypothesis deserves some consideration is the fact mentioned above that it appears—in spite of the absence of writings in Ugaritic that explicitly confirm it—to explain best the existence of perfective, imperfective, and volitive forms all bearing the {-n}. The alternative is to say that, in the verbal system visible in Ugaritic poetry, the only energetic forms in use were, as in Arabic and perhaps in proto-Hebrew,<sup>927</sup> /YQTLan/ and /YQTLanna/ and that these no longer had the modal specificity of the /YQTLa/ form, having taken on the status of an independent mood.<sup>928</sup> T.'s failure to deal with the problems posed by the shortening of a vowel when followed by the /-nna/ ending renders, however, the present state of his hypothesis unacceptable.

(9) All the comparative evidence goes against identifying the {-n} attached to /QTLa/ forms and to the infinitive used narratively with the productive 'energetic' morpheme of the /YQTL/ and the imperative, and this aspect of T.'s presentation is thus problematic. One example is cited of each on p. 500 (§73.611.2f,g); these same examples with an additional one are cited also under Energetic III, p. 506 (§73.634c,d). One of these is problematic, that is, the {-n} may be simply the enclitic morpheme (see remark below on *hlmn* in RS 3.340 iv 33' [KTU 1.18]). Examples of the ending written {-nn} and attached to these same forms are provided on p. 223 (§41.221.52c), p. 224 (§41.221.62b), and pp. 503-4 [§§73.626, 627]), but none is fully convincing (see remark above to p. 223 [§41.221.52c], etc.). If such forms exist at all, it appears considerably more plausible to see in them examples of the combination of an energetic morpheme and a pronominal suffix on /YQTL/ and imperative forms that have been re-analyzed as pronominal suffixes and become attachable to QTL forms. The best analysis of most of these forms is as the infinitive used narratively, for in the dialect of Phoenician visible in the Karatepe inscription, the infinitive (absolute) can take pronominal suffixes.

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<sup>926</sup>T. rarely alludes to this possible source of homophony (e.g., p. 663 [§75.532] on *tšty*).

<sup>927</sup>The problem in Hebrew is neither with forms of the type /yiqṭlennū/ (← /YQTL + an + hu/), nor with the rare forms of the type /yiqṭlennū/ (← /YQTL + anna + hu/), but with those of the type /YQTLēhū/, which should, all other things being equal, derive from a proto-Hebrew base /YQTL + i + hu/ (cf. Aramaic /yiqṭlennēh/).

<sup>928</sup>T. only allows for the loss of modal specificity of energetic forms when these were fused with a pronominal suffix, and then only as a possibility: "Es ist somit denkbar, daß die betreffenden Formen — zumindest bisweilen — als modal-neutrale Varianten zu energikusslosen Verbalformen mit Objektsuffixen gebraucht werden" (p. 730 [§77.411]).

(10) T. posits that the energetic morphemes began with an /a/ vowel, for only thus can he explain the absence of assimilation of the /n/ after forms that he analyses as /YQTLØ/. But, if his basic idea is correct, is it not more plausible to posit that the morpheme was not vowel-initial, since enclitic {-n} is usually not posited to be such? This would account better for the proto-West-Semitic system, where there is no evidence that the enclitic particle began with a vowel and where it was attachable to any word in the language, *a fortiori* to any of the three principal prefix-conjugation forms, /YQTLØ/, /YQTLu/, and /YQTLa/. The Arabic and Hebrew forms would then have arisen, not from /YQTL/ + /an(na)/ but from /YQTLa/ + /n(na)/. (They would, of course, be frozen forms and their semantics would not necessarily correspond to the semantics of the historic /YQTLa/.) Such an hypothesis might contain the germ of an explanation of the alternate Hebrew forms, that is /yiqṭlēhu/ and /wayyiqṭlēhu/, forms with a vowel between the verbal stem and the pronominal suffix that should, all other things being equal, have come from /i/ (an /i/-class vowel is also characteristic of the Aramaic paradigm, there before /-nn-/). I have never had an explanation for these forms since there is no /YQTLi/ form posited for early West Semitic. But if the {-n} of the energetic forms was not historically part of the verbal morpheme, one could posit that proto-Hebrew somehow developed an energetic form with a secondary /i/, e.g., if there was a by-form /ni/,<sup>929</sup> one might call on vowel harmony to account for the otherwise unexplainable vowel (/YQTLuni/ → /YQTLini/ → /YQTLi + suffix/). Whatever this hypothesis says about proto-Hebrew, it certainly says that volitive forms followed by the energetic ending were historically /YQTLa/, not /YQTLØ/, because of the absence of assimilation of the /n/.

(11) In summary, the simplest solution to the problem of the energetics in West Semitic would be to posit that they began as enclitic-*n* attached to one of the primary /YQTL/ forms; these extended forms would have become grammaticalized in various ways in the West-Semitic languages. If such be the case, the origin of the form with geminated /n/ would have to be identified as secondary (e.g., /-Vna/ → /-Vn/ to which /-na/ has again been attached). One of the common features of grammaticalization would have been that the energetic morphemes came to be limited to /-n/ and /-nna/ so as to reduce confusion with the /-na/ by which jussive forms were distinguished from indicative plural forms (jussive /YQTLū/, indicative /YQTLūna/), for that morpheme may well have had an identical origin. For the present, the Ugaritic data favor the hypothesis that the system in that language developed along the lines best known from Arabic and, to a lesser extent, from Hebrew. A strong argument along those lines is the absence of an accusative 3 m./f. pronominal suffix {-h} attached to the imperfective forms in Ugaritic: because the only attested form of these suffixes is that in which the suffix has assimilated to what was historically the energetic morpheme, a situation more like the Aramaic one than the Hebrew one, it appears plausible to conclude that the energetic part of these morphemes no longer had an independent function and that the distribution of the energetic forms elsewhere may have been reduced along the lines of Arabic. On the other hand, one may argue from {tlûn} in (RS 02.[003]<sup>+</sup> i 33 [KTU 1.14]) that the pronominal suffix form, at least, could be attached to the /YQTLu/ form (see

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<sup>929</sup>Cf. {al-li-ni-ya}, the syllabic spelling of the demonstrative particle which provides the vocalization /halliniya/; here the /i/ appears to be conditioned by the following /y/.

above remark to p. 427 [§73.132]). Nevertheless, the internal data are very few and the picture could change at any time to reveal a situation closer to that apparently revealed by the Amarna texts. With that in mind, three conclusions must be drawn with regard to T.'s presentation: (a) his hypothesis regarding the nature of the Ugaritic energetic system must be held in abeyance until data appear to confirm or disprove the retention of the assumed archaic system in Ugaritic, (b) his hypothesis regarding the existence of a third energetic form /-(v)nVn/ must be considered dubious, (c) one must admit that, unless affirmative {-n} has incorporated a pronominal suffix, it is difficult to distinguish between an energetic verbal form and a non-energetic form with the enclitic morpheme *-na* that was still productive in Ugaritic.

— p. 498 (§73.611.1b), p. 633 (§75.512). In the first section cited, the /YQTL/ form of YBL is cited as /yâbVl-/ while in the second the theme vowel of this verb is indicated as being /i/. The comparative data speak in favor of this hypothetical reconstruction, e.g., Hebrew /yēbēl/, Akkadian /ibīl/.<sup>930</sup>

— pp. 499-500 (§73.611.2). T. suggests that the final vowel of the Energetic I /-(a)nna/ may be dissimilated to /i/ when following the dual ending, i.e., he vocalizes {tm̄gyn} “/tam̄giyâ-nni/a/.” But he does not do the same when the form ends with what he reconstructs as /â/, e.g., {yphn} is vocalized “/yiphâ-*nn*a/.” Is that logical? He cannot, of course, consider that the same vowel might dissimilate following /a/, as I would reconstruct forms of this type,<sup>931</sup> for the morpheme itself, according to T., consists of /-anna/.

— p. 500 (§73.611.2c), p. 732 (§77.412.3c). In the first section cited, T. analyses both occurrences of *tm̄trn* in RS 2.[009]<sup>+</sup> iii, lines 6 and 12 (*KTU* 1.6), as /YQTLØ/ “indicative,” whereas in the second, the first token is said to be volitive, only the second indicative. This latter distinction is preferable, but the point is, of course, moot if the energetic forms constitute independent moods, rather than expansions of the other moods (see remarks to pp. 497-506 [§73.6], etc.).

— p. 500 (§73.611.2c), p. 551 (§74.412.26), p. 690 (§76.342). In the first section cited, T. identifies *tr̄hpn*, translated “flatterten,” in RS 3.340 iv 20', 31' (*KTU* 1.18) and RS 3.322<sup>+</sup> i 32 (*KTU* 1.19) as /YQTLØ/ + energetic and indicates as an alternative the analysis as /YQTLu/. On p. 690, the form is identified “orthographisch” as /YQTLu/ with no mention of the analysis as an energetic, which would, of course, have been written identically—hence the two parsings on p. 500. The imperfective aspect is on p. 690 explained as owing to the plurality inherent in the notion of ‘fluttering’. What if the verb means ‘to soar’ rather than ‘to flutter’?

— p. 500 (§73.611.2d), p. 644 (§75.522), p. 732 (§77.412.1b). On p. 500, T. identifies the root of *ngln* in RS 3.325<sup>+</sup> i 15, ii 37 (*KTU* 1.16), as GYL and he vocalizes the form /nagîlunna/ or /nagîlanna/; on p. 644, he identifies the root of the same forms as GWL and vocalizes the stem as /nagûl-/. In the first case, he translates the root by “jubeln” (the same translation is offered on p. 732, there without vocalization or root analysis), in the second by “jauchzen, kreischen.” Surely the first vocalization is preferable, for the verb is cognate with

<sup>930</sup>On the problem of the reconstruction of the first syllable with /â/, see below, remark to p. 632 (§75.511g).

<sup>931</sup>See above, remark to p. 190 (§33.311.5), etc.

Hebrew GL, ‘rejoice’, there the stem vowel is /ī/. In the apparent Arabic cognate, GWL, the stem vowel is /ū/, but that verb does not mean ‘to rejoice’ but ‘to ramble about’. This appears to be a poor basis for the vocalization of Ugaritic GL, ‘to rejoice’. If one were to attempt to correlate these data, it would appear more plausible to propose that there was a verb of movement GWL /yagūl-/ and a verb of sound production GYL /yagīl-/. However that may be, following the Hebrew vocalization for the verb with the same meaning in Ugaritic appears safer than following the Arabic vocalization of a verb with another meaning. The variety of treatment seems to have arisen from T.’s attempt on p. 644 to correlate the forms in RS 3.325<sup>+</sup> with the far more difficult form *tgwln* in RS 15.134:4 (*KTU* 1.82), a text with *ygl* in line 1. The very fact of the different orthographies just four lines apart appears, however, to discredit T.’s analysis of *tgwln* as *plene* spelling for /tagūlāna/ or, even less plausible, as the archaic form /tagwulāna/.<sup>932</sup> Seeing the form as an extremely rare example of a D-stem form of a hollow root appears to be a more acceptable solution because, after all, such forms do appear in the other Northwest-Semitic languages.<sup>933</sup> (On the D-stem vs. the L-stem of hollow and geminate roots, see below, remark to pp. 575-76 [§74.50]).

— p. 500 (§73.611.2d), p. 644 (§75.522). The arbitrary nature of T.’s classification of /YQTLn/ forms is well illustrated by his analyses of *tmtn* in RS 3.325<sup>+</sup> i 22, ii 43 (*KTU* 1.16): on p. 500, the form is given as 3 m.pl. jussive + Energic I and vocalized “*tamûtū-nna*” (the notation does not take account of the fact that the vowel of the penultimate syllable should have become short because the syllable is closed—this, however, would entail loss of the principal marker of plurality, viz., the length of the /u/-vowel, something which T. recognizes, but irregularly); the analysis as a 3 m.pl. indicative is presented as an alternative {“*tamûtūna*”). On p. 644, only the latter is indicated.

— p. 500 (§73.611.2f), p. 506 (§73.634c), p. 625 (§75.331a), p. 731 (§77.412.1b), p. 733 (§77.412.5). T. cannot seem to decide whether *hlmn* in RS 3.340 iv 33' (*KTU* 1.18) bears a pronominal suffix or not, viz., ‘he smote’ or ‘he smote him’. On p. 500, he says that “er schlug ihn” is less likely than “er schlug”; on p. 506, he translates “er schlug ihn” and adds that the analysis without the object suffix is less likely; on p. 624, he allows for both possibilities by parsing the form as “G-SK 3.m.pl. + En. [+ OS 3.m.sg.]”; on p. 731, he mentions only the analysis of the *-n* as the energic morpheme, and on p. 733 he translates without an objective suffix (“Schlage fürwahr ...”). In denying the presence of an object suffix on pp. 500 and 731, he points out that the form in RS 3.322<sup>+</sup> ii 29 (*KTU* 1.19) is *hlm*, in what, in spite of the damaged state of the passage, is clearly a back-reference to the narrative containing *hlmn*. On the other hand, since it cannot be proven that the energic morphemes, as T. would refer to them, may be attached to /QTLa/ forms, that analysis must be rejected in the case of *hlmn* and the *-n* must be analyzed either as the enclitic morpheme *-na* (viz., the productive enclitic morpheme of Ugaritic, not the energic morpheme which may historically contain this same

<sup>932</sup>On this form, see also p. 55 (§21.342.1b), p. 442 (§73.223.5), and p. 641 (§75.521).

<sup>933</sup>This is not to deny the possibility of a /yaqwVI-/ form existing in Ugaritic—cf. Hebrew /yišwāhū<sup>w</sup>/ (Isa. 42:11) the verb that corresponds to Ugaritic *šh/yšh*—but it does deny the likelihood of such a form existing alongside a ‘weak’ form of the same root, not only in the same language but in the same text.

morpheme) or else as the pronominal suffix that arose by fusion of the enclitic and pronominal morphemes (/an + hu/ → /annu/) here attached either to the /QTLa/ form or to the infinitive used narratively. (On the latter dilemma, see above, remark to p. 223 [§41.221.52c], etc.).

— p. 500 (§73.611.2h), p. 622 (§75.234). T. tags the examples in §73.611.2h as illustrating “Belege für den Energ. I in absoluter Position” (i.e., without a following objective suffix). His identification on p. 622 of {yşân[...] } in RIH 77/04+:3' (CAT 1.165) as a 3 m.s. /QTLa/ form has nothing, however, in its favor. Though only a few words of the text have been preserved, the phrase *w l ll*, ‘and at night’, and the mention of the deity *Raşap Guni* suffice to identify the text as belonging to the category of prescriptive rituals. That being the case, the verb belongs in all likelihood to the /YQTL/ category and the {â} reflects either one of the energetic forms (/YQTLan/YQTLanna/)<sup>934</sup> or else the dual (it is possible that another divine named preceded {w rşp . gn} at the end of line 2').

— p. 500 (§73.611.2h), p. 620 (§75.232). In the first section cited, T. quotes RS 18.[386]:2' (KTU/CAT 2.54) as {yşin}; in the second, he considers dividing {yşî X[...]}. Of the sign reconstructed as {n}, however, only the head remains, and the reading of {t} is, therefore, just as plausible as {n}. However that may be, only a few signs are preserved on this fragment and there is no reason whatever to entertain notions of *reconstructing* a form unattested elsewhere. Since there is no preserved context, the form is just as plausibly /QTLa/ as /YQTL/. In the first paragraph cited, T. speaks of his Energic I being “in absoluter Position,” but provides no vocalization, and I have no idea what he means. /yaşî'/ could not have “(a)nna” (T.'s representation of his Energic I) attached to it, for /yaşî'anna/ would be written {yşân} and /yaşî'nna/ is an impossible form. /yaşî'na/ is, of course possible, but that would be a simple /YQTLØ/ + enclitic {-n}, not an energetic form *per se*. In the second paragraph, he proposes that the verbal form may be a jussive /yaşî'/ unrelated to the following putative sign. This is far and away the more plausible of the two analyses, especially since this verbal form is preceded by {[...]l .}, which may be restored as {[...] â]l . yşî[...] } /'al yaşî'/ ‘may he not go forth’. It is unclear why he does not consider any of the /QTLa/ possibilities, viz., that the sign following {yşî} may have been {t} rather than {n} (the editors of the text correctly represented the presence of only the left edge of a horizontal wedge<sup>935</sup>) and that the form may be /QTLa/ in one of the second persons or 1 c.s., viz., /yaşa't-/ , which would, of course, be written {yşit(-)}; or the sign may indeed have been {n} and the form 1 c. pl. (/yaşa'nū/ (which would, of course, have been written {yşîn})). In these cases, the preceding {l} is either the correct negative for /QTLa/ forms, viz., /lā/, or the last sign of some other word.

— p. 503 (§73.626), p. 640 (§75.518). In the first section cited, T. parses {yrd[nnl]} in RS 1.013+:15' (KTU 2.3) as a possible example of the G-infinitive of YRD, ‘to descend’, with energetic ending and a pronominal suffix, but proposes no translation; in the second, he classes the passage with forms of YRD of which the analysis is uncertain. Because the root

<sup>934</sup>Idem, *Les textes rituels* (2000) 842, 843.

<sup>935</sup>M. Dietrich and O. Loretz, *Die Elfenbeinschriften und S-Texte aus Ugarit* (AOAT 13; Kevelaer: Butzon & Bercker; Neukirchen-Vluyn: Neukirchener Verlag, 1976), p. 58, #199.

YRD is intransitive, there is no theoretical basis for taking it as bearing a pronominal suffix (and an energetic with ‘dative’ suffix [‘it will descend for someone’] is even less likely with an intransitive verb, especially in prose). It would be better to cut the Gordian knot by not classing the form under the root YRD.

— p. 503 (§73.627), p. 644 (§75.522), p. 710 (§76.524.42). On p. 503, T. observes that the intransitive verb B(W)T̄ should not take an object suffix in RS 3.367 iv 31' (*KTU* 1.2), on p. 644 he proposes that the verb should mean “zerreißen, vernichten” (though he is uncertain whether it should be hollow or geminate), with the same analysis of the imperative forms in lines 28' and 29' on p. 646 (§75.523), but, on p. 710, he unaccountably translates the phrase in line 31' as “Da schämte(?) sich(?) ... Ba‘lu.” One cannot expect the intransitive verb B(W)T̄, ‘to be ashamed’, to have appeared with an accusative pronominal suffix, nor that a suffix should refer to the subject of a transitive verb (reflexives are not so expressed in the old Semitic languages). A much better case can be made here for the root being geminate and having the meaning ‘to disperse’<sup>936</sup> (though, formally, one cannot rule out that the forms in lines 28' and 29', which do not bear a suffix, were from B(W)T̄ while *ybt̄nn* in line 31' was from BTT̄<sup>937</sup>).

— p. 504 (§73.632). Two of T.’s claims regarding the Energetic II (his Energetic III, i.e., /-an/) may be questioned: (1) that forms written {yqtlk} belong necessarily to this category, that is, that the /n/ of the /-an/ morpheme would have assimilated to the /k/ of the suffix; and (2) that verbal forms with enclitic {-m} would also belong to this category, that is, that the /n/ of the /-an/ morpheme would have assimilated to the /m/. Biblical Hebrew forms such as /yišmorka/ and epigraphic {ybrk} /\*yabarrikka/ ← /\*yabarrik + ka/, ‘may he bless you’,<sup>938</sup> not to mention the standard plural form /yiqṭlūhū/, show that pronominal suffixes were not necessarily linked with an energetic morpheme in proto-Hebrew. The fact that enclitic {-m} could be attached to words representing any part of speech leads to the same conclusion regarding Ugaritic forms written {yqtlm}.

— pp. 505-6 (§73.634a), p. 594 (§74.622.3), p. 651 (§75.527g). Though less crucial here than in the case of RS 24.248:19 (see above, remark to p. 211 [§41.12], etc.), T.’s proposals that the {-n} of {t̄t̄t̄bn} in RS 1.003:54 (*KTU* 1.41) may not incorporate a pronominal suffix or may require emendation because the verbal form should be plural indicative (i.e. of the /YQTLūna/ type, for which the expected form would be {t̄t̄t̄bnh} or {t̄t̄t̄bnn}) may be considered unnecessary. If the form is 2 m.s.,<sup>939</sup> there is no problem as it stands. The proposals for emendation may be considered particularly pernicious: that the form should be

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<sup>936</sup>This is a classic interpretation of the passage: see Caquot and Szymer, *Textes ougaritiques* I (1974) 139.

<sup>937</sup>For a literary argument against this interpretation of the forms, see Pardee, *Context* I (1997) 249 n. 63.

<sup>938</sup>Kunttillet Ajrud *ybrk wyšmrk*, ‘May he bless you and keep you well’ (Z. Meshel, *Kunttillet ‘Ajrud: A Religious Centre from the Time of the Judaean Monarchy on the Border of Sinai* [Catalogue 175; Jerusalem: The Israel Museum, 1978], p. 14 of English section—which is the correct reading, not {ybrk[k]} as indicated on p. 20 of the Hebrew section; on this reading, see J. Renz, *Handbuch der althebraïschen Epigraphik* [Darmstadt: Wissenschaftliche Buchgesellschaft, 1995], vol. I. p. 62); the same form appears twice in the Ketef Hinnom priestly-blessing texts (cf. Renz, idem, p. 454-55).

<sup>939</sup>Pardee, *Les textes rituels* (2000) 151, 152.

read {tṭ[t]bn'n!} because the single {n} present consists of four wedges<sup>940</sup> is belied by the hundreds of signs with extra wedges in Ugaritic;<sup>941</sup> the emendation to {tṭ[t]bn}, i.e., from the causative stem to the simple stem ('you shall return' instead of 'you shall cause him to return') is purely hypothetical and based on the false premise that all such forms should be plural. Only on p. 651, does T. takes the form at face value, analyzing it as 3 m.pl. long form, without pronominal suffix, which is at least a superficially plausible analysis, since pronominal suffixes need not be expressed if clear from context.

— p. 506 (§73.634a). Though he puts the appropriate qualifiers in the introduction to this paragraph (p. 505), T. nonetheless interprets the {-n} of {[yḥ]rṭkn} in RIH 77/18:7' (CAT 1.175) as Energic II (i.e., his Energic III, that is, /-an/). It appears more plausible, in this prose text where there are no other certain energic affirmatives, to see in the {-n} the energic form that contains a pronominal suffix (see remark below to p. 605 [§74.632], etc.). It may be added that here he presents the form as “yḥrk-n” in spite of the fact that the fragility of the reading was known to him (below, p. 605 [§74.632], he transliterates “y/t]ḥ<sup>2</sup>r<sup>2</sup>kn”). The caution shown on p. 605 is merited, for the {ḥ} is no longer visible and the {r} is badly damaged.<sup>942</sup> The point for the interpretation of the passage is that there is no way of knowing what the subject marker was on the verb. Furthermore, there is no way of being certain who the real subject was of the following verb ({yšḥmm}) nor what the exact form was of the verb ending in {[--]rṭkn}. On p. 506, where the ambiguity of the {-n} of {[yḥ]rṭkn} is stressed, T. interprets {yšḥmm} as active (“Er ... erhitzt (es)”), leaving open the possibility that both forms may be transitive but without an expressed object suffix. In the three other places in the grammar where {yšḥmm} is treated, however (p. 605 [§74.632], p. 673 [§75.61e], p. 679 [§75.676]), the form is everywhere parsed as Š-passive (only once, p. 605, is the transitive alternative even mentioned). Finally, it must be remarked that the left side of this tablet has disappeared and the precise relationship between the ‘vinegar and salt’ mentioned in the previous line and the verbs under discussion may not for that reason be considered certain (on p. 605, the text is translated as though the lacuna was not there, though it is properly indicated in the transliteration).

— pp. 509-18 (§74.22). Though T.'s theoretical arguments for the G-passive stem are valid, even convincing, they must be tempered by two considerations: (1) the existence of the form is not firmly established by even a single explicit datum from Ugarit, neither by a writing with a ʾ-sign nor by a clear Ugaritic form in an Akkadian text; (2) many of the forms listed under the heading “Relativ sichere Belege” (pp. 511-13 [§74.222.2]) may be either third-person indefinite subject forms or may belong to the N-stem (the latter possibility must be taken into consideration particularly in prose texts, where the development of the N-stem from its original function towards the expression of passivity is more likely than in the more archaic language of poetry (see remarks below to the Gt-stem [pp. 518-32 (§74.23)] and to

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<sup>940</sup>I did not copy the sign as consisting of four wedges.

<sup>941</sup>See my article “RIH 77/27, RIH 77/12, RIH 78/26 et le principe de l'écriture cunéiforme alphabétique” forthcoming in *Syria* 79.

<sup>942</sup>The editors transcribed {[y]ḥrṭkn}: Bordreuil and Caquot, *Syria* 56 (1979) 296.

the N-stem [pp. 532-43 (§74.3)]). The plausibility of the existence of the form rests on two considerations: (1) the semantics of the N-stem and infixed-*t* forms in Ugaritic poetry are closer to a middle voice than to the passive; (2) in certain passages, particularly where there is a potential feminine subject and a *t*-preformative verb (e.g., *qšt* and *ttn* in RS 3.322+ i 16 [KTU 1.19]), taking the verb as a G-passive appears preferable to taking it as a third person indefinite subject form.

— p. 511 (§74.222.2). Because the text immediately preceding the phrase *l yīḥd šṭqšlm b ūnt* in RS 15.125:1'-2' (KTU 2.19) has disappeared and because the passive idiom *ḥD b*, 'to be taken for', is not elsewhere attested, this example may not be counted among the "Relativ sichere Belege" of the G-passive stem.

— p. 511 (§74.222.2), p. 512 (§74.222.2), p. 638 (§75.517a), p. 649 (§75.527a), p. 735 (§77.51b), p. 887 (§95.232). The same conclusion must be drawn with regard to the forms *ydk* and *tdkn* in the hippiatric texts. There is nothing in these texts that speaks against the analysis of these forms as having a third-person indefinite subject, singular in the first case plural in the second,<sup>943</sup> while there are considerations that cast doubt on the analysis as passive forms. Specifically against the latter analysis are (1) the appearance of the form *ydk* when multiple ingredients are prescribed (e.g. RS 17.120:28 [KTU 1.85]) and (2) the form *yšq* by which each paragraph of these text concludes, for this verb also is preceded by multiple ingredients which would constitute the real subject of the verb if it were passive.<sup>944</sup> So, if these verbs were passive, one would expect marked plural forms to have been used; in prose, as T. has shown, those forms would have to have been *tdkn* and *tšqn*. On p. 512, T. analyzes all the appearances of *yšq* in these texts as G-passives, translating "und sie (sc. eine Medizin) wird in [seine] Schnau[ze] gegossen," but there is no singular 'medicine' in the text, only multiple ingredients. On p. 887, the forms are identified explicitly as singular because the preceding list of ingredients constitutes a "Singularbegriff." If the analysis of  $\sqrt{\text{DK(K)}}$  and  $\sqrt{\text{YŠQ}}$  as G-passives is unlikely for contextual reasons, it is all the more unlikely for  $\sqrt{\text{MSS}}$  in these same texts, for one would not expect a G-stem form of a geminate root to be written {ymsš} or {ymšš} as is the case with this verb. T. vocalizes /yumsasu/, with no explanation for why such a hypothetical form would not have become /yumass-/ and be written {yms}. In §75.6, T. considers that the few attested forms of geminate roots with two tokens of the second consonant in the orthography may be /yaqālil-/ forms, a likely explanation of some of these forms (see remark below to pp. 577-79 [§74.511a, b], etc.). Such a solution is not, however, required for  $\sqrt{\text{MSS}}$ , for that root is intransitive in the G-stem ('melt, dissolve'), while the form is plausibly transitive in the hippiatric texts and may for that reason be parsed as a D-stem factitive.<sup>945</sup>

— p. 512 (§74.222.2), p. 518 (§74.224.3), p. 563 (§74.415), p. 644 (§75.522), p. 649 (§75.527a). The analysis on p. 644 of *ydt* in RS 3.340 i 19 (KTU 1.18) as G-active breaks

<sup>943</sup>Pardee, *Les textes hippatriques* (1985) 19, 48.

<sup>944</sup>*Ibid.*, p. 51.

<sup>945</sup>*Ibid.*, pp. 25, 69; idem, "Ugaritic Science," in *The World of the Aramaeans. Studies in Language and Literature in Honour of Paul-Eugène Dion III* (ed. P. M. M. Daviau, et al.; JSOTSS 326; Sheffield: Sheffield Academic Press, 2001) 223-54, esp. 248.

into a quite regular stream of parsings as G-passive; there is, however, no cross-reference to call this break to the attention of the reader.

— p. 512 (§74.222.2). The reading of {ʿyhsl} in RS 24.247<sup>+</sup>:14 (*CAT* 1.103<sup>+</sup>) is not just “sehr fraglich,” it must be judged a fantasy on the part of the editors of *CAT* since there remains no trace of the first three signs on the tablet and the trace of the fourth does not correspond to the form of {l}.<sup>946</sup> As to deciding between T.’s analysis of this form in its certain appearance in line 55 as a G-passive or mine as a N-stem,<sup>947</sup> it should not be left out of consideration that this is a prose text where a slippage towards a passive connotation of the N-stem may be considered possible.

— p. 512 (§74.222.2), p. 639 (§75.517a), p. 687 (§76.331). Though T. consistently parses *ytn* in RS 2.[008]<sup>+</sup> v 27 (*KTU* 1.4) as G-passive, his more detailed parsing is not so consistent: in the first section cited, the form is indicated with no sign of doubt as a jussive, in the second only as /YQTL/, in the third as /YQTLu/ with no sign of doubt. Since the form occurs in a passage of direct speech in which ‘*Anatu* is announcing that *Ba‘lu* is to be the recipient of good news (vBŠR), the parsing as /YQTLu/ is preferable to the analysis as a jussive (‘let someone give!’). Moreover, the analysis as a passive cannot be considered certain: the subject may simply be indefinite, lit. ‘(some)one will give you a house’.<sup>948</sup>

— p. 512 (§74.222.2), p. 639 (§75.517a). The fact that the ends of lines 11-14 of RS 24.248 (*KTU* 1.104) are missing means that the analysis of *ytn* in line 12 as a G-passive is particularly precarious.

— p. 512 (§74.222.2), p. 668 (§75.537a). In the first section cited, *td* in RS 2.[008]<sup>+</sup> vi 32 (*KTU* 1.4) is said to derive from “*tuwday/w*,” which in turn would be from “*tuyday/w*,” while on p. 668 the derivation is indicated simply as “*tuwday*.” The first derivation is quite unclear since the standard development in Northwest Semitic was from I-w to I-y; the idea is apparently that this was originally a I-y root that shifted to I-w in the G-passive under the influence of the preceding /u/. This would have rendered the hypothetical contraction to /û/ more plausible.

— p. 512 (§74.222.2), pp. 515-16 (§74.223.2), p. 639 (§75.517a), p. 717 (§76.541b). T. consistently prefers the analysis of *yld*, ‘will be born’, in RS 2.[004] ii 14’ (*KTU* 1.17) as a G-passive /QTLa/ form, but he never states the reasons for this preference. The form is, of course, graphically unmarked for /QTLa/ or /YQTL/, it expresses an incomplete act, and it is preceded in this passage by the conjunction *k*, not by the emphatic particle *l* (which is attested once in poetry with a /QTLa/ form expressing an incomplete act—see remark below to p. 717) nor by the conjunction *w* (which is attested in prose with /QTLa/ forms in a structure similar to the Hebrew ‘*wāw*-consecutive’—see also the remark to p. 716-17). There is, therefore, no obvious reason why it should be parsed as /QTLa/, nor do I see a less obvious reason. T. is less consistent with his alternative analyses, twice indicating /YQTLu/

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<sup>946</sup>Idem, *Les textes rituels* (2000) 541, 1278.

<sup>947</sup>Ibid., p. 552.

<sup>948</sup>Cf. Pardee, *Context I* (1997) 260.

(pp. 639 and 717) once /YQTLØ/ (p. 516). The latter appears preferable, for the utterance is indicative, not volitive.<sup>949</sup>

— p. 512 (§74.222.2), p. 635 (§75.512), p. 686 (§76.324), p. 688 (§76.332), p. 718 (§76.55), p. 786 (§83.113i), p. 801 (§83.24a). The verbs written {ytn}, from YTN, ‘to grow old’, and {ytn}, from YTN, ‘he gave’, in RS 15.082:6 and 8 (*KTU* 4.168) are both classified as /YQTL/ (everywhere but p. 718, where *ytn* is listed with stative /QTLa/ forms);<sup>950</sup> this in spite of the fact that lines 5-8 constitute a paragraph that is preceded and followed by paragraphs listing simple distributions of textiles or garments. The facts (1) that the recipients appear to be linked with the cult, indeed in one case to be a deity (*ūšḥry*, l. 10), and (2) that the motivation is registered in this case (*k ytn*, ‘because they have grown old’, l. 6 [T.’s usual interpretation is ‘when they will grow old’]) change nothing in the fact that this appears to be an administrative text registering outlays from the royal store. T.’s classification appears to arise primarily from a comparison with RS 15.115:54-64 (*KTU* 4.182) where similar vocabulary appears along with verbs that are indubitably /YQTL/ in form. That text is very differently structured, however, with the word *šnt*, ‘year’, in line 1, and a sequence of month names in lines 19-40. Unfortunately, it is too poorly preserved to permit an understanding of the relationship between these chronologically defined paragraphs, of which the structure is, as far as we can tell, always nominal, and the three final paragraphs where the imperfective forms appear. The visible structural differences appear sufficient, however, to weaken the hypothesis that RS 15.082:5-8, located in the middle of that text, would, like the three final paragraphs of RS 15.115, express imperfectively one of the distributions. *ytn* is also cited on p. 686 and *ytn* on p. 688 as examples of /YQTLu/ in the protasis and the apodosis of a temporal/conditional phrase, but, since neither verb bears an orthographic marker of aspect (/QTL/ vs. /YQTL/) or of mood (/YQTL/ vs. /YQTLu/), this classification appears arbitrary. A further complication is that the particle introducing the sentence is *k*, which can introduce a causal clause, a temporal clause, or a conditional clause. The first of these possibilities is just as plausible as the temporal classification (p. 801), if not more so: ‘As for the clothing of the *trmnm*, because it was old, (new) clothing was issued to them in the royal palace’ (*mlbš trmnm k ytn w b bt mlk mlbš ytn lhm*).

— p. 512 (§74.222.2), p. 668 (§75.537a), p. 668 (§75.537c). Because there is every reason to believe that the verb KLY is basically intransitive in Ugaritic as it is in Hebrew and because the N-stem is clearly attested (by the /QTLa/ form *nkly*), there is no reason to take *ykl* in RS 19.015:1 (*KTU* 1.91) as a G-passive. The form could be either a simple G-stem, with reference to the disappearance of the wine through use in the rituals named in the course of the text, or N-stem with a more passive connotation.<sup>951</sup> T. remarks that “*√kly* Gp

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<sup>949</sup>Pardee, *ibid.*, p. 345; Parker *apud*, Parker, ed., *Ugaritic Narrative Poetry* (1997) 56. Others consider that the birth has already taken place and that the form is for that reason perfective (cf. Wyatt, *Religious Texts* [1998] 264 with note 57).

<sup>950</sup>On p. 786, the passage is only translated and the precise parsing behind the translation of *ytn* by “alt (geworden) ist” cannot be determined.

<sup>951</sup>Pardee, *Les textes rituels* (2000) 494-96.

ist offenbar semantisch deckungsgleich mit  $\sqrt{kly}$  N” (p. 512) but he does not address the issue of why an intransitive root such as this may be expected to have both a G-passive and a N-stem. Only on p. 668 does he explicitly allow for the possibility of  $ykl$  being N-stem. The conclusion that KLY would have had a G-passive is certainly not forced upon us by the use of  $kly$  as well as  $nkly$  in administrative texts, apparently with essentially the same meaning,<sup>952</sup> for, as already observed, the G-stem was in all likelihood intransitive. Nor is it to be forgotten that passivity was not the primary notion of the N-stem (see remark below to pp. 532-43 [§74.3]) and, in a sense, a G-intransitive is as close to N-stem semantics as is a G-passive. Also in the first section cited, T. observes that  $ykl$  in RS 24.248:3 (*KTU* 1.104) may perhaps be the same form as in RS 19.015, but recognizes that the context is broken; on p. 668, he cites this second passage as a certain occurrence of either the G- or the N-stem. In this section (p. 512), T. parses the form in RS 19.015 as either a /YQTLØ/ perfective (“PK<sup>K</sup>i”) or as /YQTLu/ (“PK<sup>L</sup>”); on p. 668, the form in both texts is parsed as “PK<sup>K</sup>,” with no specification as to whether it is perfective or jussive. Because both texts are in prose, where the /YQTLØ/ perfective is not used, that analysis is ruled out. Because RS 19.015 is administrative, where the jussive is hardly to be expected, the analysis as an indicative with contracted triphthong appears necessary (/yiklayu/ → /yiklû/ or /yinkaliyu/ → /yikkalû/<sup>953</sup>); RS 24.248 is a ritual text, where jussives are not frequent, and such must be the preferred hypothetical analysis there also. It thus appears quite likely that at least RS 19.015:1 {yn . d . ykl . bd . ṛl[...]} is to be translated “Wine which is to be consumed under the supervision of [...]”<sup>954</sup> A similar translation of RS 24.248:3 is plausible,<sup>955</sup> but, given the state of the tablet, the analysis cannot be deemed certain nor may the form be exploited for the purposes of writing a descriptive grammar of Ugaritic.

— p. 513 (§74.222.2).  $ytk$  in RS 1.003:12 (*KTU* 1.41) cannot be counted among the “Relativ sichere Belege” of the G-passive because the subject or the object of the verb, or both, has/have disappeared in the preceding lacuna. As regards the length of this lacuna, T. indicates that only two or three signs have disappeared, but a comparison with RS 18.056 (*KTU* 1.87) shows that the number of missing signs may be as high as six.<sup>956</sup>

— p. 513 (§74.222.2). Because of the fragmentary state of the tablet, the precise analysis of {y‘db} in RS 1.023:11’ (*KTU* 1.50) cannot be considered even relatively certain.<sup>957</sup>

— p. 513 (§74.222.2). T.’s reconstruction or {[t]pṭh} in RS 2.[008]<sup>+</sup> vii 19 and analysis thereof as a G-passive seems to have been anticipated by Wyatt,<sup>958</sup> but T. seems not to

<sup>952</sup> T. parses  $kly$  in these passages as G-passive /QTLa/ forms (p. 515 [§74.223.1], p. 516 [§74.223.2], p. 668 [§75.537a]).

<sup>953</sup> In *Les textes rituels* (2000) 491, I vocalized “yiklâ”; for the reasons behind the revision to /yiklû/yikkalû/, see below, remark to p. 656 (§75.531e).

<sup>954</sup> Pardee, *Ritual and Cult* (2002) 215.

<sup>955</sup> Idem., *Les textes rituels* (2000) 567; idem, *Ritual and Cult* (2002) 35.

<sup>956</sup> Idem., *Les textes rituels* (2000) 172-74, 470-72.

<sup>957</sup> Ibid., p. 348.

<sup>958</sup> *Religious Texts from Ugarit* (1998) 109 “let a rift [be op]ened in the clouds” (without textual note).

have been aware of it (the work is not cited in his bibliography) and this reconstruction qualifies, therefore, as a genuine “n[eue] L[esung].” More recently, a solid case has been made for reconstructing {[i]pth}, ‘I indeed shall open’.<sup>959</sup>

— p. 513 (§74.222.2), cf. p. 310 (§54.133.1b), p. 650 (§75.527a), p. 863 (§93.33). According to T.’s analysis of *yšt* in RS 24.258:29' (*KTU* 1.114), viz., as /YQTLu/, it cannot be considered likely that *yšt* in line 31' is passive because it is preceded by what would be two subjects (*riš pqq w šrh*); the expected spelling of such a form would be {y/tštn}.<sup>960</sup> Because it is preceded by what would be two subjects, the explicit parsing of *yšt* in line 31' as 3 m.s. passive is incomprehensible. Finally, strong arguments have been provided for this verb representing the root ŠTY, ‘to drink’, rather than ŠT, ‘to put’.<sup>961</sup>

— p. 514 (§74.222.3), p. 633 (§75.512), p. 638 (§75.517a), p. 660 (§75.532), p. 668 (§75.537a). T. first explicitly analyzes *tdn* in RS 24.248:19 on p. 514 as from a root YDY ← WDY<sub>1</sub> that would mean “niederlagen, ablegen,” and this analysis is repeated, though with some hesitation, in the other paragraphs cited (it is also assumed on p. 211 [§41.12] and p. 213 [§41.132e], where the text is translated—see remark above to these pages). Such a meaning does not fit the passage, however, and, as a root DNY, ‘to approach’, is rather clearly attested in another ritual text (RS 24.266:22'-23' [*KTU* 1.119]), that identification may be preferred. In both passages, it would be a D-stem factitive meaning literally, ‘to bring about nearness, i.e., to bring near’. Here, as in RS 24.266:22' (see below, remark to p. 633 [§75.512] etc.), the writing without {y} would mean either that jussive forms were used in these directives to officials (/tadanni/ ← /tadanniy/) or that contraction took place in these D-stem forms (/tadannû/ ← /tadanniyu/).

— p. 514 (§74.222.3). For the grammar and semantics of *tprš*, ‘it (the land) will be scattered’, in RS 24.247:53' (*KTU* 1.103<sup>+</sup>), Biblical Hebrew, in particular Ezek. 17:21, is of at least equal importance as a point of comparison with Akkadian *sapāḥu* (T. cites only the latter).<sup>962</sup>

— p. 514 (§74.222.3), p. 888 (§95.234). T. does not give his reasons for considering that, according to his interpretation of { *yšp<sup>l</sup>k<sup>l</sup>* . *kmm* . *ārš* | *kšpm* . *dbbm* } (RS 92.2014:12-14 [RSO XIV 52]<sup>963</sup>), viz., “Es soll(en) wie Wasser(?) zur Erde hin ausgegossen werden die Beschwörer und Zauberer,” *yšpk* would not show number concord with the real plural subject. It is apparently in part because elsewhere in the text, the negative particle is *l*, not

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<sup>959</sup> M. Prosser, “Reconsidering the Reconstruction of *KTU* 1.4 VII 19,” *UF* 33 (2001) 467-78; cf. Pardee, “On Psalm 29: Structure and Meaning” (forthcoming).

<sup>960</sup> T., p. 458 (§73.272).

<sup>961</sup> See here above, remark to p. 310 (§54.133.1b) and Pardee, *Les textes para-mythologiques* (1988) 22-23, 72. It is true that the writing {*yšt*} is unexpected for the /YQTLu/ of a III-y root, but the very same writing is attested above (l. 16) in the mythological portion of this text, wherein, for whatever reason, “long” forms, viz., dual and plural forms that end in -n, appear with some regularity. Moreover, the form *tštn*, apparently a contracted “long” plural form (T. p. 663) of the verb ŠTY, appears three times in this text.

<sup>962</sup> Pardee, *AfO* 33 (1986) 143.

<sup>963</sup> Preliminary edition in *Les textes rituels* (2000) 829-33; see also now Bordreuil and Pardee, *Manuel* (2004), text 18 in the *Choix de textes*.

*āl*, and the expected verbal form is, therefore, indicative (/YQTLu/), not jussive (/YQTLØ/); more importantly that *yšpk* is not expected as a 3 m.pl. form—that is normally /tQTL/ in Ugaritic. This was, in any case, the verbal usage assumed in the vocalized text included in the manuscript of the *editio princeps* made available to T.—here and in the case of {tūdn} (l. 8).<sup>964</sup> It is furthermore surprising that he does not even mention the interpretation proposed in the manuscript made available to him, viz., that this form would be jussive; in that manuscript, we took the form as active, rather than passive, and as jussive, rather than indicative (/yašpukū/), with the sorcerers and speakers of evil the subject of the verb and the evil speech the object of the verb. Since then, the presence of *l tūdn* in line 8, which in context can only mean ‘they must not heed’ (see remark below to p. 612 [§75.212.12]), has led us to propose that this text shows a consistent structure of negative volitive verbal forms preceded by *l* (/lā/), rather than by the expected *āl* (/’al/). So rare a usage must have been extremely emphatic and hence appropriate for an incantation.<sup>965</sup> As for *yšpk*, I now agree with T. that it is unlikely that a 3 m.pl. form would be written with *y*-preformative, and it appears more likely that it is 3 m.s. indefinite subject: ‘May (someone) in the same manner pour out the (evil-)speakers, the sorcerers to the earth’, i.e., with the latter as the object rather than the subject of the verb.<sup>966</sup>

— p. 515 (§74.223.1), p. 569 (§74.423), p. 618 (§75.225), p. 619 (§75.228d). T.’s willingness to propose interpretations without mentioning other views is particularly striking in the case of *lāk* in RS 29.093:13 (*KTU* 2.70), parsed as G-passive /QTLa/, D-passive /QTLa/, or G infinitive “da kein Subj. genannt ist” (pp. 515, 569), with no mention made of the possibility of taking the form as an imperative, a form that also requires no stated subject in the immediate context.<sup>967</sup> Since that is the only analysis that, in my estimation, makes sense of the passage and since T. offers no overall interpretation of the passage to enable the critic to evaluate his rendering, it is difficult to take any of his multiple proposals seriously.

— p. 515 (§74.223.1). It appears plausible that the G-passive perfect in Aramaic, of which the form is /q<sup>3</sup>tīl/, is historically a verbalization of the G-passive participle, rather than a linear development from a proto-Aramaic form with /i/ in the second syllable—the presence

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<sup>964</sup>The same is immediately visible in my preliminary edition in *Les textes rituels* (2000) 830: “ti’danū” and “yašpukū.”

<sup>965</sup>T. cannot object too strongly to this analysis, since he has a section specifically devoted to the category “I vor volitiver PK<sup>K<sub>v</sub></sup>” (pp. 815-16 [§87.14]). It is a sparsely populated section, but these new examples in a well-preserved context re-enforce the existence of the grammatical category. According to Shulman, *ZAH* 13 (2000) 169 with note 7, there are three examples of *lō’* + jussive in the Hebrew Bible.

<sup>966</sup>See Bordreuil and Pardee, *Manuel* (2004) II 69. In *UF* 33 (2001) 696, T. maintains his interpretation of the form as a G-passive 3 m.s. along with the question mark after the indication of “Wasser” (apparently a plural) as the real subject. In *UF* 34 (2002) 144, Ford follows this analysis remarking that it “does not agree in number with the plural subject, presumably due to the passive voice and the distance of the subject from the preceding verb.” Recognizing the common use of the 3 m.s. active, indefinite subject, in place of the passive allows us to avoid these grammatical gymnastics.

<sup>967</sup>Pardee, *AAAS* 29-30 (1979-80) 24, 28; idem, *Context* III (2002) 110; Bordreuil and Pardee, *Manuel* (2004) 88, 89.

of /i/ in the second syllable of both may be taken as an indicator of this development, though it could, of course, represent a vowel that was secondarily lengthened under the influence of the passive participle.<sup>968</sup> T.'s citation of the form as proof that the Ugaritic G-passive /QTLa/ was of the /qutla/ type represents, therefore, only one view of the reconstruction of the proto-Aramaic form. If T.'s view is incorrect, the only remaining indicator of the /qutla/ type is Arabic. Hebrew shows /qutal/, and the indicators are thus in equilibrium. If one gives any weight to the concept of a system of markers in proto-West-Semitic, and perhaps in Ugaritic, one must prefer the hypothesis that the G-passive was /qutala/, like the D-passive /quttala/ and the Š-passive /šuqtala/. (T. prefers /quttila/ but /šuqtala/ for these forms, with no apparent concern for system [p. 569, §74.423; p. 605, §74.633]). Arabic consistently shows the /u/ - /i/ sequence in derived-stem passive forms, while Hebrew and Aramaic show /u/ - /a/. Ugaritic could in theory have hewn to either pattern, but in favor of taking the Hebrew (and Aramaic?) forms as indicative of the earlier system, one might consider the argument that the Arabic forms represent secondary formations based on the /qatila/ stative pattern—after all, Arabic in general shows more adaptations to various secondary systematizations than do Hebrew or Aramaic. On the other hand, if *rš* in RS 2.[003]<sup>+</sup> i 12 (*KTU* 1.14) is indeed G-passive of a geminate root (see remark below to p. 676 [§75.64]), the syncope of /i/ in a form that was originally /rušiša/ is easier to understand than would be the syncope of /a/ if the base form was /qutala/.

— p. 515 (§74.223.2), p. 639 (§75.517a). Given the uncertainties regarding verbal morpho-syntax in poetry, I do not see how T. can be certain that *ybl* in RS 2.002: 52, 59 (*KTU* 1.23) and in RS 3.322<sup>+</sup> iv 51 (*KTU* 1.19) is /QTLa/ or, for that matter, that the form is passive (the form could be active with indefinite subject).

— p. 516 (§74.223.2). The likelihood that the G-stem of KLY is intransitive (see above, remark to p. 512 [§74.222.2]), means that taking *kly* in RS 18.051:1 (*KTU* 4.361) and RS 18.052:1 (*KTU* 4.362) as a G-passive cannot be considered the preferred solution; far more reasonable to suppose that it is either a G-stative ('X became depleted') or D-passive ('X was used up').

— p. 516 (§74.223.2). The asterisk (the siglum for a restored word, in whole or in part) attached to the reference to *qbītm* in RS 34.126:3 (*KTU* 1.161) is in error, for the tablet is well preserved at that point and the reading certain.<sup>969</sup>

— p. 517 (§74.223.2), p. 533 (§74.32). Surely the interpretation of *ngh* in RS 2.[003]<sup>+</sup> ii 32, 33 (*KTU* 1.14), as a G-stem passive participle is just as plausible, if not more so, than T.'s

<sup>968</sup>Bauer and Leander, *Grammatik des Biblisch-Aramäischen* (Tübingen: Niemeyer, 1927) 104, hold that the Aramaic development was /qutala/ → /qutla/ (under the influence of the /qatila/ stative pattern) → /qutīl/ (under the influence of the G-stem passive participle, which would have been /qatīl/). T. holds (p. 518 [§74.223.2]) that the /q<sup>o</sup>tīl/ pattern in Aramaic constitutes the replacement by an adjectival form of the old passive participial form which should have been /qutal/. However that may be, the /qatīl/ → /q<sup>o</sup>tīl/ form is surely old and appears to have been the basis for the finite form—whether it be by mutation or by analogy to /qatīl/—which was spelled {qtyl} in old texts where the *mater lectionis* in all probability represents a phonemically long vowel.

<sup>969</sup>Bordreuil and Pardee, *Syria* 59 (1982) 122, 124; idem, *Une bibliothèque* (1991) 153; see the new photograph and copy in idem, *Manuel* (2004), text 13 in the *Choix de textes*.

analysis as G-passive or N-stem /QTLa/. The vocalized version would be ‘*adānu nagūbu wa yaši’ // šaba’u šaba’i nagūbu*, ‘The throng having been provisioned, let it go forth, even the mighty army (which will have been) provisioned’. This analysis avoids the need to take *ngb* as representing the not-particularly-common category of “precativ perfect” (note T.’s translation “sei mit Proviant ausgerüstet”). The *w* preceding the main verb in the first line of the verse is certainly no impediment to the interpretation of *ngb* as an attributive adjective, for such constructions are common in Northwest Semitic.

— p. 517 (§74.223.2), p. 605 (§74.633), p. 650 (§75.527a). I also fail to see why *nbt* // *šmrgt* in RS 2.[008]<sup>+</sup> I 31', 32' (*KTU* 1.4) should be analyzed solely as from the G-passive /QTLa/. The verse reads *kt il nbt b ksp // šmrgt b dm ḥrṣ*, “... a (throne-)stand for <sup>3</sup>*Ilu* with silver decorations // interspersed with (decorations of) ruddy gold.”<sup>970</sup> The two modifying clauses could, of course, be unmarked relative clauses, another common construction in the Northwest-Semitic languages, particularly in poetry. The analysis as passive participles is again, however, just as plausible if not more so, yet it goes unmentioned.

— p. 517 (§74.223.2), p. 650 (§75.527a), p. 675 (§75.64). The form *nbt* discussed in the preceding remark is identified in the first two sections cited here as from a hollow root and that decision is defended on p. 650. On p. 675, it is identified as from a geminate root, with no indication of why the former decision was not preferred. If the forms are passive participles, as was suggested in the preceding note, then the root must be hollow, not geminate, for a passive participle would have a long vowel between the second and third consonants and be written {nbb} (see T. p. 676 [§75.65b]).

— p. 517 (§74.223.2). RS [Varia 4]:12 (*KTU* 2.14) has been read as containing the reading {rgm}, which has always posed a problem to interpreters because the real subject of the verb is feminine. Because it would again involve the concept of a ‘precativ perfect’, this time in a prose text, T.’s analysis of the form as a G-passive perfect cannot be considered the most likely interpretation. Taking it either as an infinitive used in place of a finite form (‘let her mention’, with reference to *Tarriyelli*, who has just been mentioned) or as an imperative addressed to the recipient of the letter would better fit the grammar of Ugaritic prose<sup>971</sup>, but only the latter alternative is mentioned by T. In point of fact, however, my collation of the tablet in February of 2004 showed that the reading is incorrect: there is a {t} after {rgm} on the right edge of the tablet and the form is thus marked for the gender of the real subject, *Tarriyelli* the queen or the queen-mother.<sup>972</sup> The phrase *yšāl tryl p rgmt l mlk šmy /yiš'al tarriyelli pa ragamat lê malki šumaya/*, ‘may he ask of *Tarriyelli* so that she might mention my name to the king’, appears, therefore, to contain a new example of the syntagmeme that consists of a conjunction followed by a /QTLa/ form wherein the combination expresses the imperfective.

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<sup>970</sup>Pardee, *Context I* (1997) 256.

<sup>971</sup>Pardee, *Context III* (2002) 114 n. 218.

<sup>972</sup>See Bordreuil and Pardee, *Manuel* (2004), text 35 in the *Choix de textes*. Our thanks to the owner of the tablet, Dr. Farid Haddad, for permission to consult the original in his home.

— p. 518-32 (§74.23). T. might have done better to discuss the semantics of the Gt-stem in the introduction, rather than in the conclusion to the section (pp. 531-32 [§74.237.1]). Had he done so, the reader would have been able to gauge his semantic classification of each form cited in terms of the overall classification; in the present arrangement, the reader finds a series of semantic classifications of individual forms, followed by a theoretical discussion. However that may be, I was surprised to find the description of the semantics of certain forms classified as “pluralische (iterative bzw. durative) Nuance” (quotation from p. 520 with respect to  $\sqrt{\text{HLK}}$ ; the word “pluralische” appears again p. 521, with respect to  $\sqrt{\text{H}\text{ŠB}}$ <sup>973</sup>) although this notion of plurality does not appear below in the section on semantics. Because, in traditional comparative Semitic grammar, plurality has been linked with the D-stem, rather than with the G, a tradition followed by T. below in the section on the D-stem, and because the primary function of the t-stems in old West Semitic appears to have been reflexive (see remark below to the N-stem, pp. 532-43 [§74.3]), a defense of the analysis of the Gt as expressive of plurality would have been in order. As regards the further classificatory terms in parentheses in the above quotation, because the expression of iterativity is one of the frequent functions of the /YQTLu/ form in Ugaritic, I see no reason to link that notion in any particular way with the t-stems; note in this respect that none of the Gt /QTLa/ forms is analyzed by T. as expressive of iterativity (pp. 528-29 [§74.234]).<sup>974</sup> Finally, I see no reason to link durativity, any more than iterativity, with infixed-*t* forms. The expression of durativity does not appear to have been at the forefront in the West-Semitic aspectual system,<sup>975</sup> and I see no particular reason to expect the Gt-stem to have been in any way specialized in the expression of this aspect of action.

— p. 518 (§74.231). Because ‘attenuation’ (/a/ → /i/ in a closed unaccented syllable) is not proven to have occurred in Ugaritic, the fact that the Gt /YQTL/ was of the /yiqtatil-/ type, as opposed to Arabic /yafta‘il-/,<sup>976</sup> may be explained as a sub-section of the so-called ‘Barth-Ginsberg law’, viz., that the proto-West-Semitic /YQTL/ form /yaqtal-/ became /yiqtal-/ through dissimilation in Northwest Semitic but not in Arabic. The rule must, therefore, be expanded to say that when the vowel of the second syllable was /a/, whether the form be G or Gt (i.e., when the first syllable is closed), dissimilation occurs. This would mean that the Š-stem /QTLa/ of strong roots may have been /šiqtala/ rather than /šaqtala/—that is, if it was not /šaqtala/ (see below, remark to pp. 596-99 [§74.624]).

— p. 519 (§74.232.1), p. 527 (§74.232.22). The contraction of {‘myḍtmr} to {‘ṃṭtmr} is most easily explained by assuming the presence of an /i/-vowel attached to the first element, viz., the 1 c.s. pronominal suffix, ‘my divine uncle has protected’ or ‘let my divine uncle

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<sup>973</sup>Curiously, the verb  $\text{MH}\text{Š}$ , used three times in parallel with  $\text{H}\text{ŠB}$  with both in the Gt stem (RS 2.[014]<sup>+</sup> ii 5-6, 23-24, 29-30 [KTU 1.3]), is described only as “iterative bzw. durative (intransitive) Nuance.”

<sup>974</sup>Below, p. 532, T. compares the “durative bzw. iterativ-habituelle Funktion” of the Ugaritic Gt with Akkadian. I would have preferred a discussion in terms of the West-Semitic verbal systems that are primarily aspectual.

<sup>975</sup>Pardee, *JNES* 60 (2001) 308 note 2.

<sup>976</sup>On p. 518, T. identifies the Arabic form as /yafta‘il-; on p. 519, he cites Arabic along with Ethiopic as showing /a/ as the stem vowel. It is the former that is correct.

protect' (/ʿammīyid̥tamir/ → /ʿammid̥tamir/ [the vowel that remains after the contraction is short because in a closed syllable] → /ʿammitt̥tamir/ [/d/ → /t/ by assimilation to the non-voicedness of /t/] → /ʿammitt̥tamru/ [with case vowel appropriate for a nominalized verbal form and syncope of the stem vowel])<sup>977</sup>; T. reconstructs the original form as /ʿammVyid̥tamVru/. The same must be said of the place name ʾIlišt̥amʿu, if T.'s reconstruction of the original form of the verbal element as from the /YQTL/ be correct (as it surely is, by analogy with {ʿmyd̥tmr}):<sup>978</sup> surely the most plausible reconstruction is /ʾilīyīšt̥amiʿ/, 'my god has heard' or 'let my god hear'; here T. reconstructs the nominal element as a nominative in the absolute state (/ʾiluyīšt̥amiʿ/ or /ʾiluyīšt̥amVʿi/).

— p. 519 (§74.231.1). T. is unwilling to decide on /i/ or /a/ as the stem vowel of the Gt-/YQTL/, choosing to consider the forms {yšt̥il} and {yšt̥al} as variants of a single form rather than as, respectively, Gt and tD (the latter with metathesis of the sibilant and the /t/), as Huehnergard has argued.<sup>979</sup> Unfortunately, the form with {i} is attested only once, as {tšt̥il} in RS 15.098:6 (*KTU* 2.17:15), where these five signs are all that is preserved of the line, and the interpretation may not, therefore, be considered certain, for the signs may represent two words (see above, remark to p. 183 [§33.243.11c]). Nonetheless, if one accept the interpretation of the signs as constituting a single form, surely Huehnergard's explanation deserves more attention than it gets here.<sup>980</sup>

— p. 519 (§74.231.1). Regarding syncope of the stem vowel discussed in the preceding remark, T. says only "Bei Antritt (lang-)vokalischer Endungen is mit einer Synkopierung ... zu rechnen (vgl. etwa den PN <sup>l</sup>a-mis-tam-ru)." The example of the personal name that he cites is clear (see second previous remark), but an equally clear counter-example comes from an actual text: in *tqtnšn* (RS 1.012:58 [*KTU* 1.23]), which T. vocalizes "/tiqtanVṣâ-/", the vowel between the first {n} and the {ṣ} seems not to have elided, for, if it had, the /n/ might be expected to have assimilated to the /ṣ/. The form *ystrn* (RS 2.[008]<sup>+</sup> vii 48 [*KTU* 1.4]), if indeed from √SRR and vocalized /yistarran(na)/, may not represent an example of inner-Ugaritic syncope, for the simple G-stem of geminate roots shows similar forms, in Ugaritic as in Hebrew (e.g., Hebrew /yāsōb/ ← /yasubbu/); by analogy, the Gt form may have been /yistarr-/.

— p. 520 (§74.232.21). T. reconstructs the Gt of √HLK as /yītalVk-/ or /yittalVk-/ ← /yihtalVk-/. Against this derivation are the facts that: (1) the G-stem forms in Hebrew are suppletive in that the /YQTL/ forms and the infinitive behave generally as though the root were I-y; (2) the tD in Hebrew is formed on the strong root with the /h/ unassimilated to the preceding /t/ (/yithallēk/). With these forms in mind and considering that the G-/YQTL/ in

<sup>977</sup> Bordreuil and Pardee, *Syria* 61 (1984) 13.

<sup>978</sup> Very correctly, T. rejects on p. 527 the attempt to interpret the {y} in the more archaic form as a *mater lectionis*.

<sup>979</sup> *UF* 17 (1986) 402.

<sup>980</sup> In my vocalizations of the ritual texts (e.g., *Les textes rituels* [2000] 98), I used the form /yitqatal-/. The apparent unanimity of the West-Semitic forms for /i/ as the stem syllable of the Gt-/YQTL/ (i.e., Arabic and Aramaic, the form has disappeared from Hebrew) leads me to believe that Huehnergard's view is to be preferred. See now Bordreuil and Pardee, *Manuel* (2004) I 67.

Ugaritic is {y<sub>l</sub>k}, not {y<sub>h</sub>l<sub>k</sub>}, surely the more plausible reconstruction of {y<sub>t</sub>l<sub>k</sub>} is to treat it either as based on a biconsonantal stem like the simple G-stem, viz., as simply /yitalik-/ (cf. {ā<sub>l</sub>k} /ʾalik-/, ‘I go’—see remark below to pp. 631-32 [§75.511e-g]), or, less plausibly perhaps, as a I-y form, viz., as deriving from /yiytalik-/. On T.’s view of the semantics of the form as “pluralische (iterative bzw. durative) Nuance,” see above, introductory remarks to this section (pp. 518-32).

— p. 522 (§74.232.21), p. 628 (§75.44), p. 677 (§75.671). T. analyzes *t<sub>l</sub>pp*, ‘she makes herself X’, in RS 2.[014]<sup>+</sup> iii 1, iv 45 (*KTU* 1.3) as Gt of NPP, a root that is unattested in Ugaritic and which in Arabic means ‘to sow’ (it would mean ‘she sprinkles herself with X’ in Ugaritic), without considering the possibility that it may be /qatal<sub>t</sub>al-/ with infixed -t/ of YPY, ‘to be beautiful’,<sup>981</sup> /t<sub>l</sub>tapêpi/ ← /tiytapaypiy/, ‘she beautifies herself’.

— pp. 522-23 (§74.232.21). Without proffering any arguments to the effect that the Gt could be used as what might be called a metaphorical reflexive (i.e., the subject of the verb could not literally have effected the act in question), T. classifies *y<sub>t</sub>š<sub>i</sub>* in RS 1.002 *passim* (*KTU* 1.40) as a reflexive. Since the subject of the verb is a slaughtered animal and the indirect object a series of divinities, the real-life meaning can only be passive, viz., the sacrifice does not ‘lift itself up’, rather it is ‘lifted up to = offered to’ the divinities in question by the offerer(s).<sup>982</sup> May this example from a prose text be considered, then, an early example of the Gt with a passive function? T. rejects this in principle (p. 532: “... eine passive Funktion des Gt-Stammes nirgendwo sicher nachzuweisen ist”), but, because he does not defend his classification of *y<sub>t</sub>š<sub>i</sub>* in RS 1.002, one cannot accept that assessment at face value. If the function of this form is indeed passive, it may be an indicator that the G-passive was disappearing from Ugaritic prose. T. accepts this very conclusion based on the relative frequency of the N-stem with passive value in prose as compared with poetry (p. 543 [§74.372]), but the absence of consideration of what category of reflexivity was expressed by the Gt has resulted in that conclusion not also being applied to the Gt-stem.

— p. 524 (§74.232.21). Because of the 2<sup>d</sup> person pronoun attached to the preposition in the phrase ‘*dn yš<sub>t</sub>āl* ‘*mnk* in the letter RS 29.095:10-11 (*KTU* 2.71), the verb can hardly be a jussive, ‘Let ‘DN make requests of you’, a possibility left open by T. It appears far more likely that the form is /YQTL-u/ expressive of repeated action: “DN has been continuously repeating his request to you ...”.<sup>983</sup>

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<sup>981</sup> Cf. del Olmo Lete and Sanmartín, *Diccionario* II (2000) 326-27.

<sup>982</sup> It is difficult to avoid the conclusion that, because T. does not discuss the type of act that is being expressed by the Ugaritic verbal form, he is in fact classifying it according to its translation value (surface reflexives being common in German) rather than according to its function in Ugaritic. Note that the same tendency might be expected of a grammarian whose native language is French (where, as in German, surface reflexives can express everything from stativity to passivity), but not of one whose native language is English, where the use of what I have referred to as “metaphorical” reflexives is much less common.

<sup>983</sup> Cf. Pardee, *Context* III (2002) 111.

— p. 526 (§74.232.21). In July of 2003, I was able to recollate the tablet to assess T.’s suggestion to read as {ù} what I previously took as {å}<sup>984</sup> at the beginning of RS 5.300:9 (*KTU* 1.71). This proposal appears to be correct. The two heads of wedges that I took as horizontals can in fact be just the upper portion of verticals (the heads of verticals in this hand slant down from left to right with the result that, if only the top-most part of the head remains, the form is similar to that of a horizontal wedge). The grammatical basis for the reading {[yḥr]ʿùl}, viz. that the following verbal form, *yṭn*, should not be preceded by a /QTLa/ form in a conditional/temporal clause, is thus borne out epigraphically.

— pp. 527-28 (§74.233). T.’s suggestion that the {ʾ} of the first syllable of the Gt-imperative ({iqtl}) might not have been realized in speech is not to be retained, for it is based on the analogy of the Arabic *alif al-waṣla* whereas there is no evidence of such a phenomenon in the other Northwest-Semitic languages. Compare especially the Hebrew Niphal and Hithpael imperatives, where the syllable-carrying consonant is the stronger /h/.

— pp. 532-43 (§74.3). As usual, T. places the discussion of the semantics of a verbal stem, here the N-stem, at the end of the section rather than at the beginning (pp. 542-43 [§74.37]). Here he recognizes the marked difference of semantic value between poetic and prose usage, the latter attesting far more strongly to a passive function of this verbal stem (contrast the case of the Gt-stem: see remark above to pp. 518-32 [§74.23]). This is particularly evident in the paragraph devoted to the /QTLa/ (pp. 532-35 [§74.32]), where the presence of the {n-} leaves no doubt as to the verbal stem unless the possibility exist of the root being I-n. T. refuses, however, to draw any explicit conclusion as to the original “Grundfunktion” of the N-stem. It appears plausible to draw at the very least the following conclusion for the West-Semitic languages: the primary function of the N-stem can hardly have been passive since all these languages show at least vestigial internal passives for each of the transitive stems. Therefore, the function of the N-stem must have been other and, judging from semantic distributions in Ugaritic as well as in the later Northwest-Semitic languages, a basically middle analysis appears likely (i.e., an act stated in terms of the patient but with no expressed passivity, e.g., ‘the door opens’ in English). T. never deals explicitly with the middle as a voice, though the notion seems to be present in his classification of many Gt and N forms as simply ‘intransitive’: his four principal semantic categories for these forms are intransitive, reflexive, reciprocal, and passive. He claims that the N-stem “dient im Ug. häufig zum Ausdruck reflexiver und reziproker Sachverhalte,” a statement that I find dubious given that the existence of what I have termed the “metaphorical” reflexive (see remarks above to the Gt and below to the N) remains to be proven in Ugaritic. If one be willing to admit that the West-Semitic languages had four principal verbal voices, active, middle, reflexive, and passive, then it is clearly the N-stem that expresses the middle, the t-stems the reflexive. Though principally intransitive, the t-stems include self-referential transitive usage (e.g., *yṭsp*, ‘he gathers in X for his own benefit’, in RS 2.003+ i 18 [*KTU* 1.14]) while the N-stem includes both a de-agentifying function for transitive verbs (hence its easy passage to a passive function when the internal passives began to disappear) and that of a

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<sup>984</sup> *Les textes hippatriques* (1985) 28, 29. T. had already suggested the reading {[...yḥr]ʿùl} in *AuOr* 16 (1998) 292.

strengthened intransitivity when the G-stem was already intransitive (e.g., *nʿr*, ‘to wake up’ ← ‘R, ‘to awake’). The fact that the t-stems were basically intransitive allows them also to take on the passive function, e.g., in Aramaic, after the loss of both the N-stem (very early) and the internal passives, and in Hebrew, after the loss of the Gt and the G-passive. (Biblical Hebrew marks a middle stage, with the N-stem functioning as both middle and passive, the Dt as primarily reflexive to both the G- and the D-stems, but also occasionally passive.) Finally, the question arises as to whether what I have called the ‘middle’ functions as a “voice” or as an *Aktionsart*.<sup>985</sup> I originally expressed these categories in terms of “voice”<sup>986</sup> in imitation of Greek, where there is a good deal of morphological overlap between middle and passive, and of the traditional grammars of Greek, where the middle is presented as a voice alongside the active and the passive. In favor of this classification is the fact that, in spite of the existence of three clear morphological classes (Ablaut for the passive, prefixed N for the N-stem, infixed or prefixed T for the t-stems), all express acts in terms of the patient rather than the agent. They thus seem to cover a gamut of patient-oriented acts that go from passivity to reflexivity rather than *Aktionsarten* belonging to fundamentally different categories.

— p. 533 (§74.32), p. 540 (§74.35), p. 799 (§83.231b). T. takes *nḥtū* in RS 4.475:8 (*KTU* 2.10) as 3 m.pl. /QTLa/ of the N-stem, the same form in line 10 as a m.s. participle of the same stem. I agree with T.’s parsing of the first occurrence but observe that he does not explain why the verb, which is preceded by two personal names, is not in the dual. I explain this apparent anomaly as an expression of the fact that the reference is not to these two individuals, but to them and their men: all were defeated, not just the two leaders.<sup>987</sup> I disagree with the parsing of *nḥtū* in line 10 as a participle, because I do not see who would be the m.s. subject of the form;<sup>988</sup> T. bases his decision on the fact that the verb is negativized by *in*- rather than by *l*, but makes no attempt to determine to whom the author of the letter would have been referring. The problem is well illustrated by his own translations: on p. 540 one finds “falls er(?) doch nicht geschlagen wurde,” on p. 799, “falls sie(?) doch nicht geschlagen wurden.” It appears much more plausible to see this use of the negative marker *in*, which appears here in the expanded form *inmm*, i.e., with repeated enclitic *-m*, as a very emphatic one here used with a finite form of the verb.

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<sup>985</sup> On the basic view that *Aktionsart* was expressed by the binyanim in Biblical Hebrew, see S. Creason, “Semantic Classes of Hebrew Verbs: A Study of *Aktionsart* in the Hebrew Verbal System” (dissertation University of Chicago, 1995). This is also T.’s classification of the factitive and causative forms in Ugaritic (p. 542 [§74.371]).

<sup>986</sup> *The Semitic Languages* (1997) 138. T. identifies three classes of “Diathese”: active, passive, and reflexive (p. 423 [§71.2]). On p. 542 (§74.371), he places the N-stem in the last category, though he adds that it may have an “ingressive bzw. inchoative Funktion” which he does not attempt to fix within his three basic categories. I would differ from his basic view, therefore, in ascribing to the N-stem a “middle” function that is distinct from both the passive and the reflexive/reciprocal.

<sup>987</sup> *Context III* (2002) 108 n. 151.

<sup>988</sup> Cf. the vocalized text and translation in *Pope* (1987) 66 and the more idiomatic translation in *Context III* (2002) 108; a full new treatment in French is now available in Bordreuil and Pardee, *Manuel* (2004), text 21 in the *Choix de textes*.

— p. 533 (§74.32), p. 639 (§75.517c). On p. 533, T. lists one certain example of the N-/QTLa/ of YTN, ‘to give’ (RS 16.179:1 [KTU 4.219]), asks the reader to compare another possible example in a badly damaged text (RS 19.174A<sup>+</sup>:4 [KTU 4.669]), and disputes the reading of {ntn} and hence the interpretation as a N-stem verbal form in a third (RS 17.124:3 [KTU 4.274]—he suggests reading {ātn} here). On p. 639, all three of these texts are cited as certain examples of the form. The second example must be classified as unanalyzable because of the state of the text and the third as dubious because of the uncertainty of the meaning and/or reading; even the first example is problematic for syntactic reasons (*ntn ksp* is supposed to mean ‘given for money’), though I know of no better explanation of the what appears rather clearly to be a verbal form spelled {ntn}. Finally, the form is vocalized “*nâtana*” on the assumption that it is built directly off the I-y/w root—on the dubiety of this assumption and the inconsistency in T.’s reconstruction of the form, see below, remark to p. 635 (§75.512), etc.

— p. 534 (§74.32), p. 758 (§82.12), p. 906 (§97.71a). The reading {gZR} is certain in RS 2.002:63 (KTU 1.23), and there is no possibility of an “alt.” reading {hZR} unless it be produced by emendation.

— p. 534 (§74.32). Because the N-stem of verbs of movement does not normally express a true passive (that is done by the passive of the causative stem: G-stem ‘enter’, causative ‘cause to enter’, causative-passive ‘be brought in’) and because the N-stem is not primarily a passive stem in any case, one may doubt that the nuance of *nʿrb* in RS 11.858:45 (KTU 4.103) is simply passive, as T. classifies it. The reference is to fields passing from the territorial prerogatives of one hamlet to those of another, and one might think that both the active and the passive nuances were being avoided in favor of the middle and that the meaning is closer to English ‘go over to’.

— p. 534 (§74.32). That the verb *nškh* in RS 18.031:15 (KTU 2.38) is 3 m.pl., not 3 m.s. as T. holds, is shown by the plural suffixes in the following lines.<sup>989</sup> That the N-stem expresses a reflexive, not a passive (a possibility allowed by T.), must be deduced from the fact that no one found them in the storm. The idiom seems to belong to the same semantic field as *nimṣāʾ* in Hebrew or *se trouver* in French, i.e., ‘they were’ in the storm, i.e., this reflexive may be an example of the category of “metaphorical” reflexives discussed above (they were not looking for themselves and happened to find themselves in a storm, rather their location was in a storm). The text is a letter, which leads to the conclusion that the N-stem was perhaps more expressive of the reflexive in prose than in poetry.

— p. 535 (§74.32), p. 627 (§75.42), p. 669 (§75.537d), p. 854 (§92.234b). On what I consider to be a false analysis of *ndbh*, *ntʿy*, and *nkt* in RS 1.002 *passim* (KTU 1.40), i.e., as 1 c.pl. G-/YQTL/ forms rather than as 3 m.s. N-stem, see remark above to pp. 211-13 (§41.13), etc.

— pp. 535-36 (§74.331). As is shown by the lack of evidence for the *ʾalif*-signs representing the vowel that precedes the consonant and, for the particular example of *ʾspī*, by other orthographies when a vowel follows the {ʾ} (i.e., {ʾspū} and {ʾspā}), it is highly

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<sup>989</sup>Pardee, *Context III* (2002) 94 n. 43.

unlikely that the second {i} of *ṣpī* tells us anything about the quality of the stem vowel of the N-/YQTL/.

— p. 536 (§74.333). If *bnš* in the phrase *bnš tṗnr* RS 9.453:28 (*KTU* 4.44) means “Personal” as T. translates, then the verb *yāḥd* in that same line must be plural (if *bnš* is indeed the subject rather than *sp*), because *bnš* would be a plural construct form (normally in these administrative texts *bnš* denotes a single person not a collectivity; the plural *bnšm*, written *bnš* in the construct state, is used for a plurality of persons). In prose, of course, one would expect the 3 m.pl. to have a *t*-prefix.

— p. 537 (§74.333). T. interprets the *l* of *w mlk ynšl l t’y* (RS 19.013:22-23 [*KTU* 1.90:21-22]) as ablative and *t’y* as a common noun (“Dann darf sich der König von der Opfertätigkeit zurückziehen/ist der König von der Opfertätigkeit entbunden”) without mentioning the possibility of taking the *l t’y* as an infinitival purpose phrase (“will move away to perform the *t’y*-sacrifice”).<sup>990</sup>

— pp. 537-38 (§74.333). Proving that even the best can make what I call silly mistakes (i.e., cases where one knows better but writes something else), T. vocalizes three /YQTL/ dual forms from RS 2.[009]+ vi 17-20 (*KTU* 1.6) as plurals, viz., with the ending /-ūna/ instead of /-āna/ or /-āni/ (T. would indicate /-āni/—see above, remark to p. 210 [§§41.112.8-9], etc.): *ymšḥn*, *ynghn*, and *yntkn*. As for T.’s semantic classification of the acts in question as reciprocal, there can be no doubt that the term properly describes the real-life situation, where two deities are doing battle and inflicting various injuries on each other. On the other hand, if the acts were expressed individually, the semantic category would have to be passive, not reflexive (i.e., each is struck by the other, not by himself), and one must wonder whether the reciprocal is really a primary category of the Ugaritic N-stem or only a by-product of its basic middle or secondary passive function.

— p. 538 (§74.333). The classification of *tnṭkn ṁdm’tḥ* (RS 2.003 i 28 [*KTU* 1.14]) as reflexive fits German fine (“Seine Tränen ergossen sich”) but not English (the tears do not ‘pour themselves out’). The classification as a simple middle (‘the tears pour forth’) appears preferable, unless it be established that Ugaritic made regular use of a “metaphorical” reflexive (see discussion above, remark to pp. 522-23 [§74.232.21]).

— p. 538 (§74.333). Again T. classifies a Ugaritic verbal usage on the basis of a German reflexive translation: *ynphy* in RIH 78/14:12’ (*CAT* 1.163) is so classified on the basis of the German “sich zeigen, erscheinen” (Hebrew *nir’āḥ* is compared). Because, however, the form does not mean literally ‘the moon sees itself’<sup>991</sup> three times in a month’, and because this is a prose text, surely the interpretation as a passive is the more likely: the moon does not ‘see itself’, it ‘is seen’ by observers on earth. The passive function is surely at the origin of *nir’āḥ* in Hebrew as well (‘X is seen’, not ‘X sees itself’), though I would be the last to impugn the validity of the translation by ‘to appear’.

— p. 538 (§74.333), p. 660 (§75.532). However one may solve the problem of the /YQTL/ verbal forms in the “para-mythological” texts, can it be judged likely that *tṗlg* in line 69 of RS

<sup>990</sup>Idem, *Les textes rituels* (2000) 481, 487-88; idem, *Ritual and Cult* (2002) 74.

<sup>991</sup>Ugaritic PHY means ‘to see’, not ‘to show’, and T.’s literal translation “sich zeigen” is hardly apposite for his interpretation.

24.244 (*KTU* 1.100) is a /YQTLØ/ perfective (p. 538) while *tlû* in the preceding line but the same poetic unit<sup>992</sup> is a /YQTLu/ form (p. 660, with a question mark)?<sup>993</sup> As to the semantic value of the N-form, again the reflexive notion assumed by T. is dubious: the poison does not ‘split itself up into streamlets’, it simply ‘splits into streamlets’, i.e., the category is that of the middle—the poison itself is not the real-life agent acting upon itself, it is the divine agent, *Horânu*, or the magical devices that the latter has put to use, that actually effect(s) the division of the poison into harmless rivulets.<sup>994</sup>

— p. 539 (§74.333), p. 681 (§75.74a). T. parses the two tokens of {yprsh} in RS 3.367 iv 22' and 25' (*KTU* 1.2) as identical in form (/yipparsiḥ/), but different in meaning: the first would be a jussive, the second a /YQTLØ/ perfective. It appears more likely, however, that the two verbs *yprsh* and *yql* (which appear in a verse, lines 22'-23', for which there is no equivalent in the first battle scene), are in their first appearance either /YQTLu/ forms expressing Kothar's view of what will happen<sup>995</sup> or else /YQTLa/ forms expressing purpose clauses. Thus the distinction between the command unit and the fulfillment unit would have been more clearly marked than if the forms were formally identical, viz., /yipparsiḥu ... yaqīlu/ and /yipparsiḥ ... yaqil/. As to the semantic value of the N-stem (this parsing, based essentially on Akkadian *napalsuḥu*,<sup>996</sup> is probable though uncertain—on p. 681, the alternative parsing as G is presented, viz., /yuparsiḥ/) as a reflexive, the translation “sich hinhocken, sich hinfallen lassen” fits German, but neither English (in the battle between *Ba'lu* and *Yammu*, the latter does not ‘let himself fall’,<sup>997</sup> he falls because smitten by *Ba'lu*'s weapon) nor Ugaritic, where the voice is more simply middle, i.e., ‘he falls, collapses, crumples’.

— p. 539 (§74.333). It appears clear that at the beginning of RS 2.[008]+ viii (*KTU* 1.4) *Ba'lu* is speaking to his two messengers, *Gapnu-wa-ʾUgāru*, and T.'s parsing of *tspr* in line 8 as a plural is thus incomprehensible. Because on p. 621 (§75.233), p. 635 (§75.512), and p. 636 (§75.513), the preceding and following forms (*šá*, *rd*, and *ttn*) are all parsed as duals, the parsing of *tspr* as a plural probably represents a simple oversight.

— p. 540 (§74.342). Given the other ʾalif-preformative forms of derived stems in Ugaritic (in particular the Gt-/QTLa/), T. reluctance to accept that the N-stem imperative was so formed is to me incomprehensible—especially when he clearly prefers the analysis of the forms attested with {i}-preformative as G-stem imperatives, for which there is no parallel in

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<sup>992</sup>Pardee, *Les textes para-mythologiques* (1988) 202.

<sup>993</sup>On this latter form, see below, remarks to p. 656 (§75.531e) and to pp. 700-1 (§76.427a).

<sup>994</sup>Pardee, *Les textes para-mythologiques* (1988) 203, 218-19.

<sup>995</sup>Pardee, *Context I* (1997) 249.

<sup>996</sup>Cf. Orin D. Gensler, “Reconstructing Quadriliteral Verb Inflection: Ethiopic, Akkadian, Proto-Semitic,” *JSS* 42 (1997) 229-57, esp. p. 252.

<sup>997</sup>As to the semantic classification of the N-stem as frequently expressing the reflexive, one will note the occasional recourse by T., as here, to translations which include “lassen,” hardly an acceptable representation, since such a notion is, strictly speaking, a causative reflexive, hardly germane for the N-stem.

any of the Northwest-Semitic languages (see remarks above to p. 170 [§33.211.2], etc., p. 195 [§33.322.2c], etc., p. 202 [§33.432a], etc., p. 426 [§73.122]).

— pp. 540-41 (§74.35). On what basis is it considered equally possible that the N-participle may have had a long vowel in the second syllable (the notation is as *anceps*, viz. “*naqtāl*”)? Both Arabic and Akkadian have a *m*-preformative form with or reflecting a short vowel in the third syllable (Arab. *munfa‘il*-, Akk. *mupparsum* ← /munparVsum/ [cf. above, remark to p. 269, §51.45q-s]) and the N-stem has for the most part disappeared from Aramaic. This leaves only Hebrew as an immediate point of comparison and there the proto-form certainly had short /a/, for the *qameṣ* in the Massoretic notation system reflects proto-Hebrew /a/. It is worth pointing out in passing that the participle without *m*-preformative constitutes an important isogloss between Ugaritic and Hebrew (and with Phoenician as well, where the N-participle certainly does not show *m*-preformative).

— p. 540 (§74.35), p. 673 (§75.62), p. 677 (§75.672). The one form that made me hesitate about deriving the verb MR, that occurs in parallel with BRK, ‘to bless’, from a root MRR, viz., the putative {nmrrt} in RS 3.322+ iv 33 (*KTU* 1.19),<sup>998</sup> is stated in the first section cited here to be “wahrscheinlicher” read as {nmrrt} and that assessment is said to be “nach Kollation.”<sup>999</sup> It is not without irony that T. forges ahead with the traditional etymology from √MRR when he has himself removed its only explicit basis in Ugaritic. The form in question having disappeared from the scene, it appears all the more necessary to divorce this Ugaritic verb from √MRR, ‘to be bitter’, a basic point that I attempted to argue in the article cited in note 998. T. translates *tmrn ālk nmrrt* “sie sollen mich fürwahr mit Segen stärken, auf daß ich als Gestärkte gehen kann,” showing that he has admitted all the spurious arguments for the etymology by √MRR, ‘to be bitter’, viz. that it would come to mean ‘to be strong’, without explicit refutation of my arguments to the contrary (too detailed to repeat here—see the article cited in note 998). A far more plausible etymological point of contact is Arabic MYR, ‘to supply with provisions’, one that I did not consider in my study of the root MRR. The act of blessing in Ugaritic is regularly ascribed to a deity, and the act in question appears, therefore, to be the production of a concrete blessing (*benefacere*), rather than the abstract pronouncing of a blessing (*benedicere*); the Arabic verb MYR may be said to lead the interpretation of the Ugaritic verb MR in that direction if the form {nmrrt} is indeed disposed of. (For a summary of the various verbal roots containing MR in Ugaritic, see remark below to p. 673 [§75.62a], etc.).

— p. 541 (§74.35), p. 909 (§97.91). T. takes the subject of *nṯkp* in RS 4.475:14 (*KTU* 2.10) to be the noun in the following sequence *hm nṯkp m‘nk*, “falls dein Gegenangriff (w.: deine Antwort) zurückgeworfen/abgewehrt wird”) without even considering the possibility—I would consider it a probability—that *hm nṯkp* is itself the entire protasis, that the previously mentioned entities are the subject of the verb, and that *m‘nk* constitutes the first word of the apodosis, itself a ‘return-of-news formula’. The better translation is: “If they have been overcome, your reply and whatever (else) you may hear there put in a letter to me” (lines

<sup>998</sup>Pardee, “The Semitic Root *mrr* and the Etymology of Ugaritic *mr(r)* // *brk*,” *UF* 10 (1978) 249-88.

<sup>999</sup>In 1996, before collation, T. had proposed to read {*tmrn* . āl[kn] . m[rtm] } (*AfO* 42-43, p. 270), after collation *tmrn ālk nmrrt* (here, p. 540, with no indication of word-dividers).

14-19).<sup>1000</sup> This syntactic analysis affects the analysis of the form *ntkp*, for, if this form refers to a previously mentioned entity, it is almost certainly a finite form, not a participle, as T. analyzes it on p. 541; if that entity is plural, then the form can only be finite (the participle would be *ntkpm*).

— pp. 544-67 (§74.41). As we have seen to be the case with other verbal stems, T. discusses the semantics of the D-stem at the end of the section (pp. 566-67 [§74.417.2]), rather than at the beginning. He correctly identifies the basic function of the D-stem as intensive, with various nuances according to the semantic category of the base form, whether that be stative, fientive/intransitive, or fientive/transitive. I disagree with him strongly, however, in his frequent classification of forms and in his summary description of one function of the D-stem as expressing “entweder eine Intensivierung des Sachverhalts oder eine Kausative Aktionsart” (p. 566); on p. 567, he asserts that in this latter function the D-stem overlaps with the Š-stem (this perspective is assumed below in the section on the semantics of the Š-stem, pp. 603-4 [p. 602, §74.627]). T. has apparently been led astray here by the problem of translation, for, in German as in the other European languages with which I am acquainted, the distinction between factitive and causative is not built into the verbal system as it is in the West-Semitic languages. It is this distinction that explains why Ugaritic uses both the D- and the Š-stems for the roots ŠQY, ‘to drink’, and LHM, ‘to eat’, the latter meaning ‘to cause to drink/eat’, the former having what is for us an identical meaning but apparently a distinguishable meaning for speakers of Ugaritic (T., p. 557, does not recognize this distinction for ŠQY; previously, he has not recognized it for LHM<sup>1001</sup>). In this context, it must be remarked that the presence of the noun *šqym* (or even *šqyt*) in RS 18.041:24, 25 (*KTU* 1.86) does not disprove the existence in Ugaritic of the D-stem for that root, for there is no proof that it means ‘libation-servers’<sup>1002</sup> nor, if that is the meaning, that it is the G-stem participle, i.e., ‘he/she who gives to drink’ (it could be a /qattāl/ *nomen agentis*, ‘he who carries out a function having to do with drink’). Furthermore, a grammatically coherent interpretation of RS 2.[004], col. i (*KTU* 1.17), where non-Š-preformative verbal forms of both roots appear several times, according to which *Dānī’ilu* would ‘eat’ and ‘drink’, rather than ‘giving eat and drink to the gods’, appears to me to be out of the question.<sup>1003</sup> I find two aspects of this approach particularly remarkable: (1) that in a work so theoretically oriented, the problem with a major area of overlap in expression of *Aktionsart* between two of the principal verbal stems would not have been addressed; (2) that the overlap is explicitly identified as occurring in cases where the corresponding G-stem is transitive (p. 567), for, in Hebrew, there are very few D-stem verbs that take anything approaching the “double-accusative” structure common with the causative stem (the only one that comes to mind is LMD, ‘to teach’) whereas, on the other hand, the greatest area of apparent overlap in

<sup>1000</sup>Pardee, *Context* III (2002) 108; see now Bordreuil and Pardee, *Manuel* (2004), text 21 in the *Choix de textes*.

<sup>1001</sup>Cf. Pardee, *Context* I (1997) 343 n. 2.

<sup>1002</sup>Idem, *Les textes rituels* (2000) 467.

<sup>1003</sup>Idem, *Context* I (1997) 343-44. It is always possible that in Ugaritic, as in Arabic, ŠQY was transitive in the G-stem, a possibility that T. appears to prefer on p. 557.

function in Hebrew occurs in cases where the G-stem is stative (the distinction between *qiddēš*, ‘to make holy’, and *hiqdīš*, ‘to cause to be holy’ is not immediately obvious to speakers of most of the modern languages of scholarship).

— pp. 544-46 (§§74.412.1-16). On T.’s decision to reconstruct the D-stem /YQTL/ form as /yuqattil-/, on the basis of comparative data, rather than /yaqattil-/, on the basis of the Ugaritic 1 c.s. form, which is written {āqtl}, hence /ʾaqattil-/, see above, introduction, general remark on vocalization. It may be added here that T.’s view that the retention of the /w/ in the D-stem of I-w roots (e.g., *ywpṭn* in RS 2.[008]<sup>+</sup> iii 13' [KTU 1.4]) is owing to the preceding /u/ vowel assumes that the triphthong /awa/ is somehow problematic, which is not necessarily the case as is proven by *ātw*, ‘she arrived’ /ʾatawat/ (RS 2.[008]<sup>+</sup> iv 32' [KTU 1.4]). (On this form, see above, remark to p. 172 [§33.213.1b], etc.) Furthermore, the explanation of the 1 c.s. form /ʾaqattal-/ as secondarily derived from /\*ʾuqattal-/ because of the consonant /ʾ/ should have been expressed in terms of a general phonetic rule. That would, however, have been difficult for if anything /ʾ/ is more common with /u/ in Ugaritic than in the other Northwest-Semitic languages (compare Ugaritic *ʾšb*<sup>c</sup> /ʾuṣba<sup>c</sup>u/ with Hebrew *ʾeṣbā*<sup>c</sup>, Ugaritic *ʾum* /ʾummu/ with Hebrew *ʾēm*, Ugaritic *ʾdm* *t* /ʾudma<sup>c</sup>ātu/, with Hebrew *d<sup>mā</sup>ʾōt*, etc.).

— p. 545 (§74.412.14), p. 549 (§74.412.23), p. 639 (§75.517d). Despite some surface damage and the apparent re-impression by the scribe of one of its wedges, I consider the {w} of {twḥln} in RS 15.008:12 (KTU 2.16) to be certain.<sup>1004</sup> As T. observes (p. 549), the new reading {tdḥln} proposed in CAT is epigraphically unacceptable,<sup>1005</sup> but he considers {trḥln} to be a possible reading. In my estimation, that “reading” must be termed an emendation and, as, RHL offers nothing better than WHL for the interpretation of the text, there is no reason to adopt the emendation. As for the form of {twḥln}, I see no reason to prefer the D-stem (T.’s analysis) over the N-stem. Though there are no clear examples of the N-stem from I-w roots,<sup>1006</sup> there is no particular reason to doubt that the /YQTL/ would have had the basic form /yiwatil-/.<sup>1007</sup> Taking the form as D-stem, T. is constrained to see it as irregularly intransitive. So radical a solution appears unnecessary.

<sup>1004</sup>Pardee, *AfO* 31 (1984) 220. Since preparing the study just cited, I have re-collated and copied the text in view of its inclusion in the *Manuel d’ougaritique* that Bordreuil and I have produced (2004), text 24 in the *Choix de textes*, and for *Les textes épistolaires* (in preparation); this restudy has left no doubt in my mind about the correctness of the reading {w}.

<sup>1005</sup>He diplomatically terms {twḥln} “wahrscheinlicher als” {tdḥln}, though the sign in question cannot possibly have ever been {d}.

<sup>1006</sup>In the appropriate section (pp. 536-39 [§74.333]), T. cites no clear examples. He considers {ynp<sup>c</sup>} in RS 3.322<sup>+</sup> ii 16 (KTU 1.19) to be an N-stem from YP<sup>c</sup> (p. 537), but that analysis may not be considered certain: it is in parallel with {yp<sup>c</sup>} and there is no particular reason to expect a N-stem form here. If it is N-stem, similar phenomena are attested where a I-y/w root is not involved (e.g., {ynphy} in RIH 78/14:12' [CAT 1.163:5]). It could, in any case, be a secondary form built off of a root I-y or from a parallel root NP<sup>c</sup> (T. is a bit harsh in referring to this option as making appeal to a “Phantomwurzel √np<sup>c</sup>,” for by-forms of weak roots abound in the Semitic languages.) Whatever the case may be, more than one example is needed to establish the category and to demonstrate that original I-y and I-w roots behaved identically.

<sup>1007</sup>The D-stem shows /w/ (cf. p. 195 [§33.322.1b]), and the N-stem may, therefore, have done the same.

— pp. 546-48 (§74.412.21). In addition to the remarks offered above in the tenth general remark on T.'s multiple listing of verbal forms from broken passages, with special mention there of D-stem forms listed here, it may be remarked that T. leaves open the possibility in several of these cases of analyzing a form spelled {yâCC} as G-stem. For that to be accepted, one must also accept that the /ʔ/ had quiesced in these forms and that {â} is used as a *mater lectionis* for the resultant vowel /â/ (e.g., {yâbd}, 'he will perish', would be /yâbVd-/ ← /yaʔbVd-/). As noted above, I am loath to accept such reconstructions.

— p. 546 (§74.412.21). In his discussion of the semantics of *yâsp* in RS 24.251:36' (*KTU* 1.107:11; *CAT* 1.107:36) as a D-stem of which the function is emphasis ("etwa mit gesteigerter Bedeutung gegenüber G"), T. might have pointed out that this is the only D-stem form in a section in which the verb is repeated at least a dozen times in the G-stem and that it is the only case in this passage where the subject is plausibly plural. T. cites the passage as "x[xx]hm yâsp hmt," which he translates "Ihr(e) ... (?) möge das Gift (völlig) tilgen," with no reference to my reading {ʔil[l]hm . yâsp [.] hmt} and interpretation as 'may (all) the divinities gather the venom'.<sup>1008</sup> Since one aspect of D-stem semantics has been claimed in the past to be plurality, this example would appear to have been a rather obvious case for discussion. On the other hand, the present of a *y*-preformative plural in this "para-mythological" text cannot be considered likely. This observation leads to the conclusion that, if the reading {ʔil[l]hm} is correct, that form is either singular + enclitic-*m* or dual.

— p. 547 (§74.412.21), p. 565 (§74.416.4), p. 613 (§75.212.13), p. 615 (§75.212.5), p. 615 (§75.216). T. parses the finite form in the phrase *âl tâpq âpq* (RIH 78/20:12 [*CAT* 1.169]) as D-stem because *tâpq*, with {â}, appears to him to be a D-/YQTL/ form. *âpq* would, in that case, be an anomalous D-infinitive, however, for the standard form is apparently /quttal/. On p. 613, he says that *tâpq* may theoretically be G-/YQTL/, while on p. 615 (§75.212.5) he lists *âpq* among examples of the G-infinitive. He does not mention the possibility that *âpq* may be a common noun, which opens up the further possibility of taking *tâpq* as N-stem.<sup>1009</sup> It is remarkable that T. does not even mention this interpretation when he himself proposes various grammatical analyses but never a translation.

— p. 547 (§74.412.21). As noted above in the remark to {yrgbbʿ1} (p. 32 [§21.31]), the divine names in RS 24.246:15-28 (*KTU* 1.102) are not /YQTLØ/ forms but /YQTLu/. {yârš} in lines 18 and 24, which reappears in RS 24.250<sup>+</sup>:4 (*KTU* 1.106) should not, therefore, be parsed as "PK<sup>K</sup>i" as it is here.<sup>1010</sup>

— p. 547 (§74.412.21). The reading of the proper name {[n]iršn} in RS 11.774:17 (*KTU* 4.77), here qualified as a new reading, was first proposed in 1997 by Tropper and Vita.<sup>1011</sup> I have subsequently collated the tablet and concur with their judgment that the comparison of

<sup>1008</sup>*Les textes para-mythologiques* (1988) 238, 242, 248, 251; *Ritual and Cult* (2002) 181, 183.

<sup>1009</sup>Pardee, in *Verse in Ancient Near Eastern Prose* (1993) 212; idem, *Les textes rituels* (2000) 877, 888-89; idem, *Ritual and Cult* (2002) 160; Bordreuil and Pardee, *Manuel* (2004) II 67, 68, 148. This analysis of the form, though with a different contextual interpretation, has recently been accepted by Ford (*UF* 34 [2002] 188-89).

<sup>1010</sup>Pardee, *Les textes rituels* (2000) 521, 522-24; idem, *Ritual and Cult* (2002) 20.

<sup>1011</sup>*UF* 29, p. 680.

this line with previous lines indicates that an additional sign belonging to the name must have fallen in the lacuna.

— p. 550 (§74.412.24), p. 627 (§75.42). Granting that the context is damaged, one must nevertheless judge it more likely that *ʾnšq* in RS 3.367 iv 4' (*KTU* 1.2) is from NŠQ, 'to burn',<sup>1012</sup> than from NŠQ, 'to kiss', for there are other 1 c.s. forms preserved in the passage that have a negative rather than a positive semantic polarization (see remark above to p. 449 [§73.243.21], etc.).

— p. 550 (§74.412.24), p. 565 (§74.416.3). If the previous point be granted, then there is no specific Ugaritic datum in favor of NŠQ, 'to kiss', being primarily a D-stem in that language (in Hebrew, G-stem usages predominate).

— p. 551 (§74.412.26), p. 864 (§93.33b), p. 869 (§93.362.1), p. 871 (§93.421). T. holds that the D-stem of QRB in the phrase *ʾqrbk ʾbh b'ʾl*, 'I will bring you to her father *Ba'lu*' (RS 5.194:27 [*KTU* 1.24]), takes the so-called double-accusative complementation ('I will cause you to approach her father *Ba'lu*'). Since, however, QRB was certainly stative in the G-stem, the D-stem will have been factitive, not causative, and *ʾbh* is better analyzed as an adverbial accusative, rather than as a direct object, lit. 'I will make you be near to her father *Ba'lu*' (even more literally, 'I will produce in you a state of nearness unto her father *Ba'lu*'). (On the need to maintain the distinction between these two grammatical categories of the accusative, see remark below to p. 864 [§93.33c]).

— p. 552 (§74.412.27), p. 565 (§74.416.3). Apparently linked to his analysis of √NŠQ as appearing primarily in the D-stem (see second previous remark), T. proposes that √HBQ in the G-stem means 'to embrace' in the basic sense of 'to put one's arms around someone' while 'to embrace sexually' would be D-stem. It is, of course, possible that both nuances are expressed by the G-stem. In the Ugaritic consonantal orthography, only the attestation of a participle would permit certainty on this point (the G-stem participle would be {ḥbq}, that of the D-stem {mḥbq}).

— p. 553 (§74.412.27), p. 578 (§74.511b), p. 677 (§75.673). On p. 553, the verb in the phrase *ʾl yʾll*, 'dew forms' (RS 3.322+ i 41 [*KTU* 1.19]), is parsed as D-stem, on pp. 578 and 677 as L-stem; the two analyses are not cross-referenced and no sign of doubt is indicated anywhere. On the basis of the proposal made below (remarks to pp. 575-76 [§74.50] and to pp. 577-78, 678-79 [§74.511a, b], etc.) that the semantics of the D-stem of geminate roots would have been distinctive from those of the L-stem, this is better classified as L-stem, because it is intensive intransitive, not factitive.

— p. 554 (§74.412.27), p. 722 (§77.322b), cf. p. 317 (§54.221a), p. 742 (§81.21a), p. 912 (§97.10.1b). In the first two sections cited, one or both of the verbal forms *tmḥš* and *tšmt* in RS 3.367 iv 9' (*KTU* 1.2) are parsed as jussives; in the others they are so translated. Because, however, the speaker in the passage is the deity *Kôtaru-wa-Ḥasīsu*, there is no reason to believe that he would be expressing a wish. Is it not more likely that he is describing what will happen as a reality, viz., as an 'indicative' imperfective form (/YQTLu/, not /YQTLØ/)?

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<sup>1012</sup>Caquot and Sznycer, *Textes ougaritiques* I (1974) 135 note i.

— p. 555 (§74.412.27). T. follows Dietrich and Loretz<sup>1013</sup> in taking *yšlm* in the phrase *mlkn yšlm l ibh* (RS 24.247:54' [KTU 1.103+]), “the king will make peace with his enemy,”<sup>1014</sup> as D-stem. The prepositional complementation, among other arguments, leaves open the possibility, however, that the form is G and that a more literal translation would be ‘the king will be at peace with his enemies.’<sup>1015</sup>

— p. 555 (§74.412.27), p. 812 (§85.8b). T.’s primary interpretation of RS 24.255:22-23 (KTU 1.111:23-24) involves gender incongruence (in *mt h tšlm* ‘In, the noun *mt*, ‘death’, normally masculine, would be the subject of *tšlm*) while his second makes appeal to an unstated subject (‘the gods’ would be subject of the verb). Finding these difficulties to be insurmountable, I have suggested that the horizontal divider that precedes line 22 was misplaced and that the final two signs of line 21 are to be read with the first three of line 23 to give the word *bhmth*: {bh(22)mth . l tšlm . (23) l ln .}, “... (from) her (22) own cattle, she is not required to repay anything on this account.”<sup>1016</sup>

— p. 557 (§74.413.1), p. 561 (§74.414.3), p. 615 (§75.216), p. 639 (§75.517d), p. 669 (§75.537d). T. does not explain why he prefers to interpret *abd* and *ydy* in RS 24.244:5 *et passim* (KTU 1.100) as /QTLa/ optatives or futures (!) rather than as the imperatives that most scholars have seen here.<sup>1017</sup> The case for the /QTLa/ expressing either the optative or the future in Ugaritic cannot be said to be a strong one (cf. remark below to pp. 716-17 [§76.535a-c], p. 727 [§77.35]).

— pp. 558-59 (§74.414.1), cf. p. 464 (§73.331.1). Whatever one may think of Huehnergard’s hypothesis according to which the proto-West-Semitic D-stem /QTLa/ form was /qattil/, positing on the sole basis of {ša-li-ma}, repeated several times in RS 20.012, that such was the case in Ugaritic must be considered extremely tenuous, for there is no particular reason why that form in that text should mean “er hat bezahlt” (p. 464).<sup>1018</sup> Previously, direct data for the identification of either the first or the second vowel of the Ugaritic form have been absent. In this matter, it may be remarked that the base form in proto-Hebrew must have been /qittil/ or /qittil/ because I-guttural forms show only /i/ in the

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<sup>1013</sup>Mantik (1990) 151.

<sup>1014</sup>Pardee, *Ritual and Cult* (2002) 140.

<sup>1015</sup>Idem, *Les textes rituels* (2000) 562.

<sup>1016</sup>Pardee, *Ritual and Cult* (2002) 93; for the arguments, see idem, *Les textes rituels* (2000) 622-29.

<sup>1017</sup>See my re-edition in *Les textes para-mythologiques* (1988) 193-26. I take this opportunity to point out that the imperative /di/ for YDY in Arabic indicates that my vocalization of the imperative as /yadaya/ (ibid., p. 201; cf. T. p. 561) was probably erroneous. The Arabic would indicate /yidiya/ (so now Bordreuil and Pardee in *Manuel* [2004] II 41-43). Or the volitive form with the initial /y/ retained may indicate that the form is in fact the infinitive with the function of the imperative, viz., /yadāyu/.

<sup>1018</sup>This interpretation was taken from Huehnergard, *Ugaritic Vocabulary* (1987) 182, 321 (the text was originally published by Nougayrol as his text 96 in *Ugaritica* V). In his argument for the proto-Hebrew form being /qittil/, Huehnergard assumes /qattil/ for Ugaritic and hence for proto-West Semitic (“Historical Phonology and the Hebrew Piel,” pp. 209-29 in *Linguistics and Biblical Hebrew* [ed. W. R. Bodine; Winona Lake: Eisenbrauns, 1992]). As we shall see below, it appears to me that this view of the Ugaritic situation is incorrect and that it has led both Huehnergard and Tropper to an incorrect reconstruction of the proto-Hebrew process.

first syllable (cf. /mēʾēn/, ‘he refused’ [“compensatory lengthening”], and /biʿēr/, ‘he burned’ [“virtual doubling”]) while the vowel of the second syllable is variable (cf., for one and the same root, both /bērak/ and /bērēk/, ‘he blessed’). The most plausible hypothesis to explain the various West-Semitic forms, it appears to me, is that the Aramaic and Arabic forms with /a/ in the first syllable represent the original form, which would, therefore, have been /qattala/; /i/ in the first syllable in Hebrew reflects the common tendency towards dissimilation in that language (i.e. the first of two syllables showing /a/ ... /a/ becomes /i/) while the /i/ in the second syllable in Aramaic and, secondarily, in Hebrew shows the common tendency in these two languages for the stem vowel of the /YQTL/ form to displace the stem vowel of /QTLa/; the variation in Biblical Hebrew reflects the fact that the second change was taking place in late proto-Hebrew and was not yet complete in Biblical Hebrew as we know it.<sup>1019</sup> T. cites arguments in favor of the form /qattala/ appearing only “sporadisch” (p. 559), but there is in fact no evidence whatever for /qattila/ in Ugaritic. A form has appeared in one of the texts from the 1994 excavations that may indicate that Ugaritic shared the dissimilatory tendency of Hebrew: {ihb} in RS 94.2168:11 may be taken as 3 m.s. D-stem /QTLa/ /, ‘he loved (intensely)’, to be vocalized /ʾihhaba/. This form appears to confirm that {ihbt} in RS 16.394:53' (*KTU* 2.31:49), not treated by T. in this grammar, is indeed a verbal form.<sup>1020</sup>

— p. 559 (§74.414.1,2). T. etymologizes Ugaritic GR, ‘to attack’, by a Hebrew/Aramaic III-y root and Akkadian *garû*, mentions the possibility that the Ugaritic forms could be derived from a geminate “Wurzelvariante,” but does not mention the possibility of a hollow root, which would be supported by Hebrew G(W)R, ‘to quarrel.’

— p. 559 (§74.414.2). Because I have proposed comparative data for interpreting *hwt* in RS 29.093:15 (*KTU* 2.70) as meaning ‘repair’,<sup>1021</sup> a meaning which fits the context, in which a ‘house’ has been mentioned, I remark with astonishment that T. opts not to translate this verb nor even to propose a hypothetical interpretation—an intellectual exercise which elsewhere T. shows no tendency to eschew.

— p. 559 (§74.414.2). T. takes *khḏnn* in RS 29.093:13 (*KTU* 2.70) as a /QTLa/ form<sup>1022</sup> with no mention of the analysis—which appears to me far more plausible—as the imperative. Moreover, the meaning is not, as T. proposes, ‘to lie’ or ‘to hide’, but ‘to refuse’.<sup>1023</sup>

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<sup>1019</sup>This reconstruction may also explain the variation between forms with *hireq* and those with *seghol* better than an attempt to set up an inner-Biblical Hebrew phonetic rule that would account for the variation (A. Rubin, “A Note on the Conjugation of פ"ל Verbs in the Derived Patterns,” *ZAH* 14 [2001] 34-42).

<sup>1020</sup>The new text rules out the explanation of {ihbt} once proposed by Sanmartín Ascaso (*UF* 3 [1971] 177 n. 24) according to which the first syllable of the verb would have contracted with the preceding particle, for in RS 94.2168 {ihb} is preceded by a word-divider.

<sup>1021</sup>AAAS 29-30 (1979-80) 28.

<sup>1022</sup>On the problem of a /QTLa/ form with the ending *-nn*, see above remark to p. 223 (§41.221.52c), etc.

<sup>1023</sup>AAAS 29-30 (1979-80) 24, 28; see now *Context* III (2002) 110; Bordreuil and Pardee, *Manuel* (2004) II 88, 89; here above p. 223 (§41.221.52c), etc.

— p. 563 (§74.415). T. interprets *mlsm* in RS Varia [20]:22 {*mlsm mrkbt*} as a D-stem “Faktitiv oder kausativ,” in context “die, welche die Streitwagen (schnell) laufen machen,” with no mention of the possibility that the expression may be simply intensive, that is, ‘chariot runners (lit. those who run with respect to chariots)’.<sup>1024</sup> As a verb of movement for which the marked causative is attested in other Semitic languages,<sup>1025</sup> the interpretation as a factitive may not be preferred. (On the theoretical problem of interpreting the D-stem as expressing the causative, see above, remark to pp. 544-67 [§74.41]).

— p. 566 (§74.416.5). It is a complete mystery to me why T. chooses RS 18.028[A]:5 *l šlm* (*KTU* 4.342) as his one and only example of a “qu/ittūl”-type D-infinitive. The consonantal text provides, of course, no data on what the vowels of the form may be and I see nothing remarkable in the usage that would lead anyone to believe that the standard /quttal/ pattern has here been replaced by a pattern that is only attested as a common pattern in common-era Aramaic and Hebrew.

— p. 568 (§74.422), p. 560 (§74.414.2), p. 886 (§95.22). Again, T.’s bias against the presence of 2 m.s. verbal forms anywhere in the ritual texts leads him to analyze *tqdm* in RS 34.126:30 (*KTU* 1.161) as D-passive with no mention of the editors’ consistent presentation of the form as 2 m.s. active.<sup>1026</sup> T.’s analysis of the verb as passive and his analysis of the phrase as a whole lead him to take the following word, ‘*sr*, for him a singular absolute, as a collective; the editors took ‘*sr* as in the construct state and hence unsusceptible to analysis for number.<sup>1027</sup>

— pp. 571-72 (§74.432). Of six roots providing possible tD-stem /YQTL/ forms, only one contains a /ʔ/ or another possible indicator of differentiation in the stem vowel of the Gt- and tD-stems. The most important data are provided by the existence of forms spelled {ytšāl}; if the indirect indications that the Gt had /i/ as the stem vowel be admitted (see above, remark to p. 519 [§74.231.1]), the forms spelled with {ā} must be tD, with metathesis of the prefixed /t/ and the first root letter, as occurs in the later Northwest-Semitic languages (e.g., Hebrew /hištammēr/ ← /\*hitšammara/). T. does not, however, admit this conclusion, preferring to conclude from the existence of the Gt-stem in other Semitic languages that Ugaritic also probably had only that t-stem for the root ŠʾL, and not the tD as well. All other forms discussed in this section are either of uncertain root or else have a sibilant as their first radical and are hence open to operation of the metathesis rule. Most, however, receive full treatment both here and above in the section on Gt forms, which appears to be an unnecessary waste of space. If T. considers the analysis as a tD to be preferable in no single case, as appears to be the case from his conclusion to each entry, a simple listing here of possible forms with reference to the discussion of the Gt-forms would have been sufficient.

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<sup>1024</sup>Bordreuil and Pardee, *Semitica* 41-42 (1991-92) 46, 52; Pardee, *Les textes rituels* (2000) 895, 897.

<sup>1025</sup>Pardee, *Les textes rituels* (2000) 897.

<sup>1026</sup>Bordreuil and Pardee, *Syria* 59 (1982) 123, 128; Pardee, *Verse in Ancient Near Eastern Prose* (1993) 209; Bordreuil and Pardee, *Une bibliothèque* (1991) 154; Pardee, *Les textes rituels* (2000) 818, 819; idem, *Ritual and Cult* (2002) 88; Bordreuil and Pardee, *Manuel* (2004) II 64-65.

<sup>1027</sup>See in particular, Pardee, *Les textes rituels* (2000) 824 n. 47; idem, *Ritual and Cult* (2002) 115 n. 130.

— p. 572 (§74.433). One must ask oneself if it was worth devoting half a page of the tD section to disproving the analysis of *tbšr* in RS 2.[008]<sup>+</sup> v 26 (*KTU* 1.4) as a tD imperative. Surely there must have been a device for presenting the form under the G-stem /YQTLØ/ heading, where T. believes the form properly to belong.

— p. 573 (§74.434). T. objects to *tmz*<sup>c</sup> in RS 3.322<sup>+</sup> i 36 (*KTU* 1.19) being a tD-/QTLa/ form on the basis of his view of the context of line 46, where the form re-appears with, according to T., no plausible feminine subject if one wishes to parse the form as G- or D-stem imperfective. The contextual argument is really of no value, however, for the feminine subject in question, the girl *Puġatu*, has gone nowhere during the events narrated in the intervening lines.<sup>1028</sup>

— pp. 575-76 (§74.50). T. discounts the existence of a /qātala/ verbal stem in Ugaritic on two principal bases: (1) the stem is (almost) absent from Hebrew, and (2) it is attested only in South Semitic, viz., in Arabic and Ethiopic. Neither argument appears to be valid: though rare, there are certainly cases of /qōtēl/ forms in Hebrew<sup>1029</sup> and Arabic is a West-Semitic not a South-Semitic language. Since the form does not appear in Aramaic, the proto-Semitic form may be considered to have dropped from that language (since it exists in both West and South Semitic, it is not plausible to consider it an innovation in both branches). A further problem in this introduction to the Ugaritic situation is the assertion that the /qālala/ stem (i.e., as attested with hollow and geminate roots), known in Ugaritic grammar as the L-stem, is functionally identical with the D-stem. Though the Ugaritic /qālala/ stem may not have behaved identically, the /qōlēl/ stem in Hebrew shows one important difference from the D-stem: for hollow roots, it is attested with verbs of movement and quasi verbs of movement as the rough equivalent of the causative stem (e.g. *šōbēb*, ‘cause to go back’, or *rōmēm*, ‘cause to be high’), something that is virtually without parallel in the D-stem. In Arabic, stem III usually shows semantics distinct from stem II. Simply describing the Ugaritic L-stem as the functional replacement of the D-stem for roots without a D-stem is, therefore, not quite correct; it appears to express roughly the semantics of that stem in hollow roots but not in geminate roots. T.’s explanation does not, in any case, match the distribution of the /qōlēl/ and /qittēl/ stems in Hebrew: though it is true that very few hollow roots show D-stem forms,<sup>1030</sup> that is less true of geminate roots, where D-stem factitives are reasonably well attested (e.g., *hillēl*, ‘to praise’, *ḥillēl*, ‘to profane’). Moreover, /qillēl/ and /qōlēl/ forms are occasionally attested for the same root, with different meanings.<sup>1031</sup> All this being the case, T.’s classification of all Ugaritic C<sub>1</sub>C<sub>2</sub>C<sub>2</sub> forms from geminate roots as L-stem (pp. 578-80 [§74.511b]) must be called into question: I see nothing that impedes the classification of any number of these forms as D-stem (on the specific case of √‘ZZ, see following remark). In any event, the possibility that the D-stem and L-stem showed

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<sup>1028</sup>Pardee, *Context I* (1997) 351-52.

<sup>1029</sup>GKC §55b.

<sup>1030</sup>In Ugaritic, there is no clear case of such a form, though *tgwln* in RS 15.134:4 (*KTU* 1.82) may provide one (see remark above to p. 500 [§73.611.2d], etc.).

<sup>1031</sup>GKC 67l, note 1.

semantics roughly similar to those of the later Northwest-Semitic languages bears considering and a rethinking of the semantic expression of the L-stem in hollow roots as opposed to geminate roots appears necessary.<sup>1032</sup>

— pp. 577-79 (§74.511a, b), p. 649 (§75.526a), p. 678 (§75.673), p. 768 (§82.35). T.'s argument that {tqnn} in RS 92.2014:5, 7 (RSO XIV 52) should be L-stem, rather than G-stem, as I once proposed,<sup>1033</sup> is almost certainly correct—though he has no idea what the form means (he proposes half-heartedly that the root should be hollow, not geminate, but suggests no etymology that would square with his translation “sich aufrichten”). I argued for the analysis as a G-stem on the basis of y‘zz in RS 24.247+:57' (KTU 1.103+) which is intransitive. But, as T. holds on p. 579, the latter form is probably L-stem, though not with a factitive meaning: he proposes “sich als stark erweisen gegenüber,” which appears perfectly plausible. He fails, however, to draw the necessary conclusion with regard to the epistolary idiom *ilm t‘zzk*, which is indubitably factitive: ‘may the gods strengthen you’.<sup>1034</sup> In spite of the obvious difference in meaning, T. analyzes both forms as L-stem. By analogy to the Hebrew and Arabic systems (see preceding remark), the latter should be D-stem, the former L-stem. Reasoning from this clear semantic distinction between the two verbal stems of √‘ZZ, one may conclude that *tqnn*, which cannot have a factitive meaning in context, is indeed an L-stem form and that it expresses intransitively the stance of its subject, the scorpion. It should, therefore, be vocalized /taqāninu/, rather than /taqninu/ or /tiqnanu/ as I first proposed. To explain the Ugaritic form, I appealed to Arabic √QNN which, in the VIII<sup>th</sup> stem, means ‘to stand on the tip of something’; the scorpion’s stance on the tips of its legs with its tail curved over its back fits this general semantic field.<sup>1035</sup> The identification of the root as geminate raises, however, another problem: the finite form *tqnn* appears in a *figura etymologica* with a noun *qn*. Usually, in such cases, the noun is in fact the infinitive of the same root as the finite form. Because {qn} cannot represent the /qatāl/ infinitive (the productive infinitival form in Ugaritic) of a geminate root, it must either be a verbal noun in another form (I vocalized *qannu*)<sup>1036</sup> or else show that the root is in fact hollow, as T.

<sup>1032</sup>Rethinking of the question is what is needed, not simply a listing of a given form as both D-stem and L-stem, with no preference expressed nor even a cross-reference, as T. does (p. 553 [§74.412.27], p. 578 [§74.511b]) with *ytl* in RS 3.322+ i 41 (KTU 1.19)

<sup>1033</sup>*Les textes rituels* (2000) 830-32. This analysis was corrected in the official *editio princeps* (Bordreuil and Pardee, RSO XIV [2001] 387 and subsequently in idem., *Manuel* [2004] II 69).

<sup>1034</sup>The formula is attested twice, once in RS 1.018:6 (KTU 2.4) ({t‘zʿl[k]}), again in the practice letter RS 16.265:4 (KTU 5.9).

<sup>1035</sup>J. N. Ford, “The Verb *tqnn* in RS 1992.2014,” *UF* 33 (2001) 201-12 (cf. idem, *UF* 34 [2002] 120, 135), has recently appealed to Akkadian “*kanānu* (var. *qanānu*) ‘to twist, to coil,’ ” to explain the Ugaritic verb (p. 207). If this etymology be accepted, the Ugaritic form leaves little doubt that the root is QNN, rather than KNN (Akkadian also shows a verb *qanānu*, ‘to build a nest’, that appears to be denominative to *qinnu*, ‘nest’). Though the Akkadian root is not used of scorpions, it is used of bulls and lions (p. 208) and hence appears to provide a better contextual parallel than the Arabic root. One may further speculate that the Arabic root, which, as Ford observes, is used primarily for goats standing on peaks, is a semantic specification of the older root QNN.

<sup>1036</sup>That such a proposal is not implausible is proven by the existence of the feminine verbal noun *hnt* from the root ḤNN (RS 2.[004] i 16' [KTU 1.17]); *qnt* in RS 1.002:22 *et passim* (KTU 1.40) may provide another

proposes, rather than geminate. His morpho-syntactic solution for the full phrase *qn l tqnn*, viz., that *qn* would be G-stem, *tqnn* L-stem, with the former functioning as an ‘infinitive absolute’, certainly finds parallels in Hebrew, but one wonders if *qn* might not be a verbal noun not linked so tightly with a given verbal stem (hence my *qannu*).<sup>1037</sup>

— p. 578 (§74.511a). Though it may be judged the more likely, the analysis of {tṭbb} in RIH 78/20:19 (*CAT* 1.169) as L-stem of √T(W)B cannot be considered as certain as T.’s confident presentation might lead some to believe, for the signs may be divided as {tṭb b} and the resultant verb form analyzed as G-stem.<sup>1038</sup>

— p. 578 (§74.511b), p. 677 (§75.673). On the basis of the criterion proposed in the second and third preceding notes for distinguishing D-stem from L-stem when the root is geminate, *yklḥn* in RS 2.[008]<sup>+</sup> v 10 (*KTU* 1.4), which is clearly factitive (‘he may complete it’ ← √KLL, ‘to come to completion’) and which T. parses with no indication of doubt as L-stem in both sections cited, is better analyzed as D-stem.

— p. 578 (§74.511b), p. 581 (§74.514), pp. 677-78 (§75.673). T. does not mention the possibility that *ymnn* in RS 2.002:37 (*KTU* 1.23) and its participle *mmnnm* in lines 40, 44, and 47 of the same text might be something other than L-stem forms of a geminate or hollow root. He will go no further than to say that the meaning and etymology are “umstritten.” It has been proposed, however, that these are /qatlal-/ forms<sup>1039</sup> of the root YMN, a denominative verb from /yamīnu/, ‘right hand’.<sup>1040</sup> If this etymology be correct, the writing of the participial form without {y} shows that the base form was /maqtalil-/: /maymanin-/ → /mêmanin-/.

— p. 579 (§74.511b), p. 678 (§75.673). A good case may be made for *tpnn* in RS 22.225:6 (*KTU* 1.96) being derived from PHY, ‘to see’,<sup>1041</sup> rather than from a root PNN that T.

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example if the root of the following form, *tqṭṭ(n)*, is QṬṬ, as T. proposes (p. 579). Ford (*UF* 33, p. 209) cites *šl* in RS 19.1011:6 (*KTU* 2.61) as an example of an “infinitive absolute” from a geminate root {ŠLL, ‘to plunder’); it would be an example of the narrative use of the infinitive, joined with an independent pronoun (the full phrase is *w šl hw qrt*, ‘and he plundered the town’); the interpretation of *šl* as an infinitive may, however, not be necessary (see remark below to p. 702 [§76.521.1], etc.).

<sup>1037</sup>On p. 677 (§75.66), T. holds quite correctly that *qn* cannot be a G-stem verbal noun of the type /qatāl-/, but his conclusion that *qn* does belong to a geminate root cannot be accepted as necessary until it be proven that only the /qatāl-/ verbal noun can be used in a *figura etymologica* with a finite verb. This T. has not attempted to do and his stance that the forms *qn* and *tqnn* must be derived from a hollow root—without a proposal for such a root—must therefore be considered to be based on too narrow a view of this particular *figura etymologica*.

<sup>1038</sup>Pardee, *Les textes rituels* (2000) 892 n. 117.

<sup>1039</sup>Pa‘lel or Pi‘lel in Biblical Hebrew (GKC §55d).

<sup>1040</sup>The suggestion goes back to Cross, *Canaanite Myth* (1973) 23, n. 58. Cf. Pardee, *Context I* (1997) 280-81 note 51. In this note, I referred to the form as L-stem and indicated it as having a long first vowel, but this was not the way to deal with a form of a triconsonantal root that is considered to show a reduplicated final consonant (as opposed to a hollow root with reduplicated final consonant).

<sup>1041</sup>Ford, *UF* 30 (1998) 229; Pardee, *Ritual and Cult* (2002) 161-62.

refuses to identify etymologically.<sup>1042</sup> The two {n}s would in this analysis represent the energetic/suffixal morphemes, rather than being part of the root. The strongest internal argument for this interpretation of *tpnn* is the presence in line 2 of the word *tp*, most plausibly taken as 3 f.s. /YQTL/ of PHY, as Ford has argued.<sup>1043</sup>

— p. 579 (§74.511b), p. 678 (§75.673). As with {y‘zz} in RS 24.247<sup>+</sup> (*KTU* 1.103<sup>+</sup>) (see remark above to pp. 577-78, 678-79 [§74.511a, b]), *yšdd* in lines 35' and 37' of the same text is more plausibly D-stem or L-stem, as T. proposes, than G-stem, as I have vocalized the form.<sup>1044</sup> In this case, Hebrew provides an analogy for the L-stem of ŠDD having a transitive function and the meaning ‘destroy violently’ that is required in the Ugaritic text, and the parsing of *yšdd* as D- or L-stem is for that reason difficult to decide here.<sup>1045</sup>

— p. 580 (§74.511b), p. 678 (§75.673). By analogy with epistolary *t‘zz* (see above, remark to pp. 577-78, 678-79 [§74.511a, b]), {ilm ... [t]tmmk} in RS 92.2005:28 (RSO XIV 49) is in all likelihood D-stem, as the editors propose,<sup>1046</sup> not L-stem as T. would have it.<sup>1047</sup>

— p. 580 (§74.511c). In this section, T. proposes that four /YQTL/ forms with reduplicated final radical may be G-stem, “aus semantischen Gründen.” Three of the four are intransitive while the fourth has a simple transitive function. Though he does not explain precisely what he means by ‘semantic grounds’, the basis for this category is apparently his view that the semantic function of the L-stem should be similar to that of the D-stem, i.e., factitive or even causative. As we saw above, however (remark to pp. 575-76 [§74.50]), though L-stems of hollow roots may function as equivalents to both the D-stem and the causative stem, such is not the case of L-stems of geminate roots, where the meaning may be either intransitive or transitive and in the latter case may differ from that of the factitive D-stem. This description fits remarkably well the four cases adduced here (to the extent that the readings/restorations be accepted): *yhr[r]*, ‘he is hot’ (RS 2.[012] ii 37' [*KTU* 1.12]), *thss*, ‘she thinks of’ (RS 3.343<sup>+</sup> iii 25' [*KTU* 1.15]), *rt ymll*, ‘he kneads mud’ (RS 3.325<sup>+</sup> v 28 [*KTU* 1.16]), and *tḡdd*, ‘she swells’ (RS 2.[014]<sup>+</sup> ii 25 [*KTU* 1.3]). All four of these are, therefore, plausibly L-stem intensives rather than G-stem forms on the /YvQLvL-/ pattern; this analysis appears by far

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<sup>1042</sup>He makes no mention of del Olmo Lete’s identification of the root behind this form as identical with Arabic FNN, which he interpreted in the Ugaritic text as meaning ‘distort’ (*Annuari de Filologia* 15 [1992] 9, 11; *La religión cananea* [1992] 256-57; *Canaanite Religion* [1998] 381-82).

<sup>1043</sup>As Ford has shown (*UF* 30 [1998] 254-56), there is no objective basis for the emendation of that form to {tp<nn>} as proposed by Dietrich and Loretz (*UF* 29 [1997] 151-60; *Studien zu den ugaritischen Texten. I Mythos und Ritual in KTU 1.12, 1.24, 1.96, 1.100 und 1.114* [AOAT 269/1; Münster: Ugarit-Verlag, 2000] 234, 241-42).

<sup>1044</sup>*Les textes rituels* (2000) 551.

<sup>1045</sup>In the *Manuel* (2004) II 79, Bordreuil and I now vocalize the form as a D-stem.

<sup>1046</sup>Bordreuil and Pardee, RSO XIV (2001) 374.

<sup>1047</sup>T.’s suggestion on p. 585 (§74.53—cf. p. 788 [§83.115b]) to analyze the form as tL (“mögen sich dir gegenüber ge[recht verhalten]”) finds no parallel in the epistolary formulae and the analysis of the pronominal suffix as ‘datival’ in function is also aberrant in prose. One may not, therefore, accord it any credit.

the preferable one for *yħrr* (if the restoration of a second {r} be correct) and *tġdd*, where the semantics are intensive intransitive.

— p. 580 (§74.512), p. 678 (§75.673), p. 740 (§81.12d). Though he places a question mark before the entry on p. 580, neither here nor on p. 678, where there is no question mark, does T. provide an interpretation of *tmm* in RS 2.[022]<sup>+</sup> iii 13, 27 (*KTU* 1.5) other than as the L-stem imperative of a geminate root *TMM*, unattested elsewhere in Ugaritic, with the corresponding Arabic verb “niedertreten” cited as a possible cognate. Only on p. 740 does he address the interpretation of *tmm* as an expanded form of the adverb *tm*, ‘there’, which he judges to be implausible. In such a broken context, however, no certainty is possible, and others have seen here the adverb.<sup>1048</sup> If T.’s lexical identification be correct, the parsing as a 2 m.s. imperative (explicit on p. 678) may be called into question, for the following form may be dual or plural (see remark below to p. 625 [§75.332b]).

— p. 581 (§74.514). In line with the preceding comments, *mdllkm* in RS 1.002:21' *et passim* (*KTU* 1.40), should be D-stem, not L-stem, i.e., the basic notion is factitive, ‘make someone poor’. In context, it also has appeared likely to me that the form is passive,<sup>1049</sup> not active as T. holds here without even mentioning the other possibility. Within this text, of which the driving notions are communion, expiation, and political harmony, it appears plausible that the reference is to obtaining these virtues for the downtrodden rather than for the downtreaders.

— p. 581 (§74.514), p. 678 (§75.673). *mħllm*, ‘those who purify’, in RS 24.266:23' (*KTU* 1.119) is parsed as L-stem participle with no mention of the possibility that it may be D-stem. The latter analysis appears preferable, however,<sup>1050</sup> because (in line with preceding remarks) the semantic function is very precisely factitive to the G-stem (‘be pure’ → ‘make pure’).

— p. 582 (§74.515.1), p. 601 (§74.623.3b), p. 678 (§75.673). This root *HLL* also appears in RS 24.260:6 (*KTU* 1.115) in the following sequence of signs: {wšħl|l . ydm}. Because of the absence of word-divider in the first part of the line, some, including T. here, have divided the signs to read *w š ħll*, ‘and a sheep (for) the purifying of hands’. Nowhere else in the Ugaritic ritual texts, however, is animal sacrifice ever ordained as the means of effecting the form of purification expressed by the root *HLL*, and it thus appears more plausible to divide the signs to read *w šħll ydm*, ‘and purify the hands’.<sup>1051</sup> This analysis would provide an example of the similarity in concept between effecting a state (D-stem) and causing a state (Š-stem) that is so characteristic of the Piel and Hiphil of stative roots in Hebrew. T. could not accept this analysis, of course, because he allows for virtually no cases of 2 m.s. verbal forms in these texts, a stance that I have had occasion to criticize above (see remark to p. 211 [§41.12], etc.).

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<sup>1048</sup>Cf. Smith *apud* Parker, ed., *Ugaritic Narrative Poetry* (1997) 145.

<sup>1049</sup>*Leslau* (1991) 1185, 1187; *Les textes rituels* (2000) 119-20; *Ritual and Cult* (2002) 81-83; cf. Bordreuil and Pardee, *Manuel* (2004) II 52-53.

<sup>1050</sup>Pardee, *Les textes rituels* (2000) 667, 678 (with previous bibliography in note 96).

<sup>1051</sup>*Ibid.*, pp. 643, 644, 647-48.

— p. 582 (§74.515.2a,b). Though it is certainly plausible, for the reasons indicated here above, that *trmmt* represents a verbal noun of the L-stem (RM ‘be high’, RMM ‘make high’), the two examples adduced from geminate roots present more ambiguity. A direct link between *tbrtt* (RS 15.125:10’ [KTU 2.19]) and an eventual L-stem must be judged very unlikely for it expresses the direct factitive of the G-stem, ‘be pure/free of duty’ → ‘make pure/free of duty’ (the G-stem appears twice above in this text, in line 3’ said of the sun, in line 4’ said of the person benefiting from the manumission). For that reason it must be considered likely—to the extent that it is directly connected with a verbal stem—that that stem would have been the D, not the L. The case for *tdmm(t)*, ‘misbehavior’, being directly related to an eventual L-stem verbal form is theoretically stronger, for, judging from the proposed etymologies (Hebrew ‘to devise’, Arabic ‘to blame’), the verb would probably not have been a simple factitive.

— p. 583 (§74.522b), p. 678 (§75.674). Though T.’s interpretation of *mlkn yd hrd yddl* (the last word is plausibly emended to *ydl*) as “wird der König samt seiner *hrd*-Truppe erniedrigt/niedergeschlagen werden,” with *ydl* analyzed as L-passive (RS 24.247+:46’ [KTU 1.103+]), is certainly a possible one, it must be criticized on two grounds: (1) As observed above (remark to p. 581 [§74.514]), the simple factitive function is more plausibly expressed by the D-stem when the root is geminate; (2) one might have expected T. at least to mention the analysis of *yd* as the noun ‘hand’ → ‘power’ (rather than as the preposition meaning ‘with’) and as the object of the verb (‘... the king will lay low! the power [lit. ‘hand’] of the *hurādu*-troops’).<sup>1052</sup> As for *tdlln* near the beginning of this text, there is a problem of attachment of this line segment to the principal text from which it is separated by a break; T.’s decision to attach this segment to line 6, rather than to line 7 as I have proposed,<sup>1053</sup> has little to recommend it. Attached to line 7, as I believe more likely, the form *tdlln* is best taken as D and as factitive: “the weapon of the king will lay it (the land) low” (*mrhy mlk tdlln*).<sup>1054</sup> — p. 584 (§74.522b, §74.523), pp. 678-79 (§75.674). Because expressing the agent of a passive verb is not done in the ancient Semitic languages, T.’s interpretation of the verb in the line *hl ‘sr thrr l išt* (RS 2.002:41, 44, 48 [KTU 1.23]) as L-passive is belied by the resultant need to take the preposition *l* as marking the agent (“Siehe, der Vogel ist verbrannt vom(?) Feuer”). It is better to take the *l* as marking place rather than agent and hence perhaps either to analyze *thrr* as L-stem active (‘the bird roasts on the fire’)<sup>1055</sup> or to take the

<sup>1052</sup>Cf. idem, *AfO* 33 (1986) 125, 140, 146 (I mention the possibility of analyzing the form as N-stem; the analysis as D-stem must, however, be judged far more plausible); idem, *Les textes rituels* (2000) 549, 551, 559; idem, *Ritual and Cult* (2002) 140. Below, p. 585 (§74.53), T. suggests that {yddl} may be a token of the tL stem: /*\*yitdālil-*/ → /*yiddallil-*/. Unfortunately, that analysis does not account for the writing with two {d}s, since geminated consonants are never written twice in the old Semitic languages.

<sup>1053</sup>On the reading problem, see idem, *AfO* 33 (1986) 118, 122-23, idem, *Les textes rituels* (2000) 538, 554.

<sup>1054</sup>Idem, *AfO* 33 (1986) 130-31, 145 (the transcription “tdlln” on p. 118 was a typographical error as is clear from the subsequent references to the form); cf. idem, *Les textes rituels* (2000) 550; idem, *Ritual and Cult* (2002) 136, 139.

<sup>1055</sup>For the translation as an active form, cf. Lewis *apud* Parker, ed., *Ugaritic Narrative Poetry* (1997) 211.

direct address to the deity *ʾIlū* as continuing here (‘you roast a bird on the fire’),<sup>1056</sup> in which case the verb would be factitive, hence D-stem. To the extent that T.’s interpretation of the form *ḥrr* in RS 2.[022]<sup>+</sup> ii 5 (*KTU* 1.5) as L-stem /QTLa/ is tied in with the finite form in RS 2.002, one may doubt its validity also. On the other hand, in context, where the parallel terms are nouns and where *ḥrr* does not explicitly express a factitive, it appears plausible to take the word as an L-stem verbal noun:

|                        |   |
|------------------------|---|
| <i>yʿrb bʿl b kbdh</i> | <i>Baʿlu</i> will enter his liver (i.e., his insides)                     |
| <i>b ph yrd</i>        | will go down his mouth,   |
| <i>k ḥrr zt</i>        | like a ripe olive (lit. like the roasting [= becoming ripe] of an olive), |
| <i>ybl ʾrṣ</i>         | (like) the produce of the earth,  |
| <i>w pr ʿsm</i>        | even (like) the fruit of the trees’.                                      |

— p. 585 (§74.53). T.’s “n[eue] L[esung]” {y[ʿ]n} (RS 2.[008]<sup>+</sup> iii 10’, in place of {y[t]ʿb1} in *CAT* 1.4) is hardly qualifiable as new since it goes back to the *editio princeps*.<sup>1057</sup>

— p. 585 (§74.611). T.’s reference to the appearance of the Š-stem in Aramaic as “sporadisch im aram. Dialekten” is correct, but readers uninformed about Aramaic could take the description to mean that the form is productive in some dialects, absent from others. Such is not the case, of course, for all the ancient dialects of Aramaic show Š-stem forms, some of which are Akkadian loan-words, others native Aramaic forms, whereas the productive causative stem is the Haphel which in time becomes an Aphel. This distribution requires the conclusion that proto-Aramaic, like the proto-Canaanite languages, had a Š-stem causative. Each of these groups of languages retained the form in different ways, however: in Ugaritic the Š-stem was still productive and there is as yet no trace of slippage toward a Haphel/Aphel,<sup>1058</sup> in Aramaic many lexical Shaphels were retained after the productive causative prefix shifted from /š/ to /hʾ/, in Hebrew only one such form was retained in a full paradigm (the Št of H̄WY /hištaḥ<sup>a</sup>wā<sup>h</sup>/, ‘to bow down’) though the ultimate origin of several I-Š roots has been claimed to lie in this form, while in Arabic the productive IX<sup>th</sup> stem (/ʾistafaʿala/) is in fact the old Št-stem.

— p. 587 (§74.622.1), p. 588 (§74.622.3), p. 616 (§75.218). T. follows *CAT* in indicating {ʾšṣp} in RS 16.402:12 (*KTU* 2.33) as a certain reading, though such is in fact not the case.<sup>1059</sup>

— pp. 587-88 (§74.622.1). On T.’s decision to reconstruct the Š-stem /YQTL/ form as /yušaqtil-/ , on the basis of comparative data, rather than /yašaqtil-/ , on the basis of the Ugaritic 1 c.s. form, which is written {ʾšqtl}, hence /ʾašaqtil-/ , see above, introduction, general remark on vocalization, and the general remark to the D-stem (pp. 544-46 [§§74.412.1-16]).

<sup>1056</sup>E.g., Pardee, *Context* III (1997) 281. In the *Manuel* (2004) II 29-30), Bordreuil and I prefer the intransitive interpretation because there is no evidence for the reduplicated form *ṣḥrrt*, the parallel term in this passage, being factitive (as I took it in *Context*, viz., I translated both *ṥḥrr* and *ṣḥrrt* as ‘(you) roast’).

<sup>1057</sup>Virolleaud, *Syria* 13 (1932) 126, 127 (the {y} was still being read as “i” at the time, but the third sign was given as {n} and the missing sign was reconstructed as {ʿ}).

<sup>1058</sup>Tropper, *Kausativstamm* (1990) 113-82.

<sup>1059</sup>Pardee, *AfO* 31 (1984) 216.

— p. 589 (§74.622.3), p. 625 (§75.331e), p. 864 (§93.33c), p. 869 (§93.362.1). T. takes the Š-stem of HLK, ‘to go’, as taking double-accusative complementation in the phrase *ášhlk šbtk dmm* that occurs in various states of preservation in RS 2.[014]<sup>+</sup> v 2-3, 24 (*KTU* 1.3) and in RS 3.340 i 11 (*KTU* 1.18), partially reconstructed. Because, however, the causative stem of verbs of movement normally takes one-place direct-object complementation (‘X causes Y to go’), the two complements here are certainly to be distinguished as an adverbial accusative (*šbtk*) and a direct object (*dmm*). The meaning of the phrase is, therefore, ‘I will cause blood to run on your gray hairs’. This may be translated into good English as ‘I will cause your gray hairs to run with blood’ (or in German as “Ich werde dein graues Haar von Blut überfließen lassen” [p. 589]), but the literal translation is not ‘I will cause your gray hairs to run (somewhere) (and will also) cause blood to run (somewhere)’. (See above, with respect to QRB, the remark to p. 551 [§74.412.25], etc.).

— p. 589 (§74.622.3), p. 625 (§75.331e). The verbal form discussed in the previous remark is vocalized “/ʾašahliku/” on p. 589, “/ʾašahlik/” on p. 625.

— p. 589 (§74.622.3), p. 869 (§93.362.1). T.’s classification of the Š-stem of YTN, ‘to give’, as one of “seltene Fälle, wo der Š-Stamm die Valenz eines Verbs nicht erhöht” (p. 869), i.e., does not change the one-place complementation of the G-stem (‘X gives Y’) to a two-place complementation (‘X causes Y to give Z’), is a striking example of T.’s understanding of “Valenz” as applying purely to the surface-level expression (see below, remark to §93.3 [pp. 861-69]). He himself recognizes that the basic meaning of the form is (‘X causes Y to give Z to M’), but, since the ‘Y’ segment is never attested, the verb is classified as bivalent (i.e. as taking only one accusative complement). And he has a point, for, if the corpus were larger and ŠTN never attested in the double-accusative construction, one might be obliged to conclude that the form has become lexicalized as meaning something along the lines of ‘dispatch, have sent’. Until the corpus becomes much larger than it is now, however, the possibility must be left open that the full form of the expression will someday appear. After all, even the recognition of the existence of the form is relatively recent.<sup>1060</sup>

— p. 591 (§74.622.3), p. 691 (§76.343a). On p. 591, *yšlḥmnh*, ‘he causes him to eat’, in RS 2.[014]<sup>+</sup> i 5 (*KTU* 1.3) is analyzed as either /YQTLu/ or /YQTLØ/ whereas, on p. 691, the parsing as /YQTLu/ is given as certain. (Both parsings require, of course, accepting that the energetic endings were attached to one or the other of these two forms—see above, remarks to pp. 497-506 [§73.6]. For the similar case of *yšqynh* in line 9, see remark below to p. 662 (§75.532], etc.)

— pp. 591-92 (§74.622.3), p. 598 (§74.624), p. 670 (§75.537f), p. 704 (§76.521.3). As is clear from RS 6.021:1 (*KTU* 6.13), RS 6.028:1 (*KTU* 6.14), and RS 25.318:2 (*KTU* 6.62), one of the primary meanings of the Š-stem of ‘LY, ‘to ascend’, is ‘to present, to offer’, whereas ‘to bring an offering’, T.’s third gloss on p. 670 (“ein Opfer dabrigen”) is quite rare. Perhaps more care could have been taken to make it clear to the reader whose main point of reference is Hebrew that (a) the verb is never used in Ugaritic, as is *he<sup>e</sup>lāh* in Biblical Hebrew, to designate the effecting of a holocaust-type offering and (b) in at least two of the

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<sup>1060</sup>Caquot, *Ugaritica* VII (1978) 125, 391.

three cases cited here above, the object of the verb is an inanimate object.<sup>1061</sup> Thus the first two glosses on p. 670, ‘hinaufsteigen lassen, hinaufbringen’, especially the second, correspond more closely to standard Ugaritic usage. The only cases that even approach Hebrew usage are RS 3.322+ iv 23-24 (*KTU* 1.19), where the object of the verb is the offering term *dḡt*, of which the precise meaning is unknown, and RS 6.028, where *pgr*, ‘mortuary offering’, is the object of the verb (in the parallel text RS 6.021, it is *skn*, the ‘stela’ commemorating the offering, that is the object of the verb—on p. 704, T. cites the former text but only refers to the latter as a point of comparison).

— p. 592 (§74.622.3). T. follows *KTU/CAT* in not showing a horizontal paragraph divider between lines 5' and 6' of RS 16.196 (*KTU* 2.25). There can, however, be no doubt about the divider that was indicated on my transcription of this text made available to T., and his interpretation of these lines as constituting a continuous text may not, therefore, be accepted.

— p. 593 (§74.622.3). T. indicates {dkr} as a certain reading in RS 24.266:31' (*KTU* 1.119) when in fact the first sign has entirely disappeared.<sup>1062</sup>

— p. 594 (§74.622.3), p. 596 (§74.62), p. 651-52 (§75.527g-i). Three /YQTLØ/ forms of the Š-stem of the root T(W)B are cited on p. 594 with /i/ in a final closed syllable, 3 m.s., 3 f.s., and 2 m.s., e.g., “*yuṭaṭīb*.” On p. 596, the corresponding imperative form is vocalized “*ṭaṭīb*.” One encounters many such examples on pp. 651-52 (see remark below to those pages). As observed above, seventh general remark, comparative evidence indicates that such vowels would have been short in Ugaritic, i.e., /yaṭaṭīb/ (cf. Hebrew /yāšēb/ and /wayyāšēb/ ← /yahašīb/ vs. /yāšībū/ and /wayyašībū/ ← /yahašībū/) and /ṭaṭīb/ (cf. Hebrew /hāšēb/ ← /hašīb/ vs. /hāšībū/ ← /hašībū/).

— p. 594 (§74.622.3), p. 651 (§75.527g). In the first section cited, *yṭtb* in RS 18.[443]:12' (*KTU* 2.57) is an odd choice to lead off the list of examples of Š-stem forms of the root T(W)B, for, in this text, the context surrounding *yṭtb* has largely disappeared, and there is hence no way of being certain that the root in question is not YTB. The reading of the object of the verb as “[*rgm*]” (i.e., the idiom would be ‘to return word’) is here presented as entirely restored, a more conservative presentation than that of the editors who represented the {m} of that word as certain.<sup>1063</sup> In fact, the sign in question is badly damaged and the restoration of *rgm* is uncertain though possible.<sup>1064</sup> Finally, it should have been noted that the idiom *yṭtb* + *rgm*, i.e., with a 3<sup>d</sup> person subject of the verb, is for the present unattested in letters. Why is such an uncertain example of the 3 m.s. given? The choice could not have been just to fill out the paradigm, for a certain example of 3 m.s. in a ritual text is cited immediately after RS 18.[443]:12'. One may also ask why a single example of the 3 f.s. is provided in which the subject is not present in the verbal formula itself (RS 9.479A:14-15 *rgm tṭtb l 'bdh*,

<sup>1061</sup>Bordreuil and Pardee, *Semitica* 41-42 (1991-92) 26; Pardee, *Les textes rituels* (2000) 390-91.

<sup>1062</sup>Pardee, *Verse in Ancient Near Eastern Prose* (1993) 216, n. 31; see the copy in *Ugaritica* VII (1978) 33, the new copy in Pardee, *Les textes rituels* (2000) 1285 (textual remark on pp. 664-65), and the new photograph and copy in Bordreuil and Pardee, *Manuel* (2004), text 11 in the *Choix de textes*.

<sup>1063</sup>Dietrich and Loretz, *Die Elfenbeinschriften* (1976) 51; this representation is maintained in *KTU/CAT*.

<sup>1064</sup>Observation based on collation of the original.

‘may she return word to her servant’) while the example of RS 15.008:18-20 (*KTU* 2.16), with an explicit subject, is omitted (*ûmy* ‘my *ttb* *rgm*, ‘may my mother return word to me’).

— p. 594 (§74.622.3), p. 651 (§75.527g). Because in the following sentence an explicit subject is named (*w qdš yšr*, ‘and the *qdš*- official shall sing’), it is unlikely that the subject of the verb in *w rgm gtrm yttb* in RS 24.256:20 is indefinite (*KTU* 1.112) (“wird/soll er wiederholen”—there is no explicit antecedent for the “er”). To be preferred is either the analysis of *yttb* as a passive<sup>1065</sup> or as an active, with the *gtrm* as the subject (an alternative mentioned by T.).<sup>1066</sup>

— p. 594 (§74.622.3), p. 651 (§75.527g). The restorations of RS 1.018:6-7 (*KTU* 2.4) and of RS 1.021:12-13 (*KTU* 2.6:13-14) that have been proposed in *KTU* and/or *CAT* and repeated here as valid grammatical and lexical data are tenuous in the extreme because (a) some signs are indicated as partially visible that have in fact disappeared, (b) the restorations of the extant traces are dubious, even unlikely, and (c) the formula restored in RS 1.018 (‘*my šlm w ttb ly šlmk*, ‘with me it is fine, and return to me your well-being’) is unattested elsewhere. It is curious that T. calls attention in a note that follows this grouping of Š-stem forms of √T(W)B to the readings of RS 17.327 (*KTU* 2.35) that differ from those of *KTU/CAT* in the transcriptions that I made available to him but does not mention the fact that those same transcriptions remove from consideration the forms cited from RS 1.018 and RS 1.021.

— p. 595 (§74.623), p. 640 (§75.518). T. assumes that the lacuna at the end of RS 17.117:18' (*KTU* 5.11) has left *štn* too isolated for interpretation: on p. 595, he hesitates between the analyses as an imperative or as /QTLa/, while on p. 640 he simply lists the form as of uncertain analysis. The lacuna that follows this word is situated on the right side of the tablet where the length of any text fallen in the lacuna is difficult to determine, but it cannot have been long here. That being the case, one may suppose either that the indirect object was once expressed pronominally there or that nothing was there: *w štn [ly<sup>?</sup>] b spr*, ‘and send (lit. have given) [to me<sup>?</sup>] (word of that) in a letter’.<sup>1067</sup>

— p. 595 (§74.623). T. recognizes that from the signs {tḫmy} in RS 17.117:6 (*KTU* 5.11) must be extracted the word *ḫmy*, ‘my bread/food’. This is based in part on the presence of the word *yny*, ‘my wine’, in line 7. But he refuses to recognize that the sequence {l tt} occurs before each of these nouns and that that sequence of signs most plausibly reflects the verb of which these nouns are the accusative complement (note the writing {-y} of the 1 c.s. pronominal suffix, which indicates that the noun is in an oblique case). This verb can only be T̄WY, which when transitive in the G-stem means ‘to furnish (various forms of hospitality)’, discussed in remarks above to p. 110 (§32.144.12b), etc., to p. 211 (§41.12), etc., and below to p. 669 (§75.537d), etc.<sup>1068</sup>

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<sup>1065</sup>Pardee, *Les textes rituels* (2000) 634.

<sup>1066</sup>The problem with the latter interpretation is that this would be the only example in the ritual texts of a divine entity speaking (cf. *ibid.*, p. 640).

<sup>1067</sup>Pardee, *AfO Beiheft* 19 (1982) 45.

<sup>1068</sup>On this text, see Pardee, *ibid.*, p. 45, 47- 48.

— p. 595 (§74.623), p. 864 (§93.33c). On p. 595, T. glosses *šskn* in the phrase *šskn m<sup>c</sup> mgn rbt ātrt ym* (RS 2.[008]<sup>+</sup> i 20'-21' [KTU 1.4]) as “jmdn. mit etwas versorgen; etwas für jmdn. besorgen”; on p. 864, he lists the form among examples of the double-accusative construction, but without making clear whether both complements are direct objects or one is an adverbial accusative (on the necessity of keeping these two grammatical categories distinct, see remark below to p. 864 [§93.33c]). Because *sakānu* in Amarna Akkadian is followed by *ana*,<sup>1069</sup> and in Biblical Hebrew the G-stem normally takes prepositional complements and there is no example of the Hiphil taking a double-accusative complement, the analysis of the G-stem as basically intransitive appears necessary (the basic meaning would have to do with ‘caring about, being concerned for, being a benefactor for’). This requires the conclusion that the causative stem would normally have taken one-place complementation (‘X causes care to be evinced with regard to Y’) not two-place complementation (‘X causes Y to care for Z’). The basic meaning of the Ugaritic phrase in question may therefore have been something like ‘produce a benefit for Lady *’Aṭiratu* of the Sea in the form of a gift’ (*ātrt ym* would be the direct object of the verb, *mgn* an adverbial accusative).<sup>1070</sup>

— p. 595 (§74.623). Here T. presents as the only parsing of *šqrb* in RS 1.002:26' (KTU 1.40) that of 2 m.pl. impv. (/šaqribū/). This is certainly plausible, since other 2 m.pl. forms appear in the text. The question remains open, however, as to whether this imperative is addressed to all the participants in this rite or to the principal officiant only, as I have taken it.<sup>1071</sup> More recently, in his attempt to disprove the existence of second-person forms in the ritual texts, T. has proposed that *šqrb* here might be an infinitive,<sup>1072</sup> an analysis that must surely be qualified as a desperate solution to a problem that did not in this case require solving (on the general problem, see remark above to p. 211 [§41.12], etc.). In general, the importance of forms that must convey a verbal notion but do not bear a verbal preformative is, I believe, underestimated by T.: because many expressions of ritual prescriptions are, in these texts, imperfective verbs, it appears that analyzing those that do not bear a preformative as imperative is not a solution to be rejected out of hand, as T. tends to do. For example, in {‘šr . ‘šr . ṛb .ṛ -- ṛ.ṛ bt ṛlṛmṛ ṛ kbkbm} (RS 1.005:2-3 [KTU 1.43]), a passage not discussed in this grammar, the translation ‘prepare a feast in the temple of the Star Gods’ appears indicated by the context, but T. disallows it, considering that a sequence of verbal noun followed by common noun is more plausible (“(erfolgt) die Zurüstung des Festes”).<sup>1073</sup>

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<sup>1069</sup>CAD S, pp. 69-70.

<sup>1070</sup>Recent translators into English, either on the basis of a different understanding of the root or out of concern for a more idiomatic rendering, have placed *mgn* in the slot of the direct object: Pardee, *Context I* (1997) 126: “You really should prepare a gift for the Great Lady ...”; Smith *apud* Parker, ed., *Ugaritic Narrative Poetry* (1997) 120: “Produce, please, a gift for Lady ...”; Wyatt, *Religious Texts* (1998) 90: “prepare, I pray, a gift for the Great Lady ...”

<sup>1071</sup>*Les textes rituels* (2000) 97, 98.

<sup>1072</sup>UF 33 (2001) 683.

<sup>1073</sup>*Ibid.*, p. 684.

I do not find the argument that nominal syntax is common in the ritual texts<sup>1074</sup> an adequate basis on which to reject a particular verbal analysis when a good many imperfective forms are actually attested. T.'s treatment of this and various other passages appears to be dominated by a preconceived view of what should be there rather than by close analysis of each case both within its own context and within the broader context of the ritual corpus. T.'s characterization as 'circular reasoning'<sup>1075</sup> of my argument that the form in RS 1.005:2 is imperative because other imperative forms are attested may, of course legitimately be applied to any given case; but as there are good reasons to believe in the existence of other examples, it is his denial of any and all cases that needs to be called into question. This appears all the clearer now that T.'s bias has become obvious in his new treatment of *šqrb* in RS 1.002:26'.

— p. 596 (§74.623), p. 652 (§75.527g). On p. 596, T. analyzes *ttb* in RS 18.287:5' (*KTU* 2.50) and in RS 29.095:8 (*KTU* 2.71) as 2 m.s., on p. 652 as 2 f.s. In both cases, it is the former parsing that is correct.

— p. 596 (§74.623<sup>bis</sup>), p. 652 (§75.527g). On p. 696, T. cites *ttb* in RS 19.158B:6' (*KTU* 2.65:5) twice, first as an example of the m.s. impv., then as gender unknown; on p. 652, the latter classification is indicated. It is the latter that is correct.

— p. 596 (§74.623), p. 652 (§75.527g). On p. 596, T. analyzes *ttb* in RS 18.[482]:2' (*KTU* 2.58) as indistinguishable for imperative m.s. or f.s. In point of fact, the letter may have been addressed to two persons or, for that matter, to a number of persons: in line 5', one finds {tǵrkm<sup>l</sup>}, where {km} may be singular + enclitic *-m*, dual, or plural.

— pp. 596-99 (§74.624). T.'s hypothesis according to which the stem vowel of the Š-stem /QTLa/ was /i/ is based only on contraction phenomena in III-y/w roots: when a relevant form is written {ŠCCy}, /-iya/ is said to be a more likely basis for the preservation of the /y/ than is /-aya/. Given the variable data on monophthongization, however, one may doubt that they provide adequate criteria for deciding the question. For one argument in favor of the Ugaritic form having been /šiqṭala/, see above, remark to p. 518 (§74.231). In favor of /šaqṭila/ one may cite the common tendency in the Northwest-Semitic languages (Hebrew, Aramaic, and Phoenician) to show /i/—which in Hebrew has become /ī/ under the influence of hollow roots. If the D-stem /QTLa/ form showed at least one innovation in the direction of one of these languages (/qittala/ — see remark above to pp. 558-59 [§74.414.1], etc.), it is not implausible to suppose another in the Š-stem.

— pp. 596, 597 (§74.624), p. 670 (§75.537f). T.'s arguments against *šnwt* in RS 22.225:1 (*KTU* 1.96) being from a root ŠNW, 'to rush about', do not take into account the occasional forms of III-w roots that retain the /w/; the most striking example is ŠLW in both Hebrew

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<sup>1074</sup>Ibid.

<sup>1075</sup>Ibid.

and Aramaic.<sup>1076</sup> Ford has provided much comparative material for the interpretation by ŠNW;<sup>1077</sup> I have argued that the two verbs in this line may be participial in form.<sup>1078</sup> — p. 597 (§74.624), p. 635 (§75.512), p. 639 (§75.517a,c), p. 640 (§75.517e). Because neither /yaytin-/ nor /yawtin-/ would give /ʔâtin-/, this vocalization of {âtn}, the attested writing of the 1 c.s. /YQTL/ form of YTN, ‘to give’, and the hypothesis that the /YQTL/-form was built directly off one or the other of these two proto-forms must be considered unlikely. Either the /YQTL/ of this root was constructed on the more widely attested root-form NTN (/ʔattin-/ cf. Hebrew NTN and Akkadian NDN) or else it behaved like other I-y ← I-w roots in reflecting a biconsonantal root (/ʔatin-/).<sup>1079</sup> It is equally unlikely that the Š-stem /QTLa/ would have been /šêtin-/ (← /šaytin-/), one possibility presented on pp. 597 and 640. The first syllable of the causative stem in Hebrew shows formation at a time when I-w roots had not yet shifted to I-y, e.g., /hōwšīb/ ← /hawšaba/ (the vowel of the second syllable is secondary). So, if the proto-Ugaritic root was WTN, as T. considers likely, it is far more plausible that the Ugaritic form was /šôtin-/ (this is the other possibility proposed on pp. 597 and 640). But the hypothesis must be judged just as plausible according to which the Ugaritic Š-stem was formed from the proto-Semitic root NTN (cf. Akkadian /šuddunu/ and the Biblical Hebrew Hophal /yuttan/ ← /yahuntan/—unless that form be a Qal passive, as some grammarians hold). The basic problem with this root is that the Ugaritic consonantal writing system only tells us that the G-stem /QTLa/ differed from Hebrew/Akkadian in showing the form *ytn*, an isogloss with Phoenician. But the Phoenician G-stem /YQTL/ may have been /yattin/,<sup>1080</sup> in which case the Ugaritic form might be expected to have been identical. If so, the Ugaritic *root* was not I-y/w, as T. classifies it; only the /QTLa/ segment of the paradigm was I-y. Until further evidence becomes available, the question must remain open for Ugaritic, i.e., we cannot, on the basis of presently available evidence, know whether {âtn} represents /ʔatin-/ or /ʔattin/, whether {štn} represents /šatin-/ or /šattin-/. — p. 597 (§74.624), p. 640 (§75.517e). T. parses {štn[t]} in RS 17.434<sup>+</sup>:13 (*KTU* 2.36) as a certain example of the 1 c.s. Š-stem of the verb ‘to give’, without mentioning the fact that

<sup>1076</sup>I see no reason to doubt the presence of this verb in RS 2.[003]<sup>+</sup> iii 45 (*KTU* 1.14), though T. would emend the form *âšlw*, ‘I shall rest’, to *tšlw*, which he takes as the Š-stem of LWY, ‘to bind’ (p. 450 [§73.243.22a], p. 590 [§74.622.3], p. 671 [§75.537g]).

<sup>1077</sup>*UF* 30 (1998) 217-18.

<sup>1078</sup>*Ritual and Cult* (2002) 164-65 n. 15.

<sup>1079</sup>Pardee, *BiOr* 34 (1977) 6-7. The vocalization /ʔatin-/ is actually the first proposed by T., on p. 450 (§73.243.22b), where {âtn} is listed with I-y/w 1 c.s. /YQTL/ forms of the type *âtib*, ‘I sit’. In the paragraphs cited at the head of this remark, however, he everywhere vocalizes with a contracted vowel in the first syllable: p. 635 /ʔâtin-/ (1 c.s. G-/YQTL/), p. 639 §a, /yûtan-/ (3 m.s. G-passive /YQTL/), p. 639 §c, /nâtana/ (3 m.s. N-/QTLa/), p. 597 /šô/êtin/ (m.s. impv. Š-stem), p. 640 /ʔašê/ôtin-/ (1 c.s. Š-/YQTL/). In *UF* 31 (1999) 740, T. claimed that YTN is the “Primärwurzel” without explaining why I-n forms appear not only in Hebrew but in Akkadian as well.

<sup>1080</sup>The form {tntn} (2 m.s. /YQTL/) is cited in J. Friedrich, W. Röllig, *Phönizisch-punische Grammatik*, 3. Auflage, neu bearbeitet von Maria Giulia Amadasi Guzzo (AnOr 55; Rome: Pontifical Biblical Institute, 1999) 100, and explained as “Geminatendissimilation” (p. 103); cf. also the Punic N-stem /QTLa/ form spelled {nntn} (ibid., p. 102, explained in the same way on p. 103).

the form has been analyzed as 2 m.s. (Puduḥepa would be chiding Niqmaddu for having sent gold to the Hittite king alone, none to her).<sup>1081</sup>

— p. 597 (§74.624), p. 652 (§75.527g). T. does not mention the possibility that *škn* in RS 16.402:23 (*KTU* 2.33) may be from  $\sqrt{\text{ŠKN}}$ ,<sup>1082</sup> not the Š-stem of  $\sqrt{\text{K(W)N}}$ , as he would have it (on the interpretation of this passage, see above, remark to p. 231 [§42.5], etc.).

— p. 597 (§74.624), p. 713 (§76.525), p. 810 (§85.7c). In the first two sections cited, the text of RS 22.225:2 (*KTU* 1.96) is given as *tp aḥh w n‘m*, while in the third the particle *w* is replaced by *k*, which is said to be a “n[eue] L[esung]” (readings don’t get much newer than that!). The reading with *k* is the correct one.<sup>1083</sup>

— p. 598 (§74.624) p. 628 (§75.45), p. 864 (§93.33c). On p. 598, T. outlines two possible interpretations of the Š-stem form *šs‘n* in RIH 78/3+:24 (*CAT* 2.81), viz., as a simple strengthening of the G-stem (‘to pay’ from NS‘, lit. ‘to extract, i.e., bring forth money for someone’) or as a true causative (‘cause someone else to pay’). The *-n* on the form appears to bother him though, for on p. 598, though he analyzes the morpho-syntax of the first option as “einfach-transitiv,” he translates it as “er mir(?) bezahlt hat” and below, p. 864, classifies this text as an example of the double-accusative construction, though with a question mark. T.’s translation seems to indicate that on p. 598 he was thinking of the rare but well-attested usage of the suffixed pronoun to express an indirect object rather than a direct object. In point of fact, the context in which this phrase (*hn ksp d šs‘n [...]*) appears is too damaged to allow for a certain morpho-syntactic analysis. The three possible interpretations that come to mind are: (1) T.’s apparent analysis as an indirect object (‘the silver that he caused someone else to pay to me’); (2) double direct object (‘the silver that he caused me to pay [to someone else]’ or ‘that he caused him to pay [to me]’);<sup>1084</sup> (3) the *-n* may be a resumptive pronoun (‘the silver which he caused it to be paid’). On T.’s list of double accusative constructions, see remark below to p. 864 (§93.33c).

— p. 599 (§74.625). T. vocalizes *mšnqt* in RS 3.343+ ii 28' (*KTU* 1.15) as a Š-participle f.s. (“*mušēniq(a)t-*”), but parses it as f.du., indicating by his translation “die beiden Ammen” that he considers the participle to have the semantic value of ‘wet-nurse’ (lit. ‘she who gives suck’). The vocalized form may be the correct one, for the referent may be the goddess ‘*Anatu*, not two unmentioned surrogate wet-nurses. (Perhaps T. believes the two breasts of ‘*Anatu*, mentioned in the preceding verse, to be the entities that are designated as wet-nurses, though he does not make this explicit.)

— p. 600 (§74.625). In Biblical Hebrew, *hiqtīl* is not the “Inf.abs.” of the causative stem but the ‘infinitive construct’.

— p. 600 (§74.626.2), p. 601 (§74.626.3b), p. 679 (§75.675). One is astounded to find nary a mention of the generally adopted interpretation of *šmrr* in RS 24.244:4 *et passim* (*KTU*

<sup>1081</sup>Pardee, *AfO* 30 (1983-84) 325, 328.

<sup>1082</sup>Idem, *AfO* 31 (1984) 229.

<sup>1083</sup>Pardee, *Ritual and Cult* (2002) 162 (reading based on collation); cf. J. N. Ford, *UF* 30 (1998) 202 with note 2 and p. 255 with note 176.

<sup>1084</sup>I have placed the indirect object in brackets, but it need not have been expressed in the text if the situation was clear from context.

1.100) as ‘that which causes bitterness/illness, i.e., venom (of a snake)’.<sup>1085</sup> T. prefers to see here a Š-stem form of the verb MRR with the meaning “vertreiben.” The interpretation is possible in context: the bicolon is *mnt ntk nhš // šmrr nhš* ‘qšr, ‘My incantation (against) serpent-bite, *šmrr* of/(against) the scaly serpent’, where *šmrr* could either be parallel with *ntk nhš* or, as T. takes it, with *mnt*. The problem is etymological and grammatical. T. assumes that Ugaritic has a root MRR that is transitive both in the G-stem and in the Š-stem. But that configuration of forms is unlikely and a better solution is available (see remarks above to p. 500 [§73.611.2d], etc., and below to p. 673 [§75.62a], etc.).

— p. 601 (§74.623.3a), p. 652 (§75.527g). Because of his aversion to m.s. imperative forms in the ritual texts (see above, remark to p. 211 [§41.12], etc.), T. must take {ttb} in RS 24.250+:32 (*KTU* 1.106) as an infinitive and emend {tb} in line 23 to {t<t>b} (see remark above to p. 61 [§21.354.1d], etc.). If one be willing to accept the existence of imperative forms in these texts as well as some variation in expression, one can take the {tb} in line 23 as G-passive or infinitive (the solution preferred by T. on p. 652) and {ttb} in line 32 as an imperative addressed to the officiating priest.<sup>1086</sup>

— p. 602 (§74.623.3d). T. is constrained by the structure of the passage to admit that *šmtr* in RS 1.003:2 (*KTU* 1.41) is most likely explained as Š-stem of the root MTR ‘to cut’, but his general refusal to admit second-person volitive forms (see preceding remark) leads him to analyze the form as a verbal adjective (*šmtr ūtkl* would mean “das Geschnittene(?) der Trauben”). Far easier, as most scholars have seen, to take *šmtr* as an imperative: lit. ‘cause a bunch of grapes to be cut’.<sup>1087</sup>

— p. 605 (§74.632), p. 673 (§75.61e), p. 679 (§75.676). On p. 605 T. very correctly remarks, with regard to {yšhmm} in RIH 77/18:7’ (*CAT* 1.175), that for the meaning “erhitzen” one would expect the D-stem rather than the Š-stem, but he does not suggest another interpretation. Since a causative stem applied to a stative verb in West Semitic can function truly causatively, the interpretation ‘to heat’ proposed by the editors<sup>1088</sup> is certainly possible. On the other hand, since the preceding verb bears a {-n} that probably expresses the direct object while this verb does not (see above, remark to p. 506 [§73.634a]), we may be dealing with what is known in traditional Hebrew grammar as an “internal Hiphil,” i.e., ‘he/it will produce warmth = become hot.’ Two further textual remarks as well as one grammatical, one contextual, and one literary remark are in order here. (1) Not only is the first root letter of the preceding verb not preserved (according to the editors, it would be ḤRK, “mettre au feu”<sup>1089</sup>), but, *a fortiori*, neither is the preformative (see above, remark to p. 506 [§73.634a]); there is, therefore, no way of knowing who the subject of this verb was. (2) In his interpretation, T. links the previous verse, where vinegar and salt are mentioned

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<sup>1085</sup>This interpretation goes back to the first major philological interpretation of this text, Astour, *JNES* 27 (1968) 16.

<sup>1086</sup>Pardee, *Les textes rituels* (2000) 590-91, 597.

<sup>1087</sup>*Ibid.*, pp. 149, 151, 159 (with previous bibliography on this interpretation in note 29).

<sup>1088</sup>Bordreuil and Caquot, *Syria* 56 (1979), p. 296 “qu’il le mette au feu et chauffe.”

<sup>1089</sup>*Ibid.*

(*ḥmṣ w mlḥt*), with the verbs in line 7', but there is a lacuna of uncertain length at the beginning of line 7'; though it is extremely likely that the vinegar and salt were in the mixture that is put on the fire, we have no way of knowing exactly what the syntactic relationship was between those nouns and the verbs in line 7' and hence what the concrete relationship was between the entities designated by these terms. (3) In such a broken passage, I see no reason to come down so strongly for the analysis of the form as a Š-passive stem; only on p. 605 does T. even consider the possibility that *yšḥmm* may be in the active voice. (4) T. suggests that the subject of {*yšḥmm*} might be “der Beschwörungspriester,” but it appears fairly clear that instructions to the medicine-man were in the second person while third-person forms were used for the patient. (5) There is nothing in this text as it is preserved that indicates that there was a “Beschwörungspriester”; it may have been a strictly medical text in the same sense as the hippiatric texts or the prose section of the ‘drunkenness of *’Ilū*’ text (RS 24.258:29'-31' [KTU 1.114]).<sup>1090</sup>

— p. 607 (§74.642), p. 609 (§74.646). T. argues that the Št forms of the root ḤWY (*yšṯḥwy*, etc.) should be related to the West-Semitic root meaning ‘to live’, and mean something along the lines of ‘to honor, venerate’, rather than to the verb known from Arabic in the meaning ‘to curl up’, and mean something like ‘to curl up (at the feet of someone), to prostrate oneself’. For this, he adduces two arguments: (1) the verb appears more than once in parallel with KBD in the D-stem, which means ‘to honor’, (2) the verb takes direct objects in two passages (RS 3.367 i 15', 31' [KTU 1.2])—on p. 609 this morpho-syntactic analysis is provided as proof (“nachweisbar”) of the Š-stem taking direct-object complements. These two passages, however, serve better to refute the hypothesis than to support it, for (1) in both *tšṯḥwy* is in parallel with *tpl*, ‘to fall’, which may be taken as an indication that the notion is one of body positioning rather than of abstractly honoring, and (2) it is unlikely that the following phrase *pḥr m'd*, ‘the gathered assembly’, is the direct object of this verb that nowhere else takes direct-object complements, as no other Št-form takes such complements. It must be judged far more likely that *pḥr m'd* is an unmarked adverbial, and the verb-complement combination means something like ‘to do obeisance with regard to the gathered assembly.’<sup>1091</sup> Strangely enough, T. makes no reference to biblical usage of the same verb, of which the meaning is indubitably ‘to prostrate oneself, do obeisance’ for it is regularly complemented by the prepositional phrases *lʔ* ... or *lipnē* ... (‘to’ or ‘before’) and never takes a complement introduced by the definite direct object marker *ʔet*. Because the root in this verbal form appears only in these two languages, a remarkable lexical and morphological isogloss, it would be surprising if the meaning were not similar in the two languages. It appears necessary to conclude, therefore, that the parallelism of ŠTḤWY with KBD in Ugaritic reflects the function of the act of prostration rather than the basic meaning of the verb.

— p. 611 (§75.212.11). T. parses {*tiḥdn*[...]} in RS 88.2159:7 (RSO XIV 51) as 2 m.s. or 3 m.pl. of the G-stem (‘you/they will take’). Because the context is so badly damaged,

<sup>1090</sup>On the relationship between the prose medical prescription and the poetic mythological texts that precedes it, see Pardee, *Les textes para-mythologiques* (1988), ch. 1.

<sup>1091</sup>Cf. Pardee, *Context I* (1997) 246: “do not prostrate yourself (to) the Great [Assembly ...].”

however, there is no way to limit the possibilities to these two: I see no way of ruling out any of the forms with *t*-preformative as possibilities here.

— p. 612 (§75.212.12), p. 643 (§75.522), p. 748 (§81.3c). T. does not state why he considers it more plausible to derive *tūdn* in RS 92.2014:8 (RSO XIV 52) from an unexplained hollow root than as a denominative from /ʾudnu/, ‘ear’, which would mean ‘to hear’ (cf. Arabic with a G-stem denominative, and Hebrew *heʿezīn*, i.e., Hiphil). The presence in the following lines of *hwt*, ‘word’, that is said to resonate in the mouth and on the lips of sorcerers, leaves little doubt that ‘to hear’ fits the context far better than would √(W)D, which is only attested in Ugaritic with the meaning ‘to burden (someone with something)’ (see above, remark to p. 448 [§73.243.1], etc.).<sup>1092</sup> Perhaps T.’s preference is owing to the fact that in the manuscript made available to him, Bordreuil and I vocalized the form /tiʾdanū/,<sup>1093</sup> according to the hypothesis that {ū} was an alternative orthography for syllable-final /ʾ/; and to the fact that, if the root is ʾDN, this would be a jussive form with negative *I* rather than the expected *āl*. T.’s proposals that such writings may reflect secondary opening of such syllables accompanied by vowel harmony with the stem vowel (see above, remark to pp. 33-35 [§21.322.1], etc.) have since made me wonder whether the G-stem denominative verb in Ugaritic was not simply a /yaqtul/ form, a reconstruction rendered plausible by the fact that it takes an accusative complement (/huwāta/, ‘word’, in lines 9, 10). If so, the vocalization would have been something like /taʾudunū/ (← /\*taʾdunū/),<sup>1094</sup> a jussive form with negative *I*, meaning ‘they absolutely must not listen to the word of X’ (on *I* + jussive in this text, see above, remark to p. 514 [§74.222.3]).

— p. 614 (§75.212.2). T. leaves out an important step, or two, when he declares, in comparison with Hebrew ʾōr, ‘give light’, that Ugaritic *ār* developed from a /qta/ base, viz., he assumes the development /ʾwar/ to /ʾār/, as he would represent it (and actually does for this form, which is 2 f.s. and hence “ʾārī”).<sup>1095</sup> First, as noted above in the seventh general remark, there is no evidence from West Semitic for this sort of contraction, and the forms in question must, therefore, have arisen at an earlier stage and by an uncertain process. Second, to the extent that the statement reflects T.’s reconstruction of the G-imperative as showing a consonantal cluster at the beginning of the form (/qtul/qtil/qtal/: p. 426 [§73.121.2]), this form does nothing to prove that such was the form, for T. also reconstructs the /QTLa/ forms as /qâl-/ and as coming from a /qawam-/ base.

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<sup>1092</sup>T.’s oblique reference to {tūzn} in RS 24.289:12’ (*KTU* 4.727) cannot be taken seriously as a parallel, for there are virtually no clear cases of /d/ → /z/ in Ugaritic, as T. himself recognizes (see pp. 106-7 [§32.143.4]). Ford has recently accepted the derivation from ʾDN (← ʾDN), though he takes the benefactor of the incantation as the subject of the verb (“You shall not heed”: *UF* 34 [2002] 120, cf. pp. 137-39). Since he cites no parallels from the otherwise voluminously cited incantational literature for the motif of the *benefactor* not listening to “sorcerous accusations,” such an overall interpretation appears dubious.

<sup>1093</sup>This vocalization is now visible in the publications of the text by myself (*Les textes rituels* [2000] 830) and with Bordreuil (RSO XIV [2002] 387).

<sup>1094</sup>This vocalization has been put forward in Bordreuil and Pardee, *Manuel* (2004) II 69.

<sup>1095</sup>P. 448 (§73.243.1), p. 646 (§75.523).

— p. 615 (§75.212.4). T. here describes as certain the interpretation of *ûḥd* in RS 19.096:4, 5<sup>1</sup>, 9 (*KTU* 4.635) as a G-passive participle in spite of having above (p. 514 [§74.223.1]) qualified that interpretation as only an alternative to taking the form as G-passive /QTLa/. He does not even mention here the possibility of taking *ûzr* in RS 2.[004] i 2' *et passim* (*KTU* 1.17) as a G-passive participle, though that is the only analysis that makes sense to me in that context (see remark above to p. 474 [§73.423]).

— p. 616 (§75.222), p. 617 (§75.223). In both these sections T. proposes to read {w dū} in RS 3.322+ iii 14, 28 (*KTU* 1.19) as {w tdū}, but immediately under this assertion on p. 617 he provides a vocalization and derivation for {dū}, citing line 14 as the only occurrence of the form. This ambivalence toward the proper reading appears to reflect the fact that the subject of this and the preceding verb is plural in line 14 (*nšrm*, ‘the raptors’), singular in line 28 (*hrgb*, ‘Hirgabū’, father of the hawks). T. appears to consider nowhere the possibility that {w dū} would in fact be the correct reading and that the singular and plural forms would have been distinguished by vowel length (/du’uy/ → /du’u/ and /du’uyū/ → /du’û/). The problem with this solution is that the preceding form is apparently a 2<sup>d</sup> person jussive form, *tpr*, and it involves, therefore, form switching within a single volitive sequence: ‘May you/mayest thou fly away! Fly!’. Another possibility to consider is that the form is each time the infinitive in sequence with the preceding finite form *tpr* (da’āyu/ → /da’û/). Of course, at some point the tablet must be collated to determine, if possible, what the scribe actually wrote.

— p. 617 (§75.223). As a G-imperative of L’K, ‘to send,’ T. cites only RS 4.475:10 *lāk* (*KTU* 2.10), ignoring RS 18.113A+B:39' (*KTU* 2.42:27) (see remark above to p. 448 [§73.243.1], etc.) and RS 29.093:13 (*KTU* 2.70) (see above, remark to p. 515 [§74.223.2], etc.).

— p. 617 (§75.223). I am at a loss to understand the notation “/sa/i’(a)dī/ < \*s<sup>a</sup>/i’adī” for the vocalization and derivation of *sād*, ‘serve!’, G-imperative f.s. (RS 2.[004] v 20' [*KTU* 1.17]). Whatever may be going on in the first syllable, the presence of {ā} in the form as written indicates that syncope of the stem vowel has not occurred (compare Aramaic /q<sup>a</sup>tālī/).

— p. 617 (§75.224). T. cites RS 18.134:13 (*KTU* 2.44) as an example of {lik}, 3 m.s. /QTLa/ of L’K, ‘to send’, following the reading of *KTU/CAT*, where all three signs are indicated as certain. In fact only the {k} is certain, the second sign may be narrowed down only to {p, h, i}, while the first sign has completely disappeared. Moreover, the entire context is too damaged to make the restoration of {[i]i’ik} anything more than a guess.

— p. 617 (§75.224). RS 16.401:3' (*KTU* 2.32) is cited as an example of {likt} 2 m.s. /QTLa/, but the context is broken and the form could be 2 f.s. or 3 f.s.

— p. 617 (§75.224). T. parses *rā* in RIH 78/26:11' (*CAT* 1.176:24) as 3 m.s. /QTLa/, which he vocalizes /ra’â/ ← /ra’aya/. Two remarks are in order: (1) again the context is broken, but the presence of *ydk*, ‘your hand’, in the preceding line and *lāk*, ‘send’, in the following,<sup>1096</sup> make the analysis of *rā* as a m.s. imperative + /-a/ just as likely (the

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<sup>1096</sup>T. parses this form, with a question mark, as an infinitive (p. 481 [§73.513.2], p. 618 [§75.227]), which is, of course, always possible.

cohortative and ‘emphatic’ imperative forms of *tertia infirmæ* roots have disappeared from Biblical Hebrew, of course, but such may not have been the case in Ugaritic); (2) in either case, the vocalization may have been simply /ra’a/, rather than /ra’â/, for Hebrew /rā’āh/ (3 m.s. /QTLa/) shows that proto-Hebrew had /a/ in the second syllable, not /ā/.

— p. 618 (§75.225). T. cites Hebrew /šā’al/ as differing from Ugaritic /ša’ila/, ignoring the fact that proto-Hebrew was certainly identical to Ugaritic in this respect, for /QTLa/ forms with pronominal suffixes show the stem /ša’il-/, e.g. /ūš’ēl°kā/, ‘and he will ask you’, or /lō’š’iltihū/, ‘I did not ask him’. The non-suffixed form /šā’al/ is, therefore, simply a token of the tendency in Biblical Hebrew for the primitive /qatila/ and /qatula/ patterns to assimilate to /qatala/. This fact calls into question the hypothesis, proposed by Segert and given serious consideration by T., that the /qatila/ pattern in II-’ roots is a secondary formation by back-formation on the /YQTL/ stem vowel /a/ (i.e., /yiš’al/ shows /a/ stem vowel because of the /’/ but is semantically transitive and hypothetical /ša’ala/ would have become /ša’ila/ because /qatila/ is the expected reflect of /yiqtal/), for it would require that this dissimilation would have occurred in proto-Northwest Semitic. It appears more likely that these forms are retentions from an early stage of West Semitic when /qatila/ forms were, as in Akkadian, more numerous than is the case in later West Semitic, where the innovative verbal system led to that form being retained only for roots that are inherently stative in meaning.

— pp. 620-21 (§75.232). T. is right to stress the importance of III-’ roots for reconstructing the verbal system, in particular the ‘moods’ of the /YQTL/. Perhaps he does not stress sufficiently, however, the difficulties of deriving from the attested forms a clearly definable system of usage in poetic texts—the problems are far fewer in prose texts, but then again there are a good deal fewer examples of reasonably well preserved continuous text in prose. For example, the very first form cited, *tbû*, /tubû’u/, ‘she enters’, occurs in four occurrences of a similar formula, each time preceded by a form of the verb GLY; three times the form of the latter verb appears to agree with *tbû* in that it is written in the long form, {tgly}, apparently for /tagliyu/, but the third token is written {tgl}, which represents either the short form /tagli/ or else a contracted form /taglû/ ← /tagliyu/. T. does face up to this particular problem below in the section on III-y/w roots (p. 658 [§75.531f]), where he considers these two possibilities, as well as others, viz., that {tgly} may be *plene* writing for /taglî/ (T.’s normalization of the ‘short’ form),<sup>1097</sup> that {tgl} may be a simple error for {tgly}, or that the poet may have liked variety. He appears to come down for the last solution, for on p. 659 (§75.532) {tgl} in the idiom in question is taken simply as a ‘short’ form (/YQTLØ/). On p. 692, a paragraph (§76.344) is devoted to “PK<sup>L</sup> zur Ausschilderung bereits genannter Themen,” and the discrepancy between the texts is resolved by the assertion that the use of the /YQTLu/ form was not obligatory in such expressions. One might at least, however, if that were the case, have expected the two verbs in the expression to show the same form in a single passage. Perhaps the poet chose to explain the ‘uncovering’ of the threshold as perfective, the ensuant ‘entering’ as imperfective: ‘she arrived then set about entering’. However, as we shall see below (p. 621 [§75.232]), the verb NŠ’ appears (nearly) always

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<sup>1097</sup>On my disagreement with this vocalization, see above, remark on p. 190 (§33.311.5) and below, remark on p. 658 (§75.531f), etc.

to be in the long/imperfective form in its role of introducing a following verb. The best that one can say is that T.'s explanation requires a good deal of latitude for 'stylistic' variation on the part of the poets. Which explanation is to be preferred, (1) conscious stylistic variation in the use of perfective and imperfective forms (T.'s preferred solution), (2) random morpho-syntactic variation in a system that was no longer understood, (3) simple morphological variation (/tagliyu/ vs. /taglû/), (4) scribal variation (the form was /taglû/ but it could be written either {tgl} or {tgly}, the latter as historical writing), or (5) scribal error (read {tgl<y>} for {tgl})?

— pp. 620-21 (§75.232). To deal with certain unexpected {'}-signs in writings of III-<sup>3</sup> roots, T. suggests that /'/ may sometimes quiesce and the sign be used as a *mater lectionis* to represent the quality of the now final vowel, e.g., {yṣi} would represent /yaṣî/ ← /yaṣî'/ . He does not even mention the possibility that the {'}-signs may occasionally represent the preceding vowel when he /' closes the syllable, i.e., if one does not believe that /ybû/ represents /yubû'u/, one might consider that it represents /yubu'/ rather than /yubu/ or /yubû/.<sup>1098</sup> Particularly problematic in this section is the idea that {yṣû}, 'he lifts', could represent /yiššô/ ← /yišša'/ (on the particular problem posed by this verb, see further remark below to p. 621).

— pp. 620-21 (§75.232 [§75.232]). T.'s vocalization of jussive/perfective forms of III-<sup>3</sup> roots generally shows the stem vowel as short, e.g., {yṣi} = /yaṣî'/ and {yspi} = /yispa'/ . He does, nevertheless, incorrectly vocalize {tbi} with a long vowel ("tabû"), apparently driven by his view that hollow roots should always show a contracted vowel (see above, seventh general remark as well as the remark to p. 37 [§21.322.5a]).<sup>1099</sup>

— pp. 620-21 (§75.232). The two 3<sup>d</sup>-person forms of III-<sup>3</sup> roots that are written with {â} as the last sign are here first vocalized with /-â/ according to the theory that Ugaritic did not have a 3<sup>d</sup>-person /YQTLa/ form (i.e., {â} would be a *mater lectionis* for /â/; on this matter, see above, remark to p. 429 [§73.143], etc.); only as an alternative is the /YQTLa/ vocalization considered. On the possibility of taking these forms as true /YQTLa/ forms, see the remark just cited as well as the remark to p. 456 (§73.263), etc.

— p. 621 (§75.232), p. 734 (§77.51a). T. follows a good number of scholars<sup>1100</sup> in taking {tṣû} in RIH 78/20:2 (CAT 1.169) as a 2 m.s. /YQTLu/ form (/taṣî'u/), translated as an unmarked volitive ("du mußt hinausgehen"), without mentioning the possibility of taking the form as 3 m.pl. jussive /taṣî'û/.<sup>1101</sup>

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<sup>1098</sup>As regards this particular root, it should be noted that in Hebrew the entire Qal /YQTL/ paradigm was formed secondarily on /yabu'/, not on yabû(')/ (see above, remark to p. 37 [§21.322.5a]).

<sup>1099</sup>Equally problematic is the /û/ of "tabû'û-nna," for, in spite of the hyphen, T. is in fact proposing that the form should consist of /tabû'û + nna/, which would have to have become /tabû'unna/ in proto-Ugaritic.

<sup>1100</sup>References in Pardee, *Les textes rituels* (2000) 881, n. 40.

<sup>1101</sup>Pardee, in *Verse in Ancient Near Eastern Prose* (1993) 211 (translation only); idem, *Les textes rituels* (2000) 876 (translation), 877 (vocalization), 881-82 (commentary); idem, *Ritual and Cult* (2002) 160 (rendered in context as "it goes forth"); Bordreuil and Pardee, *Manuel* (2004) II 67 ("de sorte qu'ils s'en vont"), 68 ("taṣî'û").

— p. 621 (§75.232). T., the opponent of 2<sup>d</sup> person forms in the prescriptive ritual texts (see remark above to p. 211 [§41.12], etc.), parses *āl tšû* in RIH 77/2B<sup>+</sup>:19' (*CAT* 1.164) as 2 m.pl. jussive without considering the possibility that it may be 3 m.pl.<sup>1102</sup>

— p. 621 (§75.232). One of the principal problems for understanding the verbal system in poetry is posed by the verb NŠ', 'to lift', for all 3 m.s. and 3 f.s. forms are written with final {û}, apparently an indicator that these are all long forms, i.e., /yišša'u/, etc. Only one class of these usages is noted specifically by T., those that introduce speech ("jewels PK<sup>L</sup> in Redeeinleitung," with a cross-reference to p. 695 [§76.348a], where this usage is discussed in terms of aspect theory). T. specifically identifies other forms as appearing in "narrativer Kontext," but these are not cross-referenced to a section in §76. If a relatively clear systematic usage of the forms existed in the poetic dialect(s), all such forms must be identified as expressing inception of action, viz., 'lifting' something is always followed by a recital of what happens to the object lifted.<sup>1103</sup> The problem with this interpretation, which would be linked to the correlation between the semantics of the verb and the aspectual nature of the verbal system, is that 'short' plural and dual forms are cited as being attested in very similar contexts, that is, *tšû* and *tšá* are used to express identical inception of action (one would expect *tšûn* and *tšán* to correspond to the third-person singular forms *yšû/tšû*). When one verifies the passages, however, one sees that there is in fact only one such irreducible form: in RS 2.[022]<sup>+</sup> ii 16-17 (*KTU* 1.5) the sequence *tšá ghm w tšh* constitutes a narrative break and an introduction to speech. In contrast, if one be willing to take *tšû* in RS 3.367 i 29' (*KTU* 1.2) as a continuation of the preceding speech ('Let the gods lift their heads'), rather than as a narrative presentation of them doing so, as most of us have done ('The gods lift their heads'),<sup>1104</sup> this form would be identified as a jussive rather than as a perfective. In the third example listed here (RS 3.322<sup>+</sup> ii 40 [*KTU* 1.19]), *tšá* is preceded in the same poetic unit by *tmgyn*, with the consonants {yn} that are characteristic of the dual/plural long forms of III-y verbs, and one may conclude that the function of expressing inception of action is borne by this verb rather than by NŠ'. May *tšá* in RS 2.[022]<sup>+</sup> ii 16 legitimately be emended to *tšán* on the principle that 'one swallow does not a summer make'? Such a conclusion would be easier to reach in this case of a III-ʾ root if the data from III-y roots were more systematic but that is not, unfortunately, the case.

— p. 622 (§75.234). T. prefers the reading of RS 3.427:6' (*KTU* 2.1) indicated in my transcription made available to him to that of *KTU*, viz., {mli[...] } instead of {mlix[...]}. He reconstructs {mli[t(x)] } and parses it as a G-stem /QTLa/ form of ML', "voll sein." Hence the form would mean 'I/you am/are full'. In this epistolary text, it might be more plausible to analyze the form as D-stem, 'to fill', perhaps as an imperative, viz., 'I/you have filled' or 'fill!'.

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<sup>1102</sup>Pardee, *Les textes rituels* (2000) 836 (translation), 840 (arguments in favor of the analysis as 3<sup>d</sup> person ).

<sup>1103</sup>This explanation also fits the two occurrences of the Gt of this root in poetic texts: *ytšû* in RS 2.[004] v 6' (*KTU* 1.17) and in RS 3.322<sup>+</sup> i 21 (*KTU* 1.19) precedes the verb YTB, 'to sit', and may express the inception of this act.

<sup>1104</sup>Smith *apud* Parker, ed., *Ugaritic Narrative Poetry* (1997) 100; Pardee, *Context I* (1997) 246.

— p. 622 (§75.234). T.’s analysis of *qrān* in RS 2.[022]<sup>+</sup> i 23 (*KTU* 1.5) as G-stem /QTLa/ 3 m.s. will not meet universal acceptance: recently Smith<sup>1105</sup> and I<sup>1106</sup> have independently analyzed the form as an imperative (it would be the extended form with 1 c.s. pronominal suffix ending, /qara’ani/, ‘invite me!’).

— p. 624 (§75.32 *√hdy*). I first noticed here the abbreviation “PLKF” which is not to be found in the list of abbreviations (see also below, remark to p. 659 [§75.532]).

— p. 624 (§75.32). T. alters the options of reading RIH 78/26:25’ proposed in *CAT* (text 1.176:11): the latter read {yhḡ/mb/š} whereas T. reads {yhm/gš/b/y}, proposing that the root of the form might be HMY ‘to flow, pour out’. The third sign is without question, however, {g} and the fourth probably {b} (a trace of one of the lower wedges seems to be preserved).<sup>1107</sup>

— p. 625 (§75.32), pp. 677, 678 (§75.673). On p. 625, *yhr̄rm* in RS 2.[012] i 39’ (*KTU* 1.12) is parsed as D- or G-stem, on p. 677 as L- or D-stem; on p. 625, *hrr* in col. ii, line 9, of the same text is parsed as D-/QTLa/ 3 m.s. or G-infinitive, on p. 678 as L-/QTLa/ 3 m.s. Since there is not a single convincing example of a G-stem finite form of a geminate root showing two tokens of the geminated consonant in the writing, that analysis of *yhr̄rm* must be rejected. And because the meaning is intensive rather than factitive (‘to show excitement for’ ← G-stem ‘to be warm’), the analysis as L-stem must be preferred to that of the D-stem (see above, remarks to pp. 575-76 [§74.50], to pp. 577-78, 678-79 [§74.511a, b], and to p. 580 [§74.511c]).

— p. 625 (§75.332). The analogy with Hebrew makes it unlikely that *ylk*, ‘he goes’, was derived directly from /yahlik-/, as T. proposes here. The analogy itself is rendered plausible by the general similarity of the paradigms, where, in both languages, the /YQTL/ forms, the imperative, and (one of) the verbal noun(s), viz., /likt-/, all behave precisely like one category of I-y roots, the *yēšēb* type. For Ugaritic, this is illustrated by the 1 c.s. /YQTL/ form, written {ālk} (≈ {āt̪b}, ‘I sit’, ≈ Hebrew *’ēlēk/’ēšēb*), all of the imperative forms, which show only the second and third root consonants ({lk} ≈ {t̪b}, ‘sit!’, ≈ Hebrew *lēk/šēb*), and the aforementioned verbal noun ({lkt} ≈ Hebrew *léket/šébet*). These similarities make it likely that, in both languages, the full paradigm of HLK is suppletive, rather than showing direct derivation of all forms from the single root HLK. For the irregular contraction that the latter hypothesis assumes, see above, remark to p. 160 (§33.142.3b). Curiously, T. concludes on p. 632 that old I-w roots show a contracted vowel in the first syllable (i.e., /yât̪ib-/, on this question, see below, remark to pp. 631-32 [§75.511e-g]), but does not even broach the question for HLK (nor for HLM—see next remark), simply assuming a vocalization /yalik-/. The Hebrew data would indicate that as went YTB, so went HLK.

— p. 625 (§75.332). The problem of *hlm/ylm*, ‘to strike’, is, on the other hand, more difficult to resolve because Hebrew and Ugaritic here show different paradigms: though only

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<sup>1105</sup> *Apud* Parker, ed., *Ugaritic Narrative Poetry* (1997) 142

<sup>1106</sup> *Context* I (1997) 265.

<sup>1107</sup> Pardee, “RIH 77/27, RIH 77/12, RIH 78/26 et le principe de l’écriture cunéiforme alphabétique,” forthcoming in *Syria* 79.