
The disadvantage of a dead language attested by a small corpus of texts is that it leaves many lacunae in our knowledge of text-dependent aspects of the culture that are revealed by that language; the advantage for the grammarian is that it permits an exhaustive inclusion of the data in a grammatical description. This T. has done in the volume under review, which explains why one of the more poorly attested of the ancient Near-Eastern languages now boasts one of the thickest grammars. The disadvantageous side for the grammarian wishing to exploit these data exhaustively is that, for many grammatical categories, they are very few, all too often a single datum or none at all. The danger is always lurking, therefore, that any given datum is for one reason or another atypical or misunderstood and that a rule be proposed that in fact has no basis.

In a relatively brief time, the author has established himself as one of the principal authorities on the ancient Semitic languages, with forty-nine titles cited under his name in the bibliography, dated from 1989 to 2000, including three important monographs. Though he is familiar with the major Semitic languages, indeed has published on several of them, he has concentrated on the Northwest-Semitic languages and particularly on Ugaritic. His grammatical work has always been of the highest order and all who will in the future have reason to investigate any aspect of Ugaritic grammar will do well to start here. I have one general caveat to state and that has nothing to do with the author’s abilities but reflects rather a choice and one with which I have no dispute: in order to write so exhaustive a grammar, the author must have taken a decision on the meaning of every text in the corpus and on every word in every text from which data are cited, something that even someone so obviously brilliant as T. cannot expect to have done so early in his career with equal thoroughness everywhere. This human limitation is exacerbated by the current state of Ugaritic epigraphy, which I have described on various occasions:¹ the simple fact that many

¹See, for example, my review of the collection of Ugaritic texts by M. Dietrich, O. Loretz, and J. Sanmartín, The Cuneiform Alphabetic Texts from Ugarit, Ras Ibn Hani and Other Places (KTU: Second, Enlarged Edition) (Abhandlungen zur Literatur Alt-Syrien-Palästinas und Mesopotamiens 8; Münster: Ugarit-Verlag, 1995) in JSS 42 (1997) 132-37, and my critique of some of T.’s epigraphic observations with regard to this collection of texts in AuOr 16 (1998) 85-102. The 1995 collection will henceforth be abbreviated CAT; the first edition of this work, which dates to 1976 (Die keilalphabetischen Texte aus Ugarit, Alter Orient und Altes Testament 24/1, Kevelaer: Butzon & Bercker, Neukirchen-Vluyn: Neukirchener Verlag), will be referred to here as KTu. T. cites Ugaritic texts only by the numbers assigned to them in KTu and in CAT. New-comers to Ugaritic studies should be aware that new texts were included in CAT; for example, in section 1, texts 162-176 are new in CAT. Moreover, texts already included in KTu sometimes, though quite rarely, have been assigned different line numbers in CAT; the most conspicuous case is perhaps RS 24.251⁺, of which the recto/verso orientation was reversed in CAT—hence a reader checking a reference to “1.107” will not find the text in the same place in KTu as in CAT. For these reasons, I prefer to cite texts that appeared in KTu as “KTu …”, those that were added in CAT as “CAT …,” and to use “KTu/CAT” when a point is being made with regard to a feature shared by the two editions. T.’s convention is to cite texts by number only, with the primary reference being to CAT; if he wishes specifically to make a point regarding one edition or the other, he cites them as “KTu¹” and
of the Ugaritic texts still await an authoritative publication based on autopsy makes it certain that any study such as this one will include faulty data. That does not mean, however, as I said in my first description of the situation, that philological study “must grind to a halt” while the time-consuming work of re-editing the texts goes on. It only means that the author and his readers must not expect perfection. In all honesty, the epigraphic improvements that remain to be made will in all likelihood not affect T.’s understanding of any major aspect of Ugaritic grammar, but will have to do rather with moving pieces of data about and with filling in the picture. Thus I would have been the last to counsel T. to hold off publication of his work until the epigraphic picture were clearer. So thorough a status questionis was needed by all of us who work on Ugaritic, not least the epigraphers, for even the most basic epigraphic work must at times take into consideration both what is known epigraphically and the interpretations that these data have received. I would thus not disagree with T.’s assertion that a grammar such as this one is useful as a “Grundlage für eine fundierte Übersetzung der inhaltlich schwierigen Texte” (p. 6), though users must not forget that this grammar, like any other original grammar (as opposed to those that simply feed off of their predecessors—which is certainly not the case here!), is based on the author’s own analyses of the texts.

The counterpoint to this problem of not having been able to analyze every text with equal thoroughness is that T. has analyzed all the texts from a strictly grammatical perspective and has built up a reservoir of grammatical data previously unequaled. He thus will have grammatical insights, even on texts on which he has spent less time than the epigraphers/philologists, that the latter may not have perceived. This was brought home to me when T. pointed out to me some time ago that the only prefix attested in all prose texts and most texts in poetry for 3 m.pl. /YQTL/ forms is t-; because both y- and t- forms are attested in poetry, I had assumed that such should be the case in prose as well. But, at least with the present corpus of prose works, such is not the case. Thus my first interpretation of yrdrn g®rm in RS 24.256:18 (KTU 1.112) as a plural on analogy with t′r bn g®rm in RS 1.005:9 (KTU 1.43) has had to be modified. T.’s work is a descriptive grammar that follows the time-honored pattern: script/writing (“Schriftlehre”), phonetics, morphology (including morpho-syntax), and clause/sentence

“KTU2”; if he wishes to express explicitly that a reading appears in both editions, he cites them as “KTU1/2.”


3Throughout this review, I will refer to the various verbal forms by use of the root QTL enclosed in slashes, with the necessary specifications for more narrowly defined forms, e.g., /YQTL/ = ‘prefix-conjugation’, /QTLa/ = ‘suffix-conjugation’, /QTLØ/ = ‘prefix-conjugation with zero vowel at the end’, /yaqtul/- = ‘prefix-conjugation with stem vowel /u/ and no specification for mood’, etc. Nominal bases will usually be indicated in lower case with the vowel(s) indicating the pattern, e.g., /qatl/ or /qitªl/.

4T., pp. 432-38 (§73.223.3).

5See my Ritual and Cult at Ugarit (Writings from the Ancient World 10; Atlanta: Society of Biblical Literature, 2002) 101 n. 24.
syntax; morphology is presented in five sections, numbered as principal sections, viz., the
pronoun, the noun, number words, verbs, and particles. The body of the work consists of an
introduction, followed by these eight sections, for a total of nine. Then thirteen indices
occupy over 140 pages: general abbreviations, sigla and symbols, literary abbreviations,
bibliography, topical index, Ugaritic roots, non-Ugaritic roots, Ugaritic words in alphabetic
script, words in syllabic (Sumero-Akkadian) script, text references by KTU/CAT numbers,
text references by excavation number (“RS …” — this index is broken down into two
sections, one for alphabetic texts, the other for syllabic texts), references to texts not citable
by either of the two preceding systems, and, finally, references to texts from other sites.
This is, therefore, a reference grammar in the best tradition, with the indices provided to
make it useful (though a caveat is necessary on these — see below).

One aspect of this book that deserves special praise is the extent to which the data from
the administrative texts have been integrated into the grammar. T. is not simply aware of the
existence of these texts, but has studied them in detail, has elsewhere already made
significant contributions to their interpretation,6 and makes them a major part of this
grammar. Gone are the days when a Ugaritic grammar was devoted primarily to the
mythological texts.

Another source of data exploited significantly here are the syllabic writings of Ugaritic
words. The basic work along these lines was done by J. Huehnergard7 and W. H. van
Soldt,8 but T. is at pains to include all such data and to draw conclusions from them for
Ugaritic phonology. I have some reservations about the precision with which these data may
be used, but there is no doubting their importance.

The section that I found to be most innovative was that on the phonetic realization of the
consonants (§32.1, pp. 90-133), where he allows for more irregular correspondences with
cognates in other languages than is usually done. Though not all of his examples are
convincing (see remarks below to pp. 109-10 and 110) and though his (necessary) reliance
on the existing editions means that some forms cited simply do not exist, in general this
escape from neo-grammarian rigidity, supported by the very detailed listing of the data for
his positions, can only be praised.9 On the other hand, one would have wished that a more
thorough classification of the aberrant data according to text and context had been made: a
few Ugaritic texts show peculiar orthographies (as T. notes on occasion) and the data from

6I think particularly of his elucidation of the syntactic function of kbd in these texts (UF 29 [1997] 661-62)
which was of great help to P. Bordreuil and myself in interpreting some of the texts from the 1994
campaign. T.’s treatment of complex number phrases that include kbd (pp. 349-63 [§§62.2-62.86], pp. 388-
414 [§§69.1-2]) is one of the strongest parts of the present work as well as one of the most useful for those
who work with administrative texts.


8Studies in the Akkadian of Ugarit: Dating and Grammar (Alter Orient und Altes Testament 40; Kevelaer:

9For a detailed presentation of the data on non-standard phonetic correspondences across the Semitic
languages, see the University of Chicago dissertation by Douglas L. Penney, Towards a Prehistory of
these texts should have been cited separately from those from other texts and an attempt should have been made to determine the reason for the usages peculiar to a given text.

Another area that deserves particular mention here is that of vocalization. The Ugaritic writing system being basically consonantal in character, the phonetic system of the language may be determined by three types of data: those furnished by (1) the use of the three `alif-signs (in addition to sign #1, which corresponds to `alif in the other West-Semitic alphabetic systems, the Ugaritic alphabet has two other `alif-signs; all three express, in various ways, the vowel associated with the consonant `alif), (2) the writing of Ugaritic words in syllabic (Sumero-Akkadian) script, and (3) comparative Semitics. The use of these three types of data has developed slowly over the history of the study of Ugaritic, and some still mock attempts to present a vocalized version of a Ugaritic text, apparently misunderstanding the nature of the exercise as heuristic, pedagogical, and as a form of shorthand for expressing the vocalizer’s understanding of the grammatical structure of a text.\footnote{See Pardee, \textit{JAOS} 117 (1997) 377-78.} We have come a long way just since I began proposing a vocalization for a text under consideration, some twenty-five years ago,\footnote{I remember Moshe Held, ‘z, responding to such a proposal at an American Oriental Society meeting in the mid-seventies with the observation that “not even the great H. L. Ginsberg dared to vocalize a Ugaritic text.” As G. R. Driver (among others!) was wont to observe, \textit{dies diem docet}.} and T. has taken the process one step further by offering proposals for virtually all of the systematic facets of the language (i.e., the entire verbal side of the grammar and the systematic part of the nominal side, primarily the case system) and for most of the less-systematic facets (the stem of individual nouns, adjectives, adverbs, and particles of non-nominal origin; the stem vowel of the G-stem /YQTL/ for individual verbs). Segert had already begun the process in a similar way,\footnote{A \textit{Basic Grammar of the Ugaritic Language} (Berkeley: University of California, 1984).} but, because of the very succinct nature of that work, his presentation was essentially apodictic. This is not the case with T., who often indicates the basis of each proposal, including references to hypotheses that he does not accept. The reader not fully versed in Ugaritic studies must realize, as T. himself intimates, that, by the very nature of the exercise, the degree to which any given proposal corresponds to Late-Bronze-Age-Ugaritic reality will vary considerably. For example, it borders on absolutely certain that the Ugaritic common noun for ‘king’ was pronounced /malku/ because we have a syllabic attestation in a “polyglot” vocabulary where the word corresponding to ‘king’ in the other columns is written {ma-al-ku} in the column representing Ugaritic (for a criticism of T.’s treatment of this particular example, see below, remark to pp. 253, 254 [§51.41b]). At the other extreme, T. chooses (pp. 543-46 [§74.41-74.412.15]) to ignore the data provided by the `alif in 1 c.s. /YQTL/ verbal forms of the D-stem and the Š-stem, all of which indicate that the vowel of the preformative was /a/, in favor of those from the participle (D-stem /muqattil/) and from other Semitic languages (Akkadian and Arabic), in favor of a vocalization /yuqattil/; he explains the 1 c.s. form as arising from vowel harmony but only when following `alif (i.e., the prefix
vowel assimilates to the first stem vowel: /'uqattil/ → /'aqattil/). 13 Some would consider it more plausible that the participle and the finite forms may have had different preformative vowels and that the data from the 1 c.s. forms should outweigh those from other languages. Finally, there are grammatical categories, e.g., the energetic ending /-anna/ after the dual morpheme /-ā-/ for which a vocalization is proposed without any Ugaritic data whatever (see below, comment on p. 177 [§33.221], p. 499 [§73.611.2]); such is also the case very often for the vocalization of individual nouns. Whether one agree with the procedure or not, it must be admitted that T. has valid linguistic reasons for his stance and that he usually presents his case clearly and fairly; only time will tell—assuming that a vocalized form appears eventually in syllabic script—whether or not he is correct in any given case. Some will legitimately doubt, however, that hypotheses for the vocalization of nearly every Ugaritic form should have been presented, whether or not data from Ugarit inform the hypothesis, in a grammar of the Ugaritic language. It may also be doubted whether grammatical rules should have been formulated on the basis of words of which the Ugaritic vocalization is purely hypothetical (e.g., pp. 190-93 [§33.312]—cf. my comments below to this section). However that may be, those not well acquainted with the Ugaritic data (whether philologists with a specialty in another area or comparative Semitists or linguists of whatever persuasion) must be warned to read carefully every paragraph pertaining to whatever topic interests them in order to ascertain the basis for any given vocalism. (In this respect, one would have wished that the indices were more detailed—see fourth general remark below.) In short, the cause of vocalizing Ugaritic might have been better served by a more conservative approach to the exercise or by one wherein the basis for the proposed vocalization is indicated explicitly in every instance—which would have made of an already long work an even longer one.

Finally, T. must be praised to the high heavens for the care with which this work was proofread. One finds an occasional material error—nobody is perfect!—but, considering the number of data treated here, the number is very small. One may or may not agree with the conclusion drawn, but the data and the secondary references are almost always correctly cited. Unfortunately, where a text number is cited incorrectly in the grammar, the incorrect reference is, as is to be expected, indicated in the index (e.g., KTU/CAT 2.31:55 [RS 16.394] is cited as “2.31:54” on p. 171 and is so indexed on p. 1027; on the same page of the grammar, the syllabic text RS 20.123+i 20" is cited as “20.123+:I:3 ” and is so indexed on p. 1051).

Having sufficiently expressed my admiration for this work, I will now pass on to some more critical remarks, most of which will express either disagreements regarding

13T. recognizes (p. 175 [§33.215.21b]) that the explanation provided for the D-stem, viz., that the /a/ in the 1 c.s. form arose from vowel harmony with the first root vowel (/'uqattil/ → /'aqattil/), and which is applied also to the Š-stem (pp. 587-88 [§74.622.1]), is not borne out by the form {āṣld} /'āṣōlid/ (RS 2.002:65 [KTU 1.23]). He considers rather that the {ā} in the 1 c.s. form represents vowel coloring in what in Hebrew or Aramaic would correspond to a reduced vowel with /a/-coloring owing to the /ṭ/ belonging to the family of guttural consonants. That the feminine morpheme could occur as /-at-/ or as /-t-/ is clear, but data for true vowel reduction in Ugaritic are hard to come by (see remark below to p. 146 [§33.115.44.5] on the difficult case of {ša-āl-tu4}).
grammatical or other interpretations or else epigraphic matters. There are fewer remarks of
the latter kind than would have been the case if T. had not requested of P. Bordreuil and
myself the transcriptions of the texts that we have collated as well as the manuscript of the
up-coming publication of the Ugaritic texts from the excavations of 1986-1992. (At the time
when T. was completing his work, viz., late 1999, our manuscript of the texts from the 1994
and 1996 excavations was judged too preliminary to be made available to him.) What this
means is that I can remark on cases where T. has judged best not to adopt our reading
(primarily ritual texts and letters), on texts that I have collated since the collection of
readings was put together that was sent to him (primarily administrative texts), but cannot,
of course, say anything about the multitude of texts that I have not yet collated, but which will
surely contain as many surprises as have those that have been examined to date. That being
the case, a certain number of data analyzed by T. will prove in the long run to be non-
existent or to belong to a section of the grammar other than the one where they are presently
located. In this context, the reader should take particular notice of T.’s method of
representation of texts as found in KTU/CAT: he does not consistently indicate the
epigraphic state of the text cited, e.g., on p. 189 (§33.311.3b; cf. p. 259 [§51.42f]) the word
“ḥrṭ” is analyzed and an argument is made for its vocalization, but no indication is provided
for the reader that all three signs were indicated as damaged by the authors of KTU/CAT in
the only putative attestation of the word (RS 2.[004] ii 41’ [KTU 1.17]).

— First, a general remark on T.’s manner of expressing the plausibility of a given
interpretation. He commonly rates plausibility on three levels: examples may be (1) simply
listed, (2) listed with a question mark, or (3) relegated to a separate section at the end of the
paragraph entitled “Anm[erkung(en)]” where proposals within that category are refuted.
This three-way system is occasionally expanded to four by attaching two questions marks to
an example. Users should be aware that criteria for inclusion of examples under a given
category tend to be maximalist rather than minimalist and that question marks are not used
as frequently as they might be. Indeed, after a long and thorough exposure to the work, I
conclude that T.’s criteria for classification vary considerably. At one extreme is the desire—
explicit or implicit—to include most suggestions that have ever been made for the
interpretation of a given Ugaritic word. At the other is the need to classify everything,
including poorly attested words or those of which the epigraphic basis is uncertain. For this
reason, this grammar is both a review of what has been previously proposed and a reasoned
presentation of the data provided by the Ugaritic texts themselves—but not with equal-
handedness. At the one extreme I have been astounded by the amount of space devoted to
a highly questionable phenomenon. For example, the suggestion that qrwn in RS 24.277:11’
(KTU 1.127) would be phonetic variant of qrbn and mean ‘offering’ is given its own six-line
paragraph on p. 156 (§33.137.1) in spite of the fact that T. himself considers the analysis
unlikely (see remark below on this entry). At the other extreme, many forms are given a
single analysis with no mention of other possibilities. Because I do not have in my head an
exhaustive catalogue of what all scholars have said about every text, most of my remarks
below criticizing these cases of tunnel vision will concern my own published interpretations,
but I have checked sufficient cases in the broader literature to be certain that it is not only my
own interpretations that have been ignored here. Moreover, many of T.’s own classifications
of forms are presented as sound when in fact they are in fact questionable or even far-fetched—many of my remarks below will touch on such points. A particularly striking example is provided by the presentation of \{y\} as a *mater lectionis* (see my discussion below, note to pp. 37-38 [§21.322.5], etc.): all possible examples are cited with only the occasional remark that a given form may in fact be an example of the post-positive particle *y* (treated later in the book, pp. 833-35 [§89.3]). Finally, space is far too often devoted to interpretations that T. himself abjures at some other point in the grammar (a striking example is found on p. 377 [§65.132]—see remark below) and that should, therefore, have been entirely omitted or, at most, placed in an *Anmerkung*. Though there is no doubt that T. is an insightful grammarian, this grammar was far more maximalist than I had expected, to the point that a few entire categories, such as the \{y\} *mater lectionis* or the double writing of a geminated consonant (see remark below to p. 32), are given far more space than they merit. In these two cases, an equally strong—I believe stronger—argument could be made for reducing the former to the brief mention of some disputed instances and for including the latter only to refute it. Other apparent cases of over-inclusion are owing to the fact that the decision was made to write this book without footnotes; thus refutations of positions that T. himself does not hold must take place in the body of the text, usually in the section entitled “*Anm*[erkung(en)].” Despite the availability of this special section for the treatment of rejected interpretations, some views that T. himself considers highly questionable are presented in the main body of the text (e.g., §33.137.1 [p. 156], or §65.132 [p. 377], both mentioned above and commented on below). One nice thing about the use of footnotes is that they allow a presentation of primary data and interpretations on one level with a secondary level of discussion going on in footnotes; this allows the reader to follow the principal line of argument and to move to the second level only as desired. However that may be, T.’s inclusiveness makes of this work in some respects more of an encyclopedia of grammatical interpretations than an ordered descriptive grammar according to the author’s own view of things. All the more astounding, then, as is observed in the following remark, that reasoned views of seasoned Ugaritologists are so often left entirely out of consideration.

— A second general remark goes in the opposite direction from the preceding: I must point out the all-too-frequent non-encyclopedic nature of the analyses presented here. Call me over-sensitive, but I have been astonished at the number of times that an interpretation that I consider to be the best currently available is not even mentioned (e.g., that of \{tmtt\} in RS 18.031:16, 22 [KTU 2.38]—see below, remark to p. 250 [§51.3g], etc.). Any grammar is based on the grammarian’s analysis of the texts that are the basis of the grammar and of course T. has a right to his; but when so many far-fetched interpretations are mentioned, whether refuted or not, whether preferred or not, the absence of some that are not so far-fetched must elicit surprise.

— Third, there are multitudinous cases where T. shows indecision, giving a particular analysis or gloss in one place, another in another place. For example, \*any*- in RS 18.031:10 is two times said to mean “Schiff” and it would be the singular of \*anyt* (p. 286 [§52.5f], p. 703 [§76.521.3]), whereas elsewhere it is translated “Schiff(e), Flotte,” i.e., with possible interpretations either as a singular or as a collective (p. 192, §33.312.32b, p. 254 [§51.41b], p. 196 [§33.322.3b], p. 569 [§74.423]; on p. 195 [§33.322.2b], the same word from line 24 of
the same text is translated as “die Flotte,” with no indication of another possible meaning). In the case of RS 18.031, it appears unlikely that the king of Ugarit would have sent a single ship to Egypt for grain and one may legitimately posit that Ugaritic, like Hebrew, had a collective noun any, a nomen unitatis anyt, and a plural anyt (on the form of any, see below, remark to p. 192). Or, in a single section (p. 237, §43.2d), the term mdr glm is translated “m.-Leute” at first citation, “Schwertträger/Wachleute(?)” at the second. These presentations of alternative interpretations—acceptable up to a point, especially in the case of a language with a corpus that is so small and so poorly understood—are particularly frustrating to the reader when the multiple entries are not cross-referenced and/or do not bear a question mark. For example, the case of YR≥ in RS 2.[009]+ vi 30 (KTU 1.6) appears in eight entries of this grammar, seven times as {yrù}, three times as {yrà}, with the total of ten coming from the mention of both readings at two of the entries; in the other five entries, only one or the other of the two forms is mentioned and none of the entries is cross-referenced to the others by page or paragraph number, not even the one where the new reading {yrà} is proposed (p. 66 [§21.362]). For another example, {îdn} in RS 15.007:5 (KTU 2.15) is confidently translated as “Erlaubnis” on p. 101 (§32.142.32), whereas on p. 429 (§73.142) it is represented by “…(?)”14. Fortunately, these alternative interpretations usually reflect genuine options that arise from one level or another of ambiguity in the text; one does, however, occasionally encounter mutually exclusive interpretations presented in different sections of the grammar (see remark below to p. 51 [§21.341.21a], etc., or to p. 500 [§73.611.2d], etc., on GL). This style of presentation requires the reader to go to the text index to find all mentions of the form(s). Many of my comments below reflect this indecisiveness which becomes the more frustrating the more one uses the grammar. The ideal would have been to present in the most logical place all the data for a given feature, to reach a decision in that paragraph or section as to the most likely conclusion for Ugaritic, then to base all references to the feature in question, whether they actually precede or follow the point where the decision was reached, on that decision. It must be stated forthrightly that, because of the maximalist policy of inclusion, this grammar must be used critically and intelligently—slapdash use could bring about a revival of uncritical interpretations that would be nearly as harmful to future progress in Ugaritic grammar as some of the less well-founded approaches of the past.

—A fourth general remark, on the indices: though the text index is nearly exhaustive, the others are not and all are time-consuming to exploit. The first use to which I put this grammar was in preparing a study of RS 1.012 (KTU 4.14)15 and, while following up various references that were in the indices, I came across three that were not: hmr in RS 1.012:6, 12, 18 is interpreted as a measure on pp. 255 and 411 (though only the occurrence in line 18 is cited on p. 255), but this hmr is neither in the root-index nor in the word index; the word n‘r, which I restore in RS 1.012:7 and which is attested as a foodstuff of some kind

14In the first section cited, {îdn} appears without a textual reference but, as {îdn} is to be found only in RS 15.007:5 and as several scholars have proposed the meaning ‘permission’ for the word in that passage, one must assume that T. had that passage in mind when citing the word.

in three other texts, is twice translated “Rostmehl” on p. 411 but does not appear in either of the aforementioned indices; in the text index (i.e., at KTU/CAT 4.14), there is no reference at all to line 6, though the analysis of hmr on pp. 255 and 411 may eventually be located by going to the entries for lines 12 and 18. As regards hmr, since an important subsection of the grammar is devoted to the use of measure-words with cardinal numbers (§69.2, pp. 408-14), one would have expected at least the word hmr in this sense to have been included in the index of Ugaritic words (on hmr as a measure, see remarks below to p. 123 [§32.146.23a], etc., and to p. 137 [§33.112.32], etc.). I have found that, in general, the text indices are relatively complete, while the index of Ugaritic words is woefully inadequate, e.g., ḥār, ‘brother’ (see below, comment to p. 51 [§21.341.21], etc.), is not to be found at all in the index of Ugaritic words (only ḥâr, ‘marsh’; ḤJW, the hypothetical root of ḥār, ‘brother’, is in the index of roots, p. 950). In order to find most references to a given Ugaritic word in this grammar, one must go through the text index, which is usually not an easy thing to do, for the text index will, of course, list all citations of a given line of text and one must, therefore, plow through all such citations to find the one or ones that deal with given word in the line of text. For commonly attested words, resort to the text index is impracticable,16 and there is, therefore, no easy way of locating where the word may be discussed. A grammar cannot, of course, be expected to play the role of a dictionary; but one can expect all cited texts to be included in the text indices and, since the work will be consulted by many who are not professional Ugaritologists and who might be interested in the Ugaritic form of individual words, the word index should have been more complete. The problem posed by the inadequacy of the indices is compounded by T.’s very thoroughness (a given word may be treated in various sections of the grammar, according to whether its orthography, phonology, morphology, or syntax is at issue) and compounded again by the indecisiveness described above (finding one analysis of a word, phrase, or passage is no guarantee that the same analysis has been indicated elsewhere): it is a source of frustration for the reader to believe that T.’s interpretation of a given word or passage has been understood, only to find that in another section of the grammar another interpretation is assumed, often without a cross-reference to the other interpretation. The interpretations of passages are usually fairly easy to ascertain by the use of the text index; those of individual words or roots are, however, as already indicated, more difficult to discover. Finally, it should be noted that the indices send the user to a page, not to a numbered paragraph; thus the user has to scan a larger body of text to find a given passage than if the indexing had been by paragraph numbers (relatively few numbered paragraphs cover more than one page). It would have been to the user’s benefit for the references to be to paragraphs, with page number added if the paragraph extended beyond a single page.

— Fifth, T. is very conservative in his treatment of the syllabic data: the sign {me}, for example, is taken as /me/ and not /mi/, i.e., {mi}; variant syllabic writings are taken to represent actual phonetic variants in Ugaritic. This is not at all my field of expertise, but I

---

16It is also fraught with the difficulty created by T.’s inconsistent treatment of data; for an example, see below, remark to p. 256 (§51.41f), etc., where it is pointed out that T. has himself proposed that the primary passage cited on p. 256 for the word um, ‘mother’, in fact does not contain that word.
must confess to remaining dubious about some of the reconstructions of Ugaritic that are based on a single syllabic form as well as about supposed Ugaritic phonetic developments based on one type of form when variants are attested. I am not yet convinced that the scribes used the syllabic system with the phonetic precision that T. assumes (see, for example, remarks below to p. 88 [§31.2], p. 146 [§33.115.44.5], etc., p. 172 [§33.214.1], p. 182 [§33.242a], etc., and p. 185 [§33.243.2]). I am also puzzled by how T. can list syllabic writings of which the meaning, hence the etymology, hence the phonology are uncertain, under very precise phonological headings (e.g., on p. 196 [§33.322.41a], RS 16.246:6 [PRU 3, p. 95] {ḥa-a-P1}, listed as a retention of /āy/ followed by either a long or a short vowel: though his presumption that the vowel in the first syllable is /ā/ is certainly plausible, the syllabic writing does not in fact allow certainty as to whether that /a/ was long or short, whether the /y/ was geminated or not, whether the word is Ugaritic or not, or even whether there was a weak consonant between the two /a/-vowels indicated in the syllabic script). Though T. is correct in not using proper names for deriving Ugaritic grammar, he could have studied the syllabic writings of at least the Ugaritic proper names and tried to determine therefrom with what consistency the various signs correspond to Ugaritic phonemes. Had he done so, he may have begun to wonder whether the syllabic writing of common vocabulary is any more reliable for determining the fine points of the vocalization of Ugaritic words. Finally under this heading, T. also assumes that variant Akkadian writings of inflectional elements must be interpreted as expressing variants in Ugaritic inflection. Though the Akkadian texts from Ras Shamra do not show the same baroque plethora of variety as one encounters in the el-Amarna texts, it nevertheless appears plausible to draw the lesson from those texts that West-Semitic scribes did not write Akkadian without influence from the local language. Hence, if in a given case the data reflect what may be Akkadian phenomena alongside Ugaritic phenomena, it appears implausible to attempt to interpret all the data as reflecting Ugaritic grammar (for a specific example, see remark below to p. 304 [§54.112.2]).

六，一个一般性的注释在处理数据时，这些数据数量很少。古老的谚语‘一只燕子不能构成一个夏天’必须被记住，当处理一个语言，这个语言像乌加里特语一样少被研究。在这本语法中有很多‘夏天’，如果没有乌加里特语，T.已经希望工作每个数据从每个理解良好的文本（包括许多数据来自理解较差的文本）到他的处理。这种尝试在精确定位是可取的，但感兴趣的学生将在这里找到T.的立场是建立在数以千计的个体点的解释。这种程序的优点是，个人的解释可以被转换为语法规则，而且规则往往取决于非常小的数量的数据，有时是单个的数据。因此，每个用户都必须仔细检查规则的基础是否充分并且得到充分的理解，是否应该接受这个规则作为有效的乌加里特语规则。

七，一个一般性的注释是关于乌加里特语的‘空洞’的词根，就是那种WH/Y/C/，如{qm}，‘他 arose’。在关于语音学和形态学的章节中，T.重构了所有动词和名词形式的三音节模式。
example, on p. 199 (§33.323.3), {qm} is vocalized as /qâma/ (the circumflex accent indicates a contracted vowel) and that form is said to derive from /qawama/; on p. 642 (§75.521b), the /YQTL/ forms are reconstructed as “/yaqûm-“ ← /yaqwum-/, “/yi‘âr-“ ← /yi‘war-/, and “/yaṣîr-“ ← /yaṣîyt-/ (in the appropriate phonology section [p. 191 (§33.312.21c)], only the first of these was cited, with the remark that no form is attested in Ugaritic with the ‘weak’ consonant retained). The reader unacquainted with the historical grammar of Semitics should be aware that Ugaritic, like Biblical Hebrew and early Aramaic, shows virtually no explicit trace of the triconsonantal pattern in the verbal system of hollow roots (e.g., there are very few Piels of hollow roots in Biblical Hebrew) and very few such traces in the nominal system. All developments of the type /*qawama/ → /qâma/ in the verbal system belong, therefore, to an earlier stage of the development of the Semitic languages and it is debatable (a) that the developments leading to the (largely hypothetical!) Ugaritic forms should be indicated in detail in a grammar of Ugaritic (i.e., all such developments could have been indicated in an abbreviated way in introductory sections), and (b) that the vowel of such forms should be indicated as a contracted vowel in the same sense as qayl going to qêl involved a contraction. Particular problems are posed by forms with /â/, for, in verbs, the contraction of /wa/ to /â/ is not characteristic of the Northwest-Semitic languages while, in substantives, these languages show both /qâl/ and /qawl/ forms from hollow roots (e.g., /ṭâb-/, ‘good’, and /ṭawr-/, ‘bull’). This later situation requires the hypothesis that certain contractions in proto-West-Semitic resulted in /â/, others in /î/, others in /â/, while yet others remained uncontracted until a later date (e.g., /ṭawr-/ and /bayt-/). The theory would also have to take account of the variable behavior in Hebrew, that /âî/ /âî/ in hollow-root /YQTL/ forms are shortenable whereas /êô/ are invariable. It must also be observed that T.’s presentation of the final syllable of hollow-root forms is not consistent: for example, on p. 646, the imperative of B(Y)N is presented as “/bin/ ← *bîn” while, on p. 651, the Š-stem jussive of T(W)B is listed as “uṭatîb,” on p. 652, the corresponding imperative is listed as “/taṣîr/”17 (sic!—the first consonant should have been ŧ). The syllable-type is historically similar in all three forms and, since the jussive form corresponds to Hebrew /yaqûm/ ← /yaqûm/ or, in the Hiphil, /yaqûm/ ← /yaqûm/ ← /yaqîm/ ← /yaqîm/ ← /yaqîm/ ← /yaqîm/,18 and to Arabic /yaqul/ ← /yaqul/, it is highly likely that the Ugaritic jussive form also had a short vowel19—at the very least a reason for any posited difference

17Cf. p. 594 (§74.622.3), where a series of Š-stem jussive forms from T(W)B are cited, all with the final syllable vocalized as “-ṭîb.” I have cited Š-stem jussive forms because T. in his presentation of the G-stem /YQTL/ forms affixes a dash to every form cited (e.g., “yaṣîr-”) thereby abstaining from indicating how the jussive form would have been distinguished from the indicative.

18Note that in Hebrew the Hiphil imperative did not undergo the paradigm leveling that occurred in the Qal: in the Hiphil the m.s. and the f.s. are distinguished by two features, the gender marking and the (proto-Hebrew) length of the stem vowel: /haqîm/ vs. /haqîm/, with the original /i/ of the stem syllable retained only in the forms that end in a vowel. In the Qal, on the other hand, the m.s. also shows a long vowel (/qîm/ and /ṣît/), doubtless as the result of following the pattern of the expanded (‘emphatic’) form (/qîma/ and /ṣîta/, respectively). No such alternative form was available in the f.pl., however, hence /qîmnâ/ ← /qîmnâ/ ← /qîmnâ/.

19As T. recognizes on p. 641 (§75.521): “Die durch Kontraktion entstandenen Langvokale werden wahrscheinlich in geschlossenen Silben sekundär gekürzt.” The problem in this particular aspect of the
between Ugaritic and Hebrew/Arabic in this form should have been presented. Which brings us back to the question of whether the stem vowel of such forms should be indicated as a contracted vowel; since, as apparently in proto-Hebrew, vowels that are the result of contraction are of two types, those that produce new phonemes (e.g., /ay/ → /ê/) and those that produce vowels apparently identical to previously existing long vowels (e.g., /iy/ → /î/ ≈ /≠/), one may expect the latter type to have evolved identically whether /î/ or /≠/, while the former may, as in Hebrew, have been more impervious to shortening. It would in any case have been useful in this essay of reconstruction to attempt to distinguish between consonantal elision resulting in a pure long vowel and contractions that produced a long vowel of secondary quality (of which the most prominent would have been /ê/ and /ò/). The former may have been expected to show a subsequent development identical to the original pure long vowels while the second constitute true phonetic innovations in the language. Equally hypothetical and equally without data from actual Ugaritic forms is the reconstruction of III-weak forms of the same root as III-y when the stem vowel is reconstructed as /a/ or /i/ (e.g., {≤ly} “/alaya/” [p. 655] or “/alîy-/*” [p. 197]), but as III-w when the stem vowel is reconstructed as /u/ (e.g., {y’1} “/ya’îû ← *ya’iûwu/” [p. 661]). Again, the latter pattern is not the one that has been retained in the other Northwest-Semitic languages. The place for such detailed reconstructions is, in any case, in an historical grammar. What T. has done, to invoke a comparison with grammars of Biblical Hebrew, is to combine Gesenius’ Hebrew Grammar and Bauer and Leander’s Historische Grammatik der hebraischen Sprache into a single unified presentation. He apparently felt himself driven to such a procedure by the dearth of data for the vocalization of Ugaritic (the undertaking is closer, therefore, to doing a grammar of the Hebrew inscriptions than to doing one of Biblical Hebrew, which is in fact usually done as a grammar of the Hebrew Bible as vocalized by the Massoretes). It is debatable whether the extensively reconstructed—and highly hypothetical—version of

---

20 The problem is well illustrated by the root B(W)’. Hebrew /yabô(‘)/ can only have come from /yabu’/, and the Ugaritic /YQLØ/ form of this root must, to the extent that the two languages had similar historical developments, have been /yabu’/ (see the more detailed argument below, remarks to p. 37 [§21.322.5a], etc., and to p. 196 [§33.322.3c], etc.). Another case of inconsistency in vocalizing these forms is debated in the first of these remarks, i.e., “tabû” on pp. 37, 443, and 620, but “tabu” on p. 643, both for {thi} in RIH 78/20:18 (KTU 1.169).

21 The identification of the root as III-w or III-y appears at times to be completely arbitrary (see below, remark to p. 198 [§33.323.1b], etc.).

22 On the necessity of distinguishing between Massoretic and Biblical Hebrew, see Pardee JNES 56 (1997) 144-7.
Ugaritic grammar provided by T. is what was needed at this stage of Ugaritic studies. It would, in any case, have been advisable to give to the present work a title that better reflects its actual form: traditionally, a work entitled simply a Grammar is primarily descriptive.

— This leads to an eighth general remark, on T.’s presentation of historically long vowels in secondarily short syllables, as in the hollow-root forms cited in the preceding paragraph. In those cases, the comparative evidence makes it very likely that the proto-West-Semitic forms had already shown a short vowel in a closed syllable. There is no treatment of the question in the phonology section, and T. vacillates not only in his treatment of these forms which appear fairly clear, but also in his presentation of many forms throughout the grammar. An example chosen at random: on p. 203 {tr“n} in (RS 3.346:20 [KTU 1.2 iii 20]) is vocalized “/tarhusû(n)nî/,” but, if, historically long vowels shorten in closed syllables, the alternative form with doubled /n/ would have been /tarhusunnî/. In other cases, he equivocates by using a dash, e.g., §73.611.2a (p. 499), on III-weak Energic-I forms, of which the first is vocalized as “/tabkî-nna/.”23 If long vowels were not tolerated in closed syllables, the penultimate syllable would either have had a short vowel or else the existence must be posited of an energetic ending /-na/ which would, of course, be identical to the indicative ending /-na/. By this use of the dash, T. refuses to come to terms with the apparent necessity to conclude that the marking of plurality by vowel length would have been lost in the energetic forms (/YQTLû + nna/ → /YQTLunna/ and /YQTLû + (a)n/ → /YQTLun/). I know of no evidence in later West Semitic for the existence of such forms, and one must conclude that these patterns were avoided.

— Ninth general remark, T. defines “n[eue] L[esung]” as any reading differing from that to be found in CAT (p. 11 [§19.5]) and the siglum does not necessarily, therefore, designate a new reading of the signs on a tablet that has never been proposed before nor necessarily a new interpretation of a passage that has never been proposed before, the two primary meanings of ‘reading’ and the only primary meaning of ‘new’ that standard usage leads one to expect. Rather, in the first usage, it means a reading of one or more signs on a tablet that differs from what is indicated in CAT, no matter who proposed it or when; and, in the second, an interpretation of a passage that differs from what T. judges to be the standard tradition, whether proposed previously or not. The notion of “Lesung” is also expanded to include restorations, i.e., cases where the reading of the extant signs is not in doubt but a new restoration of destroyed signs is proposed. I have noted below a certain number of striking cases where the reading or the interpretation so identified has previously been proposed by others even by T. himself, but have not commented on all such instances. A grammar cannot, of course, serve as a manual of epigraphy or a history of scholarship, but in a grammar as bibliographically oriented as this one, more information on the basis for the “new” reading might sometimes have been expected.

23Here the question is specifically left open as to whether the vowel in question is open. Below, p. 659 (§75.532), the form is vocalized /tabkinna/. In the case of the root BNY, on the other hand, the ambiguity is left open on p. 659, where the reconstruction is /tabnû-nna/ (on the problem of reconstructing III-weak forms of the same root with /i/ and /u/, see above, seventh of these general remarks).
Tenth general remark, building off the tenth: Sometimes T. states clearly that the precise analysis, meaning, or function of a given form cited in a given section is uncertain because the context is broken, sometimes he does not. This is unfortunate because it means for all practical purposes that, unless a given form bears one of the qualifiers "ohne Kontext" or "Kontext unklar," the user of the grammar must check each form cited under a given rubric of the grammar in order to be certain whether or not its classification is certain.

— Twelfth general remark: I know for a fact that T. in his own mind distinguishes between prose and poetry for, in an e-mail exchange pre-dating the appearance of this grammar, he referred to these two strata of the language. In this grammar, however, he seldom separates out the two strata, neither in morphology nor in syntax. It would, however, have been a useful thing to do, particularly in the section on the verbal system (pp. 682-718 [§76]) for he there attempts, as he has attempted to do with Biblical Hebrew, to present a unified description, one capable of accounting for both prose and poetry, something that is not, in my estimation, yet possible—and may never be: if the distribution of verbal forms in Ugaritic poetry includes archaic forms inserted for their 'stylistic' value, rather than according to a strict morpho-syntactic system, then there is no verbal system to be described in poetry, that is, one that can be reduced to a system of morpho-syntactic rules.

— Final general remark: this is not a bibliographical grammar. Sections of the grammar are sometimes preceded by a few general references, but specific positions adopted by T. are only relatively rarely ascribed to the scholar who originally proposed them and, as we have seen in the ninth remark, more recent interpretations are also often not ascribed to their originator. The result is that one frequently finds oneself asking why such-and-such a scholar is or is not cited for such-and-such a point. In this respect, as in several others, this work is, therefore, neither fish nor fowl. T. should either have omitted all such references or

---

else included a much fuller array of them—the latter procedure would, of course, have made of a very thick work an even thicker one.

My remarks on individual points follow. The reader should note that the purpose of these notes, which by the nature of such an exercise are usually negative, is not to belittle T.'s work, but to lay out the elements of a dialogue on points that I consider questionable or worthy of further discussion. Though other specialists might agree with T. over a given position that I put forward, I firmly believe that taking these comments into consideration, whether to accept or explicitly to reject them, would make a future re-edition of this grammar a stronger one. Also in the nature of the exercise is the fact that I cite my own work far more often than proper scholarly modesty would normally allow; this is done to provide the reader with references to the full treatment of a passage or a grammatical point that I have given elsewhere but that cannot be repeated in even so lengthy a review as this one.

— p. 1 (§11.2). Only for the people who work with texts can the last 150 years of the history of Ugarit be considered the “Blütezeit” of the city. The high esteem in which the city was held by the king of Mari in the eighteenth century B.C.25, the mention of the destruction of the palace of Ugarit by fire in a letter discovered at Tell el-Amarna (EA 151:55-57, fourteenth century), and the archaeological data from the seventy years of excavations at Ras Shamra leave little doubt that the city itself was important at many periods of its long life. The only reason that “die meisten archäologischen Funde … stammen aus dieser Periode” is that, aside from the first excavator’s tendency to dig below foundation levels26 and from a very limited number of deep soundings27, virtually all archaeological effort has been devoted to the last stratum of the site.28

— pp. 1-2 (§11.3): To the list of writing systems attested at Ugarit should be added Hieroglyphic Hittite (distinct from the so-called Cypro-Minoan script, which T. accepts as possibly representing Luwian, following Woudhuizen)29 and the Phoenician linear script (represented by one of the sparse first-millennium finds).30

— p. 2 (§12.2): RS 24.245 (KTU 1.101) certainly does not belong to the hymnic genre and RS 24.252 (KTU 1.108) may be so classified only with a rather elastic definition of hymn.31

— pp. 3-5 (§13). I find it remarkable that the linguistic classification of Ugaritic could be discussed virtually without mention of Amorite (the only exception is in the criticism on p. 4

28See M. Yon, La cité d'Ougarit sur le tell de Ras Shamra (Guides Archéologiques de l'Institut Français d'Archéologie du Proche-Orient 2; Paris: Éditions Recherche sur les Civilisations, 1997).
of the proposal to identify Ugaritic, Amorite, and Eblaite as a distinct group). Because (1) speakers of what must have been a plethora of Amorite dialects occupied most of the northern part of the Fertile Crescent from late in the third millennium until late in the second millennium (paucity of texts makes a more precise statement difficult); (2) there were clearly close cultural ties in the eighteenth century between the Amorites of the middle Euphrates and several principal cities to the west, from Aleppo to Hazor and including Ugarit (see above, note 25); (3) at least as early as the eighteenth century there was a region north of the Bekaa Valley known to the Mari Amorites as “Amurru,” for these reasons it appears necessary to include in the data for the linguistic classification of Ugaritic not only those from the following millennia (i.e., Canaanite, Aramaic, and Arabic) and from second-millennium Canaan (i.e., the so-called Amarna glosses) but also those from second-millennium Syria.33

— p. 10 (§18.6). “KTU 9.530”34 is certainly a ghost text. Reference to it first appeared in M. Dietrich, O. Loretz, Word-List of the Cuneiform Alphabetic Texts from Ugarit, Ras Ibn Hani and Other Places (KTU: Second, Enlarged Edition), p. 232,35 where a preliminary reference to the text by P. Bordreuil is mentioned.36 From the partial quotation given there and the description of the find, there can be no doubt that the tablet in question is RS 34.356 (CAT 2.76), published in 1982 by Bordreuil.37 Fortunately, the only reference below in this grammar to CAT 9.530 is, according to the index and my own observation, on p. 845 (in an


33T.’s presentation of the problem is considerably more nuanced here than it was in his 1994 article in which he made a strong case for the definition of Ugaritic as a “Northern Canaanite” dialect (“Is Ugaritic A Canaanite Language?,” pp. 343-53 in Ugarit and the Bible. Proceedings of the International Symposium on Ugarit and the Bible, Manchester, September 1992 [eds. G. J. Brooke et al.; Ugaritisch-Biblische Literatur 11, Münster: Ugarit-Verlag, 1994]). In my review of this presentation, which is not cited in the bibliography of the work presently under review, I stressed the possible Amorite connection (JAOS 117 [1997] 375-78).

34One may note that this form of reference constitutes a third level of removal from KTU (see above, note 1): there was no section 9 in KTU and the last CAT number in section 9 of CAT is 529. This reference also provides another example of the incompleteness of the indices (see above): “KTU 9.530” is mentioned in the appropriate index (p. 1049) only with reference to p. 845.

35Abhandlungen zur Literatur Alt-Syrien-Palästinas und Mesopotamiens 12; Münster: Ugarit-Verlag, 1996. This text is also cited by W. G. E. Watson in his 1999 study of the Ugaritic word thm, ‘message’ (JNSL 25/2, pp. 1-16, esp. p. 7 with note 54).

36AAAS 29-30 (1979-80) 11.

“Anm[erkung]” to §91.314.1); see my remark ad loc. on the confusion created by drawing divergent data from what is in fact the same text.

— p. 10 (§19.2). T. observes here that texts from Ras Shamra that are inscribed in the Sumero-Akkadian syllabic script are cited by RS-number, but he does not indicate how to get from the number to the text itself. For that the reader must go to either P. Bordreuil and D. Pardee, La trouvaille épigraphique de l'Ougarit. 1 Concordance,38 or to van Soldt, Studies in the Akkadian of Ugarit.39 In cases where a publication is listed in neither of these sources for a given text (e.g., RS 25.428), T. should have told the reader where to go to check the quoted data.

— p. 10 (§19.2). T. further observes that CAT does not give a sequential version of RS 17.434+ but that he will cite data from that text with the fragments40 arranged sequentially, hence with a sequential line numbering. The convention chosen by T. for citing this text is “2.36+,” i.e., with reference to the lowest KTU/CAT number representing a fragment of this text. Since he himself does not provide the sequential text upon which the line numbers he cites are based, it might have been useful to the reader to indicate the study where such an arrangement was proposed, which arrangement T. in fact follows.41

— p. 13 (§2). The situation with regard to the two infrequently attested alphabet forms is not made clear here: it is said to be unclear whether these two systems were used to write Ugaritic, as was the case with the standard ‘long’ alphabet. ‘Unclear’ is indeed the best term to describe the use of the first of these systems; the situation with regard to the second is presently clear, but future finds may change that situation. (1) The ‘short’ alphabet was used to write texts; no abecedary is yet attested for this alphabet; it is uncertain whether the texts constitute a ‘Phoenicianizing’ representation of Ugaritic or are in the/a ‘Phoenician’ language; because of the latter uncertainty and because of the brevity of the texts, it is uncertain whether in every case the alphabet is the same and consists, like the Phoenician alphabet, of twenty-two signs—though there is, to my knowledge, no counter evidence to the hypothesis of twenty-two signs. (2) The twenty-seven-sign abecedary discovered in 1988 (RS 88.2215) and published preliminarily in 199542 is to date attested at Ugarit only in this one

40What was probably originally a single tablet was discovered as a series of fragments not all of which join physically one to another: RS 17.434, RS 17.434[A], RS 17.435, RS 17.436, RS 17.437, RS 17.438, RS 17.438[A], RS 17.438[B]. An error in Bordreuil and Pardee, La trouvaille (1989) 148, has led the authors of CAT to create two ghost numbers (CAT 9.176 and 9.177). We indicated that the fragments RS 17.438[A] and RS 17.438[B] were unpublished when in fact they had been included in the editor's transcription of RS 17.438:12-13 (Caquot, Ugaritica VII [1978] 133). Since I first collated the tablet in 1980 and since our preparatory work for La trouvaille, the two tiny fragments have in fact been glued back on to the larger fragment.
text (another similar abecedary was discovered in 1933 at Beth Shemesh in Palestine); it represents the alphabet in the order known from South Semitic ({h, l, ḥ, m ...}); though some of the sign forms show similarities with their equivalents in the 'short' alphabet, there is no text of which I am aware inscribed with the system of forms attested by this abecedary. Below, p. 14 (§21.113), T. does not take a position on the origin of the two alphabetic orders, that of the 'long' alphabet for which several abecedaries are attested ({ā, b, g ...}) and that of the twenty-seven-sign system ({h, l, ḥ, m ...}), in contrast to his preference in 1996 for seeing both orders as of Northwest-Semitic origin. 

— p. 15 (§21.21). The absence of texts betraying experimental stages of the standard alphabetic cuneiform system (the so-called 'long' alphabet) appears to me an insufficient basis on which to qualify as 'likely' the hypothesis that this alphabet “in seinen Grundzügen von außen übernommen wurde.” Why would the experimental tablets have been saved? As it is, probably 99% of the extant Ugaritic tablets date from the last sixty years of the city and well over 90% from its last decade (this is in contrast with the texts in Akkadian, a higher percentage of which record matters that made them worth keeping). For a tablet representing the origins of the system to be found today, it would have to have happened to be in a building that burned, to have been in the part of the building where the fire was hot enough to bake it, and that accidentally baked tablet would have to have been included with rubble used as fill in the construction of a new building, rather than being accidentally trodden under foot and crushed. Though such a series of events could, of course, occur, an argument from silence in such a case is hardly convincing. 

— p. 15 (§21.221). The number of thirty signs in the standard alphabet is not “mit der Anzahl der Tage des Mondmonats identisch”: roughly half of lunar months are twenty-nine days long (or, put another way, the lunar month is roughly twenty-nine and a half days long). There is no evidence of which I am aware indicating that the addition of three signs ({i, û} and {ŝ}) to the basic consonantal inventory of twenty-seven was motivated by any consideration other than usage in writing texts. 

— pp. 15-16 (§21.222). The long discussion of the hypothesis put forward by Dietrich and Loretz according to which there existed in Ugaritic a separate sign that consisted of two

---

43 I am loath to accept that the Beth Shemesh abecedary constitutes a fourth distinct alphabetic system (as T. posits below, p. 15 [§21.12]). Though the Palestinian version is written from right to left while the one from Ugarit is from left to right, and though there is at least one important difference between the two ({d} is in a different position), it is hard to escape the conclusion that the two texts represent at most variants of a single system (see the edition of RS 88.2215 by P. Bordreuil and D. Pardee, “Seconde Partie: Une bibliothèque au sud de la ville. Textes de la ‘Maison d’Ourtenou’ trouvés en 1986, 1988 et 1992. Textes alphabétiques en ougaritique” in Études ougaritiques. I. Travaux 1985-1995 (ed. D. Arnaud, M. Yon; Ras Shamra-Ougarit XIV; Paris: Éditions Recherche sur les Civilisations, 2001) 341-92, 411-14, esp. pp. 341-48, text 32.


45 What is meant by “Grundzügen” is not specified. Below (p. 28, §21.242) one reads: “Die Ugariter setzten das lineare Alphabet in ein Keilschriftalphabet um ...” without mention of borrowing the basic features from elsewhere. There are as yet no data indicating that either the short alphabet or the twenty-seven-sign {h, ḥ, m} alphabet, of which a significant number of forms are different from the corresponding signs in the standard alphabet, preceded the standard alphabet.
overlapping oblique wedges within a circle allows for more examples of the phenomenon than do in fact exist. Of texts that I have collated, RS 1.003 contains one example (a note in CAT refers to this as an encircled ‘®’, but the sign within the circle in line 22 is certainly {®}), RS 24.244 (CAT 1.100)46 shows no examples (CAT does not register the disputed sign in this text, but T. mentions possible examples in lines 4, and 20—these are, however, only somewhat carelessly formed {®}s), RS 24.250+ contains no examples (a note in CAT claims that both {®}s in line 32 are encircled), RS 24.252 (CAT 1.108) one example, while in RS 24.271 (CAT 1.123) one finds two examples (the first {®} in line 5 and the only {®} in line 25) rather than the five examples claimed in CAT. T. refers to some of these cases as lightly encircled, but the distinction between a shallow partial arc produced accidentally as the stylus was rotated to inscribe {®} and a purposeful roughly inscribed complete circle47 is important. Since the case of RS 24.271:5 almost certainly represents the indication of an error in the text (the scribe wrote {®} for the first {®} of the divine name that appears elsewhere as {®w km®}), that of RS 24.252:9 may well have the same origin (a satisfactory explanation of {®gl®l mßt} has not been found), and the other certain examples occur in unexplained words ({{hmt} in RS 1.003 and an otherwise unattested divine name in RS 24.271:25), the hypothesis that the rough circle was an indication of a faulty sign appears valid for these cases.48 The fact that certain examples exist only for {®} is intriguing, as is the fact that in all cases the {®} in question is the so-called “three-lobed” form (the second from the left in the table of sign forms on p. 19);49 but the hypothesis of a single sign having this form does not explain the texts mentioned. The case of the Hurrian text RS 5.182 (CAT 1.66) may be different, for (1) that text is Hurrian, not Ugaritic, (2) the encircled sign is {≤}, not {®},50 and (3) {®} appears elsewhere in the text while {≤} does not (as is expected in a Hurrian text). Something does, therefore, appear to be going on here that is different from the Ugaritic examples cited.

— pp. 16-19 (§21.231). The only general criticism that I would voice with regard to the table of sign forms (for the preparation of which T. thanks J.-P. Vita) is that the listing of a few forms of each sign might lead the reader unacquainted with Ugaritic epigraphy to believe that the sets are exhaustive. Such as not the case, though the number of variant forms will vary from sign to sign and according to the degree of variation that the preparer of

46CAT is cited here because the hypothesis was formed after the publication of KTU.


48T. comes down explicitly for this solution in the cases of RS 24.252:9 (KTU 1.108) and RS 24.271:5 (KTU 1.123) on p. 16 (§21.222) and p. 66 (§21.364); on p. 98 (§32.134), he cites the latter text as an example of dissimilation of phonetic emphasis.

49The representation of the encircled {®}, fourth column of this table, is incorrect in that it shows the encircled element as a simple triangle; the attested form in the texts discussed above is that of the second column with a rough circle around it.

50A. Herdner, Corpus des tablettes en cunéiformes alphabétiques découvertes à Ras Shamra-Ugarit de 1929 à 1939 (Mission de Ras Shamra 10; Bibliothèque Archéologique et Historique 79; Paris: Imprimerie Nationale; Geuthners, 1963) 267.
Four remarks are necessary, however, on details of the table: (1) Neither in this table of sign forms (pp. 18, 19) nor below in T.'s discussion of variant forms (pp. 22-26 §21.235) is one of the most distinctive forms of {h}, {t}, and {q} mentioned. I refer to the form in which the upper tip of the right-hand wedge is extended exaggeratedly upwards so that this tip, in the cases of {h} and {t}, is significantly higher than the upper edge of the (upper) central wedge. This form is best attested in administrative documents, including letters. (2) The third form of {l} that is listed, viz., one consisting of three vertical wedges followed by a small horizontal wedge placed at the upper right corner of the sign (p. 18), occurs only, to my knowledge, in a single text, RS 17.117, where the four wedges are to be read as a “ligatured” form of two signs, {l} and {t}. This interpretation of the wedges in question is not recognized in CAT but it constitutes what is in my opinion the only plausible epigraphic explanation of this configuration of wedges as well as the only set of readings that makes sense of the text. (3) The representation of the lateral wedges of {s} (sign #30 in the abecedaries) as sometimes four-sided (p. 19) is incorrect. These wedges, like virtually all of the components of the Ugaritic signs, are triangular in form (the principal exception is the ‘three-lobed’ {t}); it is only the placement of these small wedges one above the other, with the bottom edge of the upper wedge not always obliterating the upper tip of the wedge below, that gives the optical illusion of four sided wedges. The phenomenon can occur elsewhere, for example in {h} and {i}, and a false impression is created by indicating the optical illusion only in this case. As Ellison shows, the degree of deformation in signs consisting of multiple horizontal wedges (or oblique wedges in the case of most examples of {s}) depended on how soft the clay was when inscribed, with the most deformation occurring when the clay was too soft. (4) The representation of the encircled {≤} as a valid form of {®} may be incorrect (see above, remark to pp. 15-16 §21.222).

With regard to the convention of representing Ugaritic signs as consisting of a triangular head affixed to a line and its abandonment by P. Bordreuil and most subsequent epigraphers of Ugaritic in favor of representing each stroke as a true triangle, some remarks are in order. The Ugaritic scribes habitually produced signs consisting of groups of single triangles, i.e., they created each wedge by a simple impression of the stylus into the clay with a minimum of rotation to produce varying angles. To state it most simply: they did not impress a small triangle as the ‘head’ of a wedge, then draw a line to represent its tail, as the conventional representation would lead one to believe was the case. This is just as true of syllabic signs as of alphabetic ones; however, the fact that syllabic signs often consist of a greater number of wedges than alphabetic ones means that some of the wedges in a syllabic sign must be smaller than others—these smaller wedges often have a very narrow head and thus come closer to being a simple line in the clay than


do the deeply impressed wedges. That the ancients perceived of these sign elements as true triangles is indicated by the fact that, in at least some cases, syllabic signs are carved in stone as true triangles rather than with a large head and a fine tail. On the impressions of the seal of ‘Ammiyidtamru (a.k.a. ‘Ammittamru),\(^54\) the wedges are rendered as triangles with no indication of the inner lines of a wedge impressed in clay. (I am not the one to pronounce on cuneiform traditions in the ancient Near East; but someone better qualified than I could do an interesting study of the variable representations of cuneiform signs in different media and at different periods.) On the other hand, it is an option, when passing from the three-dimensional impression in the clay to a two-dimensional representation, to produce, instead of a tracing of the outline of the triangle in the clay (i.e., what one sees at the surface), the lines formed by the edges of the stylus as they penetrate into the clay. This is what the traditional representation does, but cannot limit itself to these lines: in the case of a long wedge, a top stroke must be added to close the head; this stroke does not represent a stylus edge but where the head of the stylus happens to meet the clay. The inner lines of the small oblique wedge are even more difficult to represent and the results of copying this wedge according to the Assyriological tradition vary from a simple triangle to a triangle set at an angle with a short tail. This means of representation must, therefore, be judged more open to subjective treatment on the part of the epigrapher than the first. A third option is also possible: to use perspective to represent in two dimensions the three-dimensional form of the sign, that is, both the outer edges of the sign and the internal lines produced by the stylus edges.\(^55\) This is certainly the truest representation but it requires a certain level of draftsmanship and an investment of time that few are willing to make. What is important, it appears to me, is that the proponents of the first two methods realize that each is a conventional representation in two dimensions of a three-dimensional form. I prefer the second because it represents a single category of lines in the clay, but what proponents of the Assyriological convention produce will be just as readable to anyone educated in both traditions.\(^56\) By attempting three-dimensionality, the third method should be the most representational and hence in some sense the best. Any of the three, if carefully rendered, will provide the user of an edition with a readable record of what the editor has seen on the tablet.

— p. 21 (§21.233). I disagree with T.’s assertion that the lower edge of ‘{≤} is usually strongly concave (“In der Regel … stark (konkav) gekrümmt”). I would call the ‘{≤} of RS 24,252 (KTU 1.108) strongly concave,\(^57\) but in most cases the visual roundness of this lower


\(^{55}\) J. du Four did this in his copy of RS 22,003 provided for P. Bordreuil’s study of that text (Syria 58 [1981] 301-10) and my student R. Hawley is doing the same in his study of the Ugaritic epistolary texts.

\(^{56}\) I agree, therefore, with T. that “Tatsächlich besitzen die Keile des ug. LA [Langalphabet] jedoch eine längliche Dreiecksform.” But that is the outline of the wedge on the tablet surface; others may still prefer to attempt to represent the inner lines of the wedge.

\(^{57}\) See copy in Pardee, Les textes para-mythologiques (1988) 77 and the textual remark to line 9 on p. 79.
edge is not nearly so strongly marked. This is because, to produce such a form, the scribe had to rotate the stylus with a lifting motion from left to right while impressing the sign; relatively rarely, in my experience, is such a strongly marked rotation visible. Indeed, I would have to agree with Pitard\textsuperscript{58} (whom T. is criticizing in the quoted remark) that the lower edge of ‘≤’ usually shows little if any more roundness than the two upper edges, though it is true that, if one edge is to be rounded, it will be the lower edge. Such questions are answered with more detail, and the answers are fully illustrated, in Ellison’s study of Ugaritic palaeography.\textsuperscript{59}

What I missed most in paragraph 21.233 was an explicit statement to the effect that only one of the various forms of ‘≤’ will usually occur on a given tablet:\textsuperscript{60} this sign is, therefore, one of the most useful for purposes of distinguishing hands, and its form(s) must be observed when deciding whether fragments may or may not be assigned to a single tablet.\textsuperscript{61} If one wishes to posit that fragments with different forms of ‘≤’ belong to a single tablet, one must further posit that the hypothetical tablet was inscribed by two scribes, a rare phenomenon but one that does occur (RS 1.003 [KTU 1.41] is one of the clearest cases with which I have dealt\textsuperscript{62}).

— pp. 22-26 (§21.235). There is no discussion of one of the few sign-form variants for which a legitimate claim may be made that it is chronologically significant: I refer to the two-wedged {g} and what are apparent archaic representations thereof with a single wedge. On the impressions of the seal of ‘Ammi≠yi≠tamru (see above, remark to p. 20), it is clear that the {g} was represented with two wedges, a vertical wedge and a horizontal with the head of the wedge overlapping the right upper point of the vertical. (This is phrased with regard to the seal itself, which was carved in the positive with the result that the impression is seen in the negative.) Because the seal was that of ‘Ammi≠tamru II, the form may be dated to some time between ca. 1275 and ca. 1230 (depending on the dates assigned to the reign of ‘Ammi≠tamru). By chance, I was copying RS 16.382 (KTU 3.5) at the same time as the seal of ‘Ammi≠tamru and noticed that the {g} in that text has a vertical left edge, a long upper edge and a strongly concave right edge. Since this text is dated explicitly to ‘Ammi≠tamru, it appears certain that the scribe was attempting to produce a one-wedged form of the {g} while retaining the extension of the right upper edge characteristic of the two-wedged form. Checking a new text dated to the time of the same king, RS 94.2168, it was observed that the {g} in that text has basically the same form but without the extreme concavity of the right edge of the {g} in RS 16.382. It appears difficult to avoid the conclusion that the {g} of the seal was formed in imitation of the linear {g} consisting of two

\textsuperscript{58} JNES 51 (1992) 261-79.

\textsuperscript{59}Paleographic Study (2002) 252-77, figs. 964-1050.

\textsuperscript{60}This is implied in the recapitulation of W. Pitard’s views, but the point should be made loud and clear since some Ugaritologists appear to be unaware of the fact and/or its implications.


\textsuperscript{62}See ch. 3 in my Les textes rituels (2000).
strokes known from the first-millennium linear alphabet; that the {g}s in RS 16.382 and RS 94.2168 are adaptations of that form to one that can be produced without removing the stylus from the clay; and that the {g} produced as a triangle centered on the central vertical axis rather than with a vertical left outer edge represents a third stage of evolution. Truly two-wedged forms are attested on RS 15.111 (KTU 3.3), which I copied after RS 16.382, and on several tablets from Ras Ibn Hani. The data, at least those of which I am aware, are yet too sparse to allow much to be said about the implications of these forms for the history of the cuneiform alphabet; but the least that can be said is that there was an archaic form of {g} in use in the time of ‘Ammiṭtamru II.

— p. 22 (§21.235). The {û} in RS 1.001:10 (KTU 1.39) has three, not two, vertical wedges.

— p. 23 (§21.235). What is cited as a four-wedged {h} in RS 1.012:2 (KTU 4.14) is in fact {i} (the right half of the lower vertical wedge is preserved); moreover, this {i} actually contains five horizontal wedges.

— p. 23 (§21.235). Only the second {d} in RS 1.012:8 (KTU 4.14) shows a fourth lower wedge, not both examples of this sign as claimed by T.

— p. 24 (§21.235). Both {n}s in RS 1.012:2 (KTU 4.14) have five wedges (T. claims only the first), and there are two further examples of five-wedged {n}s in lines 8 and 10 of this text that are not mentioned by T.

— p. 26 (§21.236). Because the seal of ‘Ammiṭtamru discussed above (note to pp. 22-26) shows no signs with extra wedges ( {n, r, l, i, û} are extant in that inscription, signs that are elsewhere all attested with extra wedges), T.’s hypothesis according to which the extra-wedged forms would be typologically the older must be rejected on present evidence. It may be observed that the hypothesis goes against the principle of economy that may have been observed at the time of the invention of the cuneiform alphabet: only two wedges are sufficient to distinguish {r} from {k}, not four, and so forth, mutatis mutandis, in the cases of {t, â, n}, {g, s, l}, {p, h}, {g, z, s, ſ}, {ď, ř}, and {b, d}. In that context, it may further be

63The {g} in RS 16.191*:11 (KTU 3.4) (my thanks to R. C. Hawley for demonstrating this example to me) and in RS 88.2215 (RSo XIV 32) (see above and note 43 for references) are indubitable examples in clay of the two-wedged {g}, plausibly explainable as an imitation of the linear form (Bordreuil and Pardee, CRAI 1995, p. 857, and RSo XIV, p. 343). The problem here is the origin of the alphabetic tradition that it represents and the date of the inscription of the abecedary; there is, in any case, nothing about the tablet itself or its find spot that requires an early dating. See further, R. C. Hawley and D. Pardee, “Le texte juridique RS 16.382: nouvelle étude épigraphique,” forthcoming in Semitica 52.

64See my preliminary statement on {g} in the texts from Ras Ibn Hani in “RIH 77/27, RIH 77/12, RIH 78/26 et le principe de l’écriture cunéiforme alphabétique” forthcoming in Syria 79.


67Ibid.

68Ibid. The five-wedged examples in lines 8 and 10 were already pointed out by Herdner, CTA (1963) 231.

69Strictly speaking, only one wedge would have been necessary, but the principle of symmetry (and/or of representation of the underlying linear form) appears to have dictated the standard form (compare the case of {b, d}). On these matters, see my article cited above, note 64.
observed that the liberty to produce a sign with extra wedges only exists at the outer limit of a combination of wedges: producing a two-wedged \{t\} would result in \{â\}, producing a three-wedged \{â\} would result in \{n\},\(^{70}\) but \{n\} can be produced with as many wedges as the scribe wishes because there is not a sign that consists normally of four, five, or more horizontal wedges in a row. It thus appears more plausible to posit that the original cuneiform alphabet consisted of minimal numbers of distinguishing wedges and that, for reasons difficult to define precisely today, scribes allowed themselves to add wedges to signs in cases where no ambiguity would result. As for the other texts from the time of ‘Ammi®tamru discussed above, RS 16.382 shows a majority of \{n\}s and \{y\}s with four wedges per row, but no other signs with extra wedges; RS 15.111 shows very similar characteristics and may indeed have been written by the same scribe; in RS 94.2168, seven of nineteen \{n\}s have four wedges, but no other signs, including \{y\}, consist of extra wedges. It is thus certain that the option of producing signs with extra wedges already existed in the time of ‘Ammi®tamru II; on the basis of present evidence, one can hypothesize that the spread of the practice to the other available signs (in particular \{h\}, \{r\}, \{h\}, and \{i\}) also occurred in the time of ‘Ammi®tamru II but in the hands of other scribes.

— p. 28 (§21.243). An hypothesis of Dietrich and Loretz presented here, according to which the partial abecedaries inscribed on the exercise tablet RS 16.265 (KTU 5.9) would have been broken up into eight “Ausspracheblöcken,” is not confirmed by examination of the original: the tablet in question had been used more than once and what Dietrich and Loretz took for meaningful dividers are in all cases remains of a previous inscription or other imperfections in the surface of the tablet.

— p. 28 (§21.244). T. provides excellent reasons for not using the order of the Latin alphabet when alphabetizing Ugaritic,\(^{71}\) then says that he is adopting the traditional system of using a modified Hebrew order\(^{72}\) (after presenting the two, he says “… wird in der vorliegenden Grammatik das erstere Ordnungssystem verwendet”; cf. also p. 32),\(^{73}\) but uses the Latin order in the indices (see p. 950). The use of two ordering systems, or of three if one considers the lack of a system to be a system, must be qualified as a major source of confusion in a work as dependent on its indices for use as this one is. Having recognized the existence of that confusion, however, it must be observed that T. provides no good reason for not using the Ugaritic order as the single alphabetic ordering system. It is close enough

\(^{70}\)For examples of scribal errors of this category, see pp. 57-58 (§21.352.2). Errors of the opposite type, i.e., too few wedges in a given sign, are listed in the preceding paragraph (§21.352.1).


\(^{72}\)The non-Hebrew consonants are placed after the consonant to which they are most closely related phonetically, e.g., \{s\} after \{s\}, and/or by the principle of identical base form in Arabic (\{t\} after \{t\})—with the exception of \{š\} which, in imitation of Hebrew, is moved from its place between \{k\} and \{l\} in the Ugaritic order to just after \{r\}.

\(^{73}\) The Hebrew-influenced ordering is, however, not always used in the body of the work, as in §21.335 (pp. 44-45), a listing of words containing \{š\} that follows no particular alphabetic order, or in §33.312.32 (pp. 192-93), where the order is \{â\}, \{g\}, \{d\}, \{â\}, \{z\}, \{š\}, \{š\}, \{p\}, \{t\}, \{p\}, \{r\}, \{l\}, \{́\}.

*Archiv für Orientforschung* 50 (2003/2004) online version
to the Hebrew/Aramaic order that West Semitists can have no difficulty adapting to it. But the basic reason why I believe that it should be used is that it was the order used by the Ugaritians themselves when writing their abecedaries. Why should any other order be adopted? We have all managed to deal with arbitrary orders such as those devised by G. R. Driver or F. Gröndahl; let the rest of the world adapt to the order used by the Ugaritians themselves! Probably the principal practical reason for not using the Ugaritic order—though T. only alludes to it without presenting it as a problem—is the difficulty of dealing with the three extra signs added at the end of the Ugaritic alphabet: does one alphabetize āl, for example, under the first letter of the alphabet and īl under the twenty-eighth? Not necessarily: by the principle of listing words first by roots rather than by graphemes, the problem can largely be avoided. For example, the Ugaritic order is maintained by listing āl, īl, and īl in that order, i.e., the Ugaritic order, under the root 'L; except in rare proper names, [s] is attested only as a variant spelling of words with etymological [s] and the former may be subsumed under the latter, e.g., KSM: ksm/kşm or SSW: ssō, șsw, etc. This method requires treating non-Semitic proper names as roots, but that appears to be a small price to pay for using the Ugaritic order adapted to the modern concept of root.

— p. 32 (§21.274). T. could have struck another blow for authenticity by transcribing the {'-signs with an {'-e.g., {ā, ĕ, ū}. He chooses rather to use {a, i, u} “im Anschluß an die Textausgabe KTU,” which is, of course, no reason at all. I admit that there is an element of conventionality here and that the use of a single symbol rather than a digraph corresponds to the Ugaritic use of a single symbol. I have, however, met too many people who had difficulty understanding that the transliteration with a vowel really means a consonant plus a vowel to be comfortable with that convention.

— p. 32 (§21.31). None of the examples cited here of double writing of a consonant that appears on both sides of a “Morphemgrenze” is convincing: e.g., what T. normalizes as “/yargub-ba‘IV/” was in all likelihood /yargubu-ba‘lu/. One will note that in all the cases discussed here the morpheme boundary is in fact also a lexical boundary, i.e., all cases represent proper nouns composed of two elements. (The problem of hollow-root forms such as {mtt} is discussed below under morphology, §75.521c [p. 642] and is judged here already to be an unlikely case of double writing of a geminated consonant at a true morpheme boundary, i.e., {mtt} does not represent, for example, /matta/ ↔ /mît+ta/ but /mît+a+ta/.) Historically, there would most often been a vowel at a lexical boundary (i.e., the first word would have ended with a case or mood vowel) and such boundaries are usually indicated graphically by the word-divider. So one would expect by the force of this fact and the resultant scribal tradition that even a phrase such as /yargub ba‘lu/, ‘may Ba‘lu be fearsome’

———.

74Canaanite Myths and Legends (Old Testament Studies, No. 3; Edinburgh: Clark, 1956) 133 (where the order devised is defended by this truly remarkable statement: “The letters of the alphabet [in the glossary] follow approximately the phonetic order of the sounds, not the arbitrary order adopted by the Ugaritic scribes”).

75Die Personennamen der Texte aus Ugarit (Studia Pohl 1; Rome: Pontifical Biblical Institute, 1967) 360 (also an order based on phonetic principles, but different from Driver’s).

would, in spite of the fact that the two tokens of /b/ are phonetically contiguous, have been
written as two words, with two {b} signs, usually with a word-divider between them.\footnote{One does actually find \{yrgb\ . b\’l\} in RS 2.[009]⁴ vi 57 (\textit{KTU} 1.6), but the meaning of the words in context and hence their vocalization are uncertain. On the two personal names \{"ādn\’m\} and \{"ādn\’nm\}, see my article cited in the previous note: the first elements of these two names may have been different (\{\‘ad\’n\}/ and \{\‘ad\’n\’\}), rather than identical but for the presence of a vowel after \{\‘ad\’n\’\} in \{\‘ad\’n\’m\}, as T. hypothesizes.}

But in a list of divine names, as all admit the situation of \{yr\gbb\’l\} in RS 24.246 (\textit{KTU} 1.102) to be, in which the elements of compound names are never separated by a word-divider, one expects the writing with two \{b\}s to be an indication that there was a vowel between them.

— pp. 33-35 (§21.322.1). By his repeated use of \{mād\} as a form illustrating the problems of syllable-final \{\‘\}/, T. leads his reader to believe that he accepts the example as valid; only on p. 36 (§21.322.4b) does he propound his own view that \{mīd\} is a noun (\{m\u’d-\}),\footnote{Below, p. 251 (§51.41a), T. indicates a \{qutl\}/ base for a substantive meaning “viel,” p. 255 (§51.41e) a \{qutl\}/ base for another substantive meaning “Menge,” while on p. 748 (§81.3f) he avers that a \{qutl\}/ base is as plausible as the \{qut\}/ base proposed on p. 36. Given the Akkadian forms \{mādī\} and \{mādumma\} that he cites in §81.3f, the \{qutl\}/ base appears more likely. The hypothesis of a \{qutl\}/ base in Ugaritic seems to be founded primarily on the writing \{mīd\}/ attested several times in RS 2.[022]⁺, col. iii (\textit{KTU} 1.5), though that passage is broken and the precise meaning of the form cannot be judged certain. The Hebrew form \{m\u’d\}/ is cited on p. 255 as also coming from a \{qutl\}/ base, but this also is uncertain, for \{m\a’d\}/ would have been represented in Ugaritic script as \{m\i\d\}/, just as \{r\a’s\}/ is written \{r\i’\s\}/ = Hebrew \{rō\(’\)\}s, or as \{\s\a’n\}/ is written \{\s\i\n\}/ = Hebrew \{šō\(’\)\}n; the Hebrew form \{m\u’d\}/ could be a similar form, with re-syllabified \{\‘\}/, i.e., \{m\a’d\}/ → \{m\a’\}/ → \{m\a’\}/ → \{m\a’\}/ → \{m\a’\}/ (see below, remark to p. 169 [§33.181.2], etc., on \{\s\i\r\}).} which is a more plausible explanation (though \{mīd\} could as well be from a \{qutl\}/ base as from \{qutl\}/).

— pp. 33-35 (§21.322.1), p. 452 (§73.244.2), pp. 611-13 (§75.212.12). T.’s own theory to explain writings of the type \{y\u’h\}/—which, to his credit, he does not permit to overshadow other theories,— viz., that \{\‘\}/ sporadically became \{\‘\}/, with \{\u\}/ used as a \textit{mater lectionis} for this vowel (cf. also below, p. 180 [§33.231.3]; on pp. 452, 612 the theory is preferred), has never struck me as very plausible, for two reasons: (1) there are virtually no certain cases of the use of \textit{matres lectionis} in Ugaritic—it is asking too much of this very dubious orthographic category to explain another whole category of forms; (2) when \{\‘\}/ truly quiesces, it is not (always) represented in the writing,\footnote{It must be stressed that there is very little explicit evidence in the Ugaritic writing system for the quiescence of \{\‘\}/. §32.322.1 (p. 134) describes the phenomenon as a form of contraction but only refers the reader back to §21.322.2 without providing any examples. The general regularity with which the three \{\‘\}/-signs are used and the possibility of explaining most aberrant writings as reflecting secondary opening of syllables originally closed by \{\‘\}/ (see continuation of remark above) make it implausible, in my estimation, that quiescence of \{\‘\}/ had by the end of the thirteenth century become more than a sporadic feature of the language.} as in \{m\s\m\}/ (‘seal’), attested along
The last form may be interpreted as showing that /ma’šamānu/ was realized in three different ways: [ma’šamānu] (= {mîšmn}), [maš(a)mānu] (= {mâšmn}), and [ma’aš(a)mānu]—three different phonetic realizations (or five, counting the forms with loss of the historical stem vowel) that are very similar but that the scribes recorded. Thus there may be no need to develop a separate theory to account for the [ũ] in {yuḥḍ}. The alternative theory according to which the secondary vowel after /’/ shows harmony with the following vowel accounts for the forms with [i] as well as for those with [ũ]: [yuḥḍ] = [ya’ḥud-], [yuḥḍ] = [ya’uḥud-]. The last form may in turn have become [yu’uḥud-], for this would account for the corresponding forms in Hebrew: /ya’uḥuz-/ → /yu’uḥuz-/ → /yu’uḥuz-/ → /yō(’)ḥōz-/ → /yō(’)ḥēz/ (by dissimilation). I would hold that most of the forms discussed in §21.322.4d (pp. 36-37) may be amenable to this explanation. ’BD is probably not an exception, though here Hebrew does not provide the imperative form to show what the proto-Hebrew stem vowel was, as in the cases of ’MR and ’KL; Akkadian abātu, however, has ibut, while in Arabic all three possibilities are attested (/yaqtu/, /yaqtal/, and /yaqtil/). In any case, it is uncertain whether this verb is transitive (‘to destroy’) or intransitive (‘to fall into ruin, to perish’) in Ugaritic and in the one case of [ũ] with this root, viz. tūbd in RS 18.038:21 (KTU 2.39), the form may be either G-passive (perhaps [tu’badu] ← /tu’badu/) or simply a /yaqtul/, as in Akkadian abātu or in Arabic ([ta’budu] ← /ta’budu/). ’HB, ‘to love’, once written [yūhb] in the 3 m.s. YQTL in a context where the analysis as a passive appears unlikely (RS 2.[022]* v 18 [KTU 1.5]), does appear to provide a valid exception because /yiqtal-/ is expected for a II-h root. Moreover, Hebrew shows two cases of III-weak roots with /ō/ in the preformative syllable, ’BH, ‘to be willing’, and ’PH, ‘to bake’, and explaining all forms of this category by vowel harmony entails the further hypothesis that the /yaqtul-/ form was maintained relatively late in the cases of these two roots (on p. 658 [§75.532], T. reconstructs Ugaritic

---

81 See p. 158 (§33.141.41); on p. 274 (§51.48a), this noun is classified as “{mVqtVl-ān}” from the root WSM, with secondary /’/.

82 T. represents such a form as having a very brief secondary vowel, i.e., as “ma’aš(a)mân-” (p. 36). Judging from the Massoretic vocalization system, at least, it is unlikely that such an ultra-short vowel could exist in a syllable closed by the disappearance of the following vowel. Thus, in T.’s hypothesis, one should retain either /ma’ašamān-/ or /ma’ašmān-/.

83 Below, p. 50 (§21.341.11), T. cites examples such as these to prove that [’] could be used as a mater lectionis. The multiple spellings of the word meaning ‘seal’ (cf. also {mîhd}, {mîhd}, and {mâhd}, all meaning ‘citizen of mîhd) seem rather to show that syllable-final ‘alif was sometimes heard and registered by the scribes, sometimes not. Comparisons with the Massoretic vocalization of the Hebrew Bible, where /’/ may quiesce, or behave like a guttural, or behave like a strong consonant, i.e., take silent shewa, are obvious.

84 This was Sivan’s solution to the problem (UF 28 [1996] 558).

85 One wonders to what “sprachvergleichenden Gründen” T. is referring when he discounts on this basis the explanation by vowel harmony (p. 612). The grounds would appear to be based more on theory than on attested forms. About Hebrew ʾemôr and ʾekól in any case, there can be no doubt and one may entertain the possibility that Ugaritic also had /yaqtu/- in the case of ’BD.
‘PY as /yaqtil-/ but does not give his reasons). The explanation of all /YųTL/ forms in both Ugaritic and Hebrew by vowel harmony is not, therefore, without problems.

— p. 34 (§21.322.1.6), p. 57 (§21.352.1), p. 215 (§41.221.11a), p. 549 (§74.412.23), p. 748 (§81.3f), p. 762 (§82.22), p. 872 (§93.422.1). The correction of {måb} to {måd} in RS 15.008:11 (KTU 2.16) may not be considered among the relatively certain scribal errors (see remark below to pp. 56-65 [§21.35]) when måd would function as an adverb in the text in question and T. himself parses that form as an adjective (first reference here). Below, in the section devoted to mid (p. 748 [§81.3f]), T. again asserts that {måb} in this text should be emended to {måd}, but does not explain how the adjective would function here as an adverb. The emendation to {måd} becomes all the less convincing in RS 15.008:11 when one observes that {mid} is attested in its regular adverbial function in line 10 of the same text.

— p. 37 (§21.322.4d), p. 536 (§74.333), p. 615 (§75.215). In the first paragraph cited, {tåsp} in RIH 77/18:3’ (KTU 1.175) is analyzed as a D-stem, in the other two as N-stem. Though the latter analysis is described as “sehr unsicher” on p. 536, the analysis as a D-stem is not repeated in the appropriate place in the treatment of the verb.

— pp. 37-38 (§21.322.5), pp. 50-55 (§21.34-21.342.2). I would hold that Ugaritic verbal morpho-syntax is too uncertainly defined to require acceptance of any significant number of the cases of verb-final {≥}-signs (treated in the first section cited) or of {≥}-signs in various other slots (treated in the second section) that T. interprets here as matres lectionis. It is telling that of the examples of putative uses of matres lectionis many come from difficult passages or from a disputed use of a given sign.86 The many examples cited of {y} as a mater lectionis (pp. 51-55 [§21.341.2]; pp. 215-20 [§41.221.1]) are even less convincing, for four primary reasons: (1) an affirmative particle -y exists in Ugaritic, and T. provides no criteria for distinguishing this particle from the mater (he does so no more here, pp. 833-35 [§89.3], than in his original article on the topic);87 (2) T. claims as examples cases where the vowel before the {y} cannot have been of the /i,e/ class (for example, in all examples claimed in §21.341.21b where the case can be determined, the noun in question is in the nominative);88 (3) in Hebrew, such consonants were not used to note short vowels before

86In most cases, T. presents quiescence as an option, one that is often dropped in the corresponding morphology section below. E.g., {yšì} in RS 2.[003]4 ii 32, 34 (KTU 1.14) is reconstructed on p. 37 as either /yasi‘/ or /yasî/, but below, p. 443 (§73.233.1), only /yasî‘/ is indicated. It is a mystery to me how he can consider quiescence followed by the use of {i} as a mater lectionis to be a serious option when the very next form cited, {tbi} in RIH 78/20:18 (KTU 1.169) is reconstructed as /tabu‘i/ (this should, of course, be /tabû‘/—see above, seventh general remark), for, if quiescence had occurred and if matres lectionis were used, the form should have been written {tbeh}. On p. 38, T. actually considers this an option for {tbeh} in RS 2.[014]1 v 7 (KTU 1.3—the form is mis-cited as {ybeh}), but below he vocalizes the form variously as /tabû‘/ (pp. 620, 643) and as /tabu‘/ (alternative on p. 643), i.e., without quiescence of the /≥/ in a /YQTLØ/ form, in spite of the irregular orthography that such a vocalization assumes. On this verb in particular, see above, note 20 and remark to p. 37 (§21.322.5a), etc.


88The attempt below, pp. 306-7 (§51.121.1-2), to explain these examples as exhibiting the oblique ending in the nominative case may not be accepted. The clearest example is the phrase ìly ìgōrt in the letter RS 15.008:4-5 (KTU 2.16), which is certainly the subject of the verb. It boggles my mind that so many, T.
Rabbinic Hebrew/Aramaic (in which forms of the languages vocalic length is no longer phonemic), but in at least the case of the Ugaritic preposition b in its regular proclitic function there is no reason to believe that the vowel was long:89 (4) in other West-Semitic languages where matres lectionis are used (e.g., Arabic, Aramaic, and Hebrew) several consonants are used more or less systematically, but in Ugaritic {y} would be the only sign used with any frequency (there are no claimed cases of {w}, which is one of the principal matres in all of the languages cited, only a few highly dubious cases of {h}, and the few plausible cases of {≥} are closely tied in with the peculiar three-{≥} system of Ugaritic). This should not be taken as a denial that the particle -y was used more frequently after /i/-vowels,90 but that is not the same grammatical phenomenon as the use of matres lectionis—as is shown by the absence of {w} after /u/-vowels, absent because there is no post-positive w particle!91 T.’s stance seems oddly influenced by the later usages of matres lectionis in that he takes the Ugaritic phenomenon as a weird form of such a practice rather than as a broader usage than he is willing to accept of an admittedly Ugaritic particle.92

— pp. 37-38 (§21.322.5), p. 134 (§32.221.2), p. 187 (§33.273). No form cited on pp. 37-38 is of a sort to prove that the preceding vowel lengthened when /≥/ quiesced; indeed, in none of the cases cited here is it proven that /′/ did quiesce. Nevertheless, in the other two paragraphs cited, quiescence of /′/ with the preceding vowel resulting in a long vowel is given as a rule (albeit with T.’s characteristic indecision as to what really happened: /a′/ is indicated as going either to /â/ or to /ô/). All of the examples are from verbal forms ending in /′/ of which T. finds the orthography unexpected, e.g., {ymẕa} where he expects /yimṣa′/, which should be written /ymṣi/. I believe it safe to say that the presence or absence of {y} in III-y verbal forms in poetry is not regular enough to require that the rare unexpected uses of the{≥}-signs be interpreted as denoting quiesced /′/. It is also possible that in a text such as RS 2.[012] (KTU 1.12), which shows certain orthographic peculiarities, {ymẕa} reflects /yimṣa′/, i.e., that the {≥}-sign was chosen on the basis of the preceding vowel because...
there was no following vowel. The least plausible example cited here is \{yś\}, which T. takes as \[yiśšō] < \*yinša\," in keeping with his hypothesis that /a\'/ contracts to /ô/ (pp. 33-35 \[§21.322.1\], p. 180 \[§33.231.3\]; see remark below to p. 621 \[§75.232\]). One final example: though T. admits that the imperative of Niśº written \{šâ\} may be the lengthened form (/ša\'a/),\(^93\) indeed on p. 429 he deems that explanation "wahrscheinlicher," he nevertheless includes it here as an example of /a\'/, which here has become /â/ rather than /ô/! Without wishing to do Ugaritic grammar on the basis of Hebrew, one may nevertheless point out that it is precisely this type of quiesced /≥/ that did not show "compensatory lengthening" in proto-Hebrew, for 'he will find' is /yimsâ/, not /yimsô/ (i.e., is vocalized with qamesº rather than with holêm, showing that proto-Hebrew had /a/ here rather than /â/, which would have gone to /ô/ by the 'Canaanite shift').

— p. 37 \(\{§21.322.5a\}\), p. 175 \(\{§33.215.21b\}\), p. 186 \(\{§33.26\}\), p. 443 \(\{§75.233.1\}\), p. 452 \(\{§73.244.1\}\), p. 455 \(\{§73.262\}\), p. 526 \(\{§74.232.21\}\), p. 620 \(\{§75.232\}\), p. 643 \(\{§75.522\}\), p. 729 \(\{§77.38\}\), p. 913 \(\{§97.10.2b\}\). The form \{ubâ\}, 'I will indeed enter' (RS 24.244:72 [KTU 1.100]), is the only form of the root B', 'to enter, arrive', that T. considers valid as an indicator of the stem vowel of this verb. (I would add \{ubû\} in RIH 78/20/18 [CAT 1.169]—see here below and the remark to p. 196 \(\{§33.322.3c\}\), etc.) The \{û\} is anomalous, but he proposes (p. 175) that it shows vowel harmony with the stem vowel, which would, therefore, have been /ü/. This now appears more plausible to me than my vocalization "≥ubâ\'a,"\(^94\) for which there was no good theoretical basis. The problem is directly related to the Hebrew data: unlike other hollow roots, B' does not show different indicative and jussive/ 'wâw-consecutive' forms: /yâqûm/ vs. /yâqôm/ and /wayyâqom/, /yâšîm/ vs. /yašêm/ and /wayyâšêm/, but /yaḇo\(\acute{\i}\)/ for all three. The use of a single form must be owing at least in part to the quiescence of the \{ālîf\}, but does the /ô/ come from the /ā/ of a proto-Hebrew indicative /yaqâlu/ or from the /u/ of a proto-Hebrew jussive/perfective /yaqûl/ (derived from theoretical /šyaqûl/)?? The fact that the preformative contains /a/ argues strongly in favor of the latter solution (contrast /yēbôš/ ← /yibâšû/) and the Ugaritic evidence appears more plausibly explained along the same lines, admitting T.'s explanation of the \{û\}. That said, the vocalization of \{tbi\}, 'you must (not) enter' (RIH 78/20/18 [CAT 1.169]), as /tabû'/ (on pp. 37, 443, 620, T. writes "tabû,' considering that the stem vowel of hollow roots represents a contraction) cannot be accepted: because closed syllables did not tolerate long vowels in proto-West Semitic (hence the Hebrew jussive/ 'wâw-consecutive' forms cited above), the Ugaritic form must be either /tVbû/\(\acute{\i}\)/, i.e., the /\(\acute{\i}\)/ must have quiesced leaving the syllable open, or /tVbû/\(\acute{\i}\)/, i.e., theoretical /štabû/ has become /šVbû/ (T. proposes /u/ for this very same form on p. 643). Because the entire Hebrew paradigm reflects the latter form (viz., /yabû'/ ← /yâbô\(\acute{\i}\)/), it must be accepted as the only really plausible vocalization of the Ugaritic /YQTLØ/ form. On the basis of the Hebrew data alone, which the Ugaritic data appear to fit, at least not to contradict, the conclusion must be drawn, as indeed was to be expected, that the existence of the jussive/perfective form of B' with /u/ as the stem vowel

\(^93\)Here T. vocalizes /ša\'â/. On the length of this vowel, see remark below to pp. 428-29 \(\{§73.14\}\), etc.

pre-dates the quiescence of /u/. Finally, the possibility must be kept open that the vowel harmony occurred not just in the first person but was characteristic of the entire paradigm, i.e., the vowel harmony that produced the shift from /a/ to /u/ in the preformative syllable may not have occurred only in the environment of the consonant /v/ (see below, note 422).


T. vacillates considerably in his treatment of the admittedly difficult form yrāım in RS 2.[022]+ ii 6 (KTU 1.5): he proposes that it may be (1) emended to {yrā . nn}, which he would take as a /QTLa/ form (this possibility is mentioned on pp. 38, 481, 485, 622); (2) analyzed as a narrative infinitive (on p. 38 the form is incorrectly identified as an “Inf.abs.”⁹⁵) + energetic ending (pp. 38, 481, 485, 500, 622, 732); (3) analyzed as infinitive + energetic ending + pronominal suffix (pp. 38, 500, 506, 638). None of these solutions appears likely, for there is no proof that enerics consisted of morphemes that could be attached to the /QTLa/ perfective or to the infinitive used narratively (on T.’s theory regarding the enerics, see below, remarks to pp. 497-506 [§73.6], etc.). It is far more likely, therefore, that the form consists either of the infinitive + enclitic -nā (the productive enclitic morpheme, not that morpheme as possibly at the origin of the energetic verbal forms) or as the infinitive + the secondary pronominal suffix -nnu (which arose from fusion of the energetic morpheme -an with the pronominal element -hu and was subsequently re-analyzed as pronominal in nature⁹⁶).

— pp. 40-50 (§21.33). Whether T.’s hypothesis regarding {rå} according to which that sign represents a new phoneme, the dental affricate /v/ (see below, remarks to p. 497-506 [§73.6], etc.). It is far more likely, therefore, that the form consists of the infinitive + enclitic -nā (the productive enclitic morpheme, not that morpheme as possibly at the origin of the energetic verbal forms) or as the infinitive + the secondary pronominal suffix -nnu (which arose from fusion of the energetic morpheme -an with the pronominal element -hu and was subsequently re-analyzed as pronominal in nature⁹⁶).

— pp. 44-49 (§21.335.1-2). The only new word in the texts from 1994-2002 for which the writing with {rå} is attested is {‘pq}, ‘boundary marker’ (RS 94.2965:6 and 9, alongside {‘pq} in lines 3 and 12).

— p. 44 (§21.335.1b), p. 103 (§32.143.24), p. 404 (§69.172a). The text RS 1.012 (KTU 4.14) hardly allows for the interpretation of ḫswn/ḥswn as “(Kopf-)Salat” (preceded with a cautious “evtl.” or “etwa” or a question mark). This provides a good example of the danger alluded to in my opening remarks: until one examines every text in equal detail, one cannot write such a work as this with full awareness of the problems involved in the interpretation of every detail of every text. Until I began my work on RS 1.012 (see above, note 15), I

---

⁹⁵On the impropriety of this term in Ugaritic grammar, see below, remark to p. 652 (§75.529). If one uses it as shorthand for a comparison with Biblical Hebrew, it should be enclosed in quotation marks or some other indication should be provided that there was no “infinitive absolute” in Ugaritic that would have been morphologically distinct from the verbal noun that functioned as a normal infinitive.

⁹⁶Cf. S. B. Parker, Studies in the Grammar of Ugaritic Prose Texts (dissertation, Johns Hopkins University; Ann Arbor: University Microfilms, 1967) 19-20; Pardee, JNES 43 (1984) 244-45, n. 14. Cf. below, remark to pp. 222-23 (§41.221.52c), etc. On attaching the secondary pronominal morphemes -n and -nn to verbal nouns and adjectives, see remarks below to p. 223 (§41.221.52c), etc., and to p. 500 (§73.611.2f), etc.

had no strong opinion on whether ḫswn should mean '(a head of) salad' (cf. Akk. ḥassu) or 'thyme (or some closely related plant)', in the latter case the term would designate the green or dried leaves of the plant (cf. Akk. ḫasū). I have come to the conclusion, however, that RS 1.012 contains a list of high-class rations for an unnamed group to be used over the space of a month. Because of the perishability of lettuce, the word more plausibly designates therefore 'thyme' in that text.98 Thus the etymological comparisons with words meaning '(head of) lettuce' for the elucidation of the function of {ṣ} cited here are inapplicable. A more cautious approach would have listed the etymological connections for both interpretations. But, when writing a work such as this, one must make decisions.

— p. 45 (§21.335.1d). I have given my reasons for disbelieving that šṭm in RS 15.062:6 (KTU 4.158) means "zwei (Wagen-)Plattformen" and that šlt māt trm in the same text (line 7) means "300 Deichseln" (i.e., chariot shafts)99. As in the preceding case, therefore, the etymological connections with the meaning 'chariot platform' have no relevance for the function of {ṣ}.

— p. 46 (§21.335.1f), p. 249 (§51.3e). I see no reason to consider Ugaritic ks to be a loan-word from Akkadian when it is attested in all the major branches of the West-Semitic languages: if the word was borrowed, it must have entered proto-West-Semitic, not directly into Ugaritic. However that may be, the vocalization /kās/- (p. 249), viz., as though the vowel were the result of contraction, reflects neither Akkadian kāsu nor any of the later Northwest-Semitic forms (Aramaic/Syriac kās, Hebrew kōs). Only Arabic ka’s- breaks the pattern, and if it represents the original form, then the West-Semitic word is certainly not an Akkadian loan-word, for either the other West-Semitic languages should also show /ʼ/ or else the /ʼ/ should not be present in Arabic because the word would have been borrowed after the loss of /ʼ/ in Akkadian. Because the /ʼ/ appears only in Arabic, it appears more likely that it is secondary there and that the original form was /kās/-; the data do not allow a decision as to whether the word was originally East- or West-Semitic or simply common Semitic.

— p. 46 (§21.335.1g). The basis for the new interpretation of kṛṣū as "ein Flüssigkeitsmaß fur Öl," rather than as "ein Mastfutter; alternativ: 'Masttiere'"100 is to be found in the texts from the 1994 excavations cited here as attestations of the word but without the context proving the interpretation. The data are, however, somewhat garbled and incomplete, probably because they were transmitted by e-mail: kṛṣi is found in RS 94.2392+9, kṛṣū in RS 94.2563:28′ and RS 94.2600:14, kṛṣim (dual oblique) in RS 94.2395:2 and RS

98This conclusion is supported by the appearance of the phrase ḫswn ḥrb, 'dried ḫswn', in a new text (RS 94.2600), for thyme is used in either green or dried form whereas such is not the case, to my knowledge, of lettuce (cf. Pardee, AuOr 20 [2002] 173).

99"Trois comptes ougaritiques: RS 15.062, RS 18.024, RIH 78/02," Syria 77 (2000) 23-67, esp. p. 32-34. Judging by the prices indicated, šṭm should mean 'two mares' and šlt māt trm should mean 'three hundred doves'. Considerations of price aside, it should be obvious that 'three hundred chariot shafts' could have no direct connection with 'two chariot platforms', as T.'s citation of the former in support of the latter interpretation would lead one to believe his view to be.

94.2392⁺:4, kṛṣāt (plural) in RS 94.2392⁺:12. The fact that two of the new occurrences of kṛṣū are in the nominative and the form is each time written with {ṣ} and {ū}, undermines T.’s hypothesis that the writing with {ṣ} in RS 16.198A⁺:17 (KTU 4.225) is owing to the fact that the /s/ is there followed by /i/ which produces the pronunciation [śs].

— p. 47 (§21.335.1j), p. 180 (§33.231.22c), p. 472 (§73.411, .412). In the first reference cited here, {sknt} in RS 15.012:2 (KTU 4.135) is classified tentatively as a personal name, in the others as a G-stem participle, in fact the feminine form of sākinu, ‘governor’. Because there is no other reference in the Ugaritic texts to a female governor and because sknt in RS 15.012 is modified by what looks for all the world like a masculine gentilic (syny, the man from Siyanu’), the hypothesis that the word is a proper name must be preferred. However that may be, the paucity of forms hardly allows for the use of the word “Regel” (p. 472) to describe the feminine G-stem participle, which, according to the rule, would be /qāṭilt-/ ← /qāṭīlt-/: two forms speak in favor of the rule ([qrît] /qārī’t-/- and [ylt] /yālīlt-/- ← /yālidt-/-), though for the time being only this {sknt} speaks against it and it may be irrelevant because the base form might be different.

— pp. 48-49 (§21.335.2d). The reading {[ymṣ]š h[m …]} (misprinted here as {[ymṣ]s h[m …]}) in RS 5.300:3 (KTU 1.71) may not be considered epigraphically likely.

— p. 49 (§21.335.2e). The interpretation of {ḥši} in RIH 78/16:7 (CAT 1.171) as an aberrant writing of ḥdt, ‘month’, is made dubious by the absence of other aberrant writings in this text. With some exceptions, a given writing system tends to be observed throughout a given text.

— p. 51 (§21.341.13). T. claims that final {’} in foreign proper names has a “vokalische Funktion”, but I see no reason to doubt that in a name such as tlmî/tlmi (nominative and genitive forms), the scribes were noting a consonantal pronunciation, /talmî’u/ and /talmî’î/, or /talmî’u/ and /talmî’i/.

Nor do I see any particular reason to doubt that the {ū} of {ḥlū} is consonantal (the identification with Akkadian ḥalū, “ein Wollkleid,” though not impossible, is uncertain, for the word occurs only once, as part of the name of a feast: {ḥlū . dg} in RS 19.015:12 [KTU 1.91]).

— p. 51 (§21.341.21a), p. 67 (§21.383), p. 176 (§33.215.31b), p. 219 (§41.221.17), p. 338 (§55.12). In addition to the general considerations offered above with regard to the putative use of {y} as a mater lectionis (remarks on pp. 35-37, 50-55), it must be stated here that it is highly unlikely that the {y} of {ūhy}, ‘my brother’ (nominative case), is a mater (pp. 51, 219), i.e., that the word was pronounced /’užî/, for the writing of ʿâh, ‘brother’, with {ū} for {â} reflects the pronunciation of the word, wherein the first syllable shows vowel harmony

103Indeed, the {ū} may not belong with {ḥl}: at least in theory, it could be the conjunction /’ū/. About all that can be said about {ḥlū} is that we know neither that the three signs represent a single word nor, if they do, what that word means (cf. Pardee, Les textes rituels [2000] 507-8; idem, Ritual and Cult [2002] 215).
with the long case vowel /û/ (i.e., /ˈahlûya/ → /ˈahlûya/).\textsuperscript{104} The \{y\} here is not, therefore, the post-positive particle, as I would judge the examples of -y attached to other words cited in §21.341.21a (p. 51) to be, but of the 1 c.s. pronominal suffix that has retained its consonantal character when attached to a noun that ends in a long vowel. This is proven by \{ûhy\} itself, which T. treats differently in the sections cited: he holds explicitly on pp. 67 and 176 that the \{û\} is owing to vowel harmony with the nominative case vowel, ignoring in those statements that, according to p. 219, the 1 c.s. suffixal form is claimed to have been /ˈahlî/. It is simply implausible that /ˈuhlî/ would have arisen from /ˈahlî/. One must conclude that \{ûhy\} was pronounced /ˈahlûya/;\textsuperscript{105} the writing \{iûy\}, attested for the genitive case, reflects a pronunciation /ˈihîya/; \{âhy\}, attested for all three cases, either reflects an older and 'more correct' pronunciation (/ˈahlûya/, /ˈahlûya/, /ˈahlîya/) or a different scribal tradition (a 'morpho-phonemic' one, as T. terms it, i.e. historical writing). On p. 219, T. cites a single case from poetry which he considers to prove that \(abh\) bearing a 1 c.s. pronominal suffix was written without \{y\} (RS 3.340 i 24 [KTU 1.18]), alongside two cases of \(abh\), 'father', which T. presents as similar;\textsuperscript{106} on p. 176, he explicitly vocalizes \{âhy\} as /ˈahlû-ya/, though he abstains from fully vocalizing the forms \{ûhy\} and \{iûy\} (he indicates the stems as /ˈuhlû-/ and /ˈihî-/).\textsuperscript{107} Given that the writings \{ûhy\} and \{iûy\} are attested only in prose, I would hold that these writings show either that the three poetic forms were not in fact pronominal (the explicit expression of possession is not always necessary, especially in poetry\textsuperscript{108}) or that alternative forms existed (older /ˈahlîya/, which became /ˈihîya/, alongside younger /ˈahlî/, the latter of which would have arisen by analogy with nouns with a short case vowel). As regards the latter interpretation, Hebrew shows a similar situation with the 3 m.s. suffix, which appears as both /ˈahlîw/ and /ˈahlîhî/. Against that interpretation of the Ugaritic data, however, stand two facts: (1) 'my brother' is never written simply \{îh\}, as one might expect if the suffix were in the nominative pronounced /-î/, i.e., why would /ˈahlî/ not have become /ˈihî/?;\textsuperscript{109} (2) one would expect the prose forms to be the more recent, not the older. It


\textsuperscript{105}Van Soldt, Studies (1991) 403 n. 9.

\textsuperscript{106}On \(abh\), see below, remark to p. 175 (§33.215.22), etc..

\textsuperscript{107}On pp. 248-49 (§51.3a), p. 253 (§51.41a), and p. 338 (§55.12) T. indicates why he vocalizes the stems of \(ab\) and \(aḥ\) with a transliteration showing contraction: the original roots would have been 'BW and 'HW. This hypothesis is borne out by the feminine forms /ˈahlît/, 'sister', and /hâmît/, 'mother-in-law (on husband's side)'; in Hebrew (with corresponding forms in other Semitic languages), for the /î/ from /ā/ appears before the gender marker in these words and must, therefore, have arisen from contraction of radical elements rather than from an originally long case vowel, as is theoretically possible for the masculine form of these words. See further below, remark to pp. 248-49 (§51.3a).

\textsuperscript{108}On the interpretation of yd’tk bt, ‘I know you, daughter’, see below, remark to p. 215 (§41.221.11c), etc.

\textsuperscript{109}In this respect, T.’s presentation of the data is misleading: on p. 67, he introduces the contrasting usages of forms with /ā/ over against those with /ū/ and /i/ as occurring in “Stat. cs./pron.” without making clear that the latter occur only in the pronominal state; on p. 176, he explicitly describes the forms with /ū/ and /i/ as “St. cs./pron.” Though the section on p. 219 is devoted to the pronominal forms with \{y\}, the reader
appears necessary to conclude, therefore, that the poetic form \{\text{âh}\} adduced on p. 219 is either not pronominal or else that it stands outside the regular pattern for the noun \{\text{âh}\}. These considerations require that two orthographic paradigms for prose usage of the noun \text{âh} with pronominal suffixes be indicated for Ugaritic:

(1) Nominative Accusative Genitive
\{\text{ûhûy}\} = /'ûhûya/ \{\text{âhûy}\} = /'âhûya/ \{\text{iḥîy}\} = /'iḥîya/

(2) Nominative/Accusative/Genitive written with a single orthography, \{\text{âhûy}\}, that represents either the three pronunciations of paradigm 1 in ‘historical writing’ or three different pronunciations:

/'âhûya/ /'âhûya/ /'aḥîya/

In any case, explaining \{\text{ûhûy}\} as arising from /'aḥî/ has no plausible phonetic basis; thus disappears one of T.’s major cases for the use of matres lectionis in Ugaritic. (For further remarks on \text{âh} and \text{âb}, see below on p. 175 [§33.215.22], etc., where T.’s view that the case vowel of \text{âb} would have been short in the absolute state is discounted, and pp. 248-49 [§51.3]).

— p. 52 (§21.341.21a), p. 554 (§74.412.27), p. 739 (§81.12c), p. 808 (§85.2c), p. 883 (§94.21), p. 902 (§97.23a). While granting that \{\text{y≤msn}\} in RS 18.075:22’ (\text{KTU} 2.41:21 = \text{PRU} V 65:20) is difficult to interpret (\{ w l. âlp. ânk. \text{mnm} (21) ḫṣrîl. w . ūhy (22) \text{y≤msn} . ūm\}), T.’s various renditions are unfortunately characterized by the indecision to which reference was made in the introduction. In the second paragraph cited, the form is parsed as a 3 m.s. jussive bearing a 1 c.s. object suffix (the latter element of the parsing bears a question mark, but no other option is suggested) and is translated as though the function of the suffix was dative: “das soll mir(?) mein Bruder dort (auf Lasttiere) aufladen.” This dative translation is also given on pp. 739 and 808, without the question mark, and in both those paragraphs it is suggested that the loading would have been done on ships, rather than on pack animals. On p. 52, the suffix is not expressed in the translation (“Mein Bruder soll (es) dort aufladen”), while on pp. 883 and 902 the suffix appears to be expressed by “für mich,” with no question mark; in none of these last three translations is the mode of transport suggested. It appears far more likely to me that the suffix is third person rather than first and that its antecedent is either the feminine noun ḫṣrt\(^{110}\) or, less likely in my opinion, the indefinite pronoun \text{mnm} (taking ḫṣrt as verbal, in line with T.’s interpretation).\(^{111}\) Such morpho-syntax is completely regular and leaves only the question of why the stress was placed on loading rather than on sending. That question is most plausibly answered by reference to the role of the addressee’s servant who, according to lines 15’-16’ is empowered to speak on behalf of the addressee to the writer of the present letter (see below, remark to p. 425): according to lines 20’-22’, the addressee’s only job is to

\(^{110}\)Parker, Studies (1967) 13, 22, 28.

authorize the transport of the merchandise, the servant will take care of the rest. Because the beginning of the letter has disappeared and with it the identification of the correspondents, there is simply no way of knowing what would have been the preferred mode of transport.

— p. 52 (§21.341.21a), p. 220 (§41.221.2), p. 816 (§87.21), p. 872 (§93.422.1). T.’s various renditions of the following phrase in the text just discussed (RS 18.075:23 [KTU 2.41:22]: [w . [û]ly . âl . yb’r{n}]) shows similar indecision regarding the function of âl, viz., whether that be positive or negative, and whether the {-n} on the verb be the 1 c.s. pronominal suffix: pp. 52, 872 “soll mich nicht im Stich lassen”; p. 816 “soll mich(?) nicht im Stich lassen”; p. 220 “er möge mich (nicht) verlassen.” One notes that none of these translations comes from the section devoted to âl as an “Affirmationspartikel” (p. 805-7 [§85.1]), as is to be expected, since T. correctly holds that âl does not appear in positive formulations in prose. It appears likely to me that the {-n} is indeed the 1 c.s. pronominal suffix, for (1) this verb is transitive and one would expect the object suffix to be expressed here and (2) the object must be the writer, not his “lack,” for one would hardly expect the recipient of the letter to ‘abandon’ or to ‘destroy’ there where he is what the writer is lacking. As for the meaning of ‘BR, although the question is truly up in the air, I believe that a better case can be made for ‘destroy’ than for ‘abandon’, as T. recognizes for the not-dissimilar case of RS 92.2010:23 (RSO XIV 50). Though there are texts in which ‘BR may plausibly be translated ‘abandon’, none requires that translation. In the present case, the phrase ‘And may my brother not destroy me’ plausibly means ‘may he not allow me to perish (by not sending me what I need)—the key word is HSR (see preceding remark), which must express real ‘need’ rather than a frivolous desire.

— p. 52 (§21.341.21b), p. 181 (§33.233), p. 287 (§52.5f), pp. 444-45 (§73.233.41-42), p. 459 (§73.273.4), pp. 537-38 (§74.333), p. 624 (§75.237f). The second paragraph of this list is devoted to a grammatical category set up on the basis of a single form, viz., {tnšân} in RS 24.247:+47 (KTU 1.103), which is claimed to prove the sporadic occurrence of /í/ → /á/ in Ugaritic (what is known in Hebrew grammar as Philippi’s law): “*tinnaš’ī’na → /tinnaš’ā’na/” (understood as a 3 f.pl. imperfective form). In the two paragraphs on pp. 444-45 the form is cited to prove that the 3 m.pl. and 3 f.pl. /YQTL/ forms were not identical; in §73.233.42, it is cited as “der Schlüssel zur Rekonstruktion” of the form. Indeed, it is the only form with an

---

112 I would, nevertheless, disagree with his analysis of âl ttn in RS 16..265:14 as negative (p. 724 [§77.325]). In my estimation all the verbal forms in lines 9-14 of this practice letter are positively polarized and the scribe included âl ttn to demonstrate his erudition (see Pardee and Whiting, “Aspects of Epistolary Verbal Usage in Ugaritic and Akkadian,” BSOAS 50 [1987] 1-31, esp. p. 11). This, therefore, is one of those exceptions that proves a rule.


115 T. does not explain here why in his view the syllable /a/’/ would be written {â}: it is, of course, because the /’/ would have quiesced and the {â}-sign have been used as a mater lectionis. He only includes this form, i.e., “/tinnaš’ānā/” on pp. 455 and 459.
[*] sign in the proper position and it is an unexpected sign! I remark (1) that the subject of this verb is mrḥy, (2) that, for that word to be a plural, the {*y} would have to be a mater lectionis (the topic treated in §21.341.21b), (3) that mrḥy appears twice in this text (lines 7 and 47?) and once more in another text of similar genre (RS 24.302:10' [KTU 1.140]) and it is unlikely that so rare a usage as that of the mater lectionis would appear twice in the same word in one text, three times across two different texts, (4) that the equivalent in the Mesopotamian texts, viz., {GIŠ.TUKUL}, normally appears without {MEŠ} (though the associated verb is usually in the plural), (5) that mrḥm is attested as a feminine plural (p. 287). I conclude that (a) the {*y} is not a mater lectionis, (b) the Ugaritic noun is not plural but singular, (c) the {*y} is, therefore, the archaic feminine morpheme /-ay/ (on which see remarks to p. 274 [§51.46k], etc., p. 276, [§51.5c], etc., and pp. 282-84 [§52.4]), (d) the verb {tnšān} is, therefore, 3 f.s. energetic and to be vocalized /tinnaši’an(na)/, and (e) the Ugaritic form of the omen may have come about because the first adapter into West Semitic of a Mesopotamian omen took {GIŠ.TUKUL} at face value as a singular.116 As the singular form mrḥ appears to be attested as a masculine (tšʾm mrḥ ʾḥd kbd, ‘91 spears’ [RS 15.083:9-10 = KTU 4.169]),117 the feminine dual and plural, both written mrḥm (the gender is determined by the use of the feminine form of the number, tt mrḥm, ‘two lances’), are perhaps to be classified, therefore, at least in the present state of the evidence, to have functioned as the dual/plural of the feminine singular form mrḥy.

— p. 52 ([§21.341.21c]), p. 84 ([§23.51]), p. 85 ([§23.522a]), p. 187 ([§33.275]), p. 755 ([§82.11]), p. 758 ([§82.12]), p. 800 ([§83.24]); cf. p. 172 ([§33.214.1]). Instead of lumping {by}, {ly}, and {ky} into a single category, where all would consist of /C≠/ + {*y} mater lectionis, it is more plausible to take the {*y} as the post-positive particle which could be attached to other particles each of which had its own history. As is shown by the syllabic transcriptions of b and l, respectively {bi-i} and {le-e},118 these particles had different realizations, each of which plausibly had its own historical/etymological origin119 rather than resulting from phonetic conditioning.120 Thus {by} = /bi+ya/,121 {ly} = /lē+ya/ (/lē/ ← /lay/), and {ky} =

116 On these several points, see Pardee, Les textes rituels (2000) 554.
117 Cf. T., p. 356 ([§62.431b]).
120 Huehnergard, Ugaritic Vocabulary (1987) 53, 262. Huehnergard’s not implausible hypothesis according to which [li-i] represents [le] is rendered implausible by T. (p. 187 [§33.275]) who, because of the alternative writing with {*y}, thinks that [li-i] represents [lē] (i.e., as [bi-i] must represent [bī] so [le-e] must represent [lē], itself a secondary lengthening of [le] which is in turn a coloring of original /lli/ in proximity with the consonant /l/—a coloring which would only occur here!).
121 See above, remark on pp. 37-38 ([§21.322.5]), etc.
It is perhaps possible, though not particularly likely that /bi/ had a long vowel;\textsuperscript{122} it is more likely that the syllabic writings {bi-i} and {le-e} were used to disambiguate the vowel quality of the polyvalent syllabic signs, i.e., {BI} could be read as /bi/ or /bé/ and {LI} as /li/ or /le/\textsuperscript{123} The argument in §33.275.2 (p. 187) that the forms of b and l with the 1 c.s. pronominal suffix, viz., {ly} and {by}, indicate that the vowel of each preposition was long is valueless, as T. himself recognizes (here and p. 219 [§41.221.17c]), for that is the expected form whether the vowel be short or long (unless the vowel be /u/): the 1 c.s. pronominal suffix on nouns is written [y] when the noun is in the genitive and accusative cases and the singular case vowels are assumed by all to have been short in Ugaritic. Why then devote a section to a valueless argument? I would observe here that, while {by}, ‘in me’, and {by}, ‘in’, were doubtless vocalized identically as /biya/, the preposition l + the post-positive particle would have been realized as /lêya/, while the same preposition + the 1 c.s. pronominal suffix would have been /layya/\textsuperscript{124} for paradigmatic reasons, i.e., the form would have developed in proto-Ugaritic before the /ay/ diphthong had monophthongized (cf. Hebrew /-ay/ [first common singular suffix on a dual or plural noun] which has developed from /-ay/ [the proto-Hebrew dual oblique morpheme] plus /-ya/ [the proto-Hebrew 1 c.s. pronominal suffix], i.e., /-ayya/ → /-ayy/ → /-ay/). — p. 52 (§21.341.12c), p. 185 (§33.243.15), p. 259 (§51.42c), p. 259 (§51.42c bis), p. 438 (§73.223.41), p. 441 (§73.223.42.12), p. 840 (§91.21a). It is telling that, in support of his translation of RS 3.334:7 by šnt ml•t (KTU 2.2) as “in einem vollen Jahr,” this is the only text cited on pp. 185, 259, in which a feminine singular adjective shows syncope of the /a/ vowel before the /t/ of the feminine morpheme. In addition, on p. 185, “gegenüber,” with reference to the form written {ml±t}, says more than the situation warrants: in the sections on phonology, that word is used to introduce one or more writings that vary from the one on which a particular point is based and which require a phonological or phonetic analysis that differs from that of the rule under discussion. Here, however, T. takes {mlit} in RS 3.334:7 as an adjective, which cannot be the case of {mlat} in RS 1.009:11 (KTU 1.46) and RS 24.253:3 (KTU 1.109), the example cited in contrast, because in these texts the preceding noun is ym, ‘day’ (b ym ml±t), which is masculine in gender. It appears quite clear, therefore, that b ym ml±t, “am Vollmondstag,” means literally ‘on the day of fullness’. T. agrees with this analysis, since he translates {mlit} by “Fülle”;\textsuperscript{125} he nevertheless considers both forms to be derived from a qatilat base, with {mlit}, according to T., showing syncope of the /a/ of the feminine morpheme, {mlat} showing retention of the /a/. Three remarks are called for with regard to these analyses: (1) the tablet is broken immediately after {mlit} in RS 3.334:7 and there is no surety whatever that the three words quoted above constitute a

\textsuperscript{122}Etymologically the vowel would have been short, but it is just possible that the citation form was heard as having a long vowel (so Huehnergard, \textit{Ugaritic Vocabulary} [1987] 53). I confess that I do not find even that particularly likely.


\textsuperscript{124}Not /liya/, as indicated by T., p. 197 (§33.322.42b).

\textsuperscript{125}One may, however, query the gloss “Vollmondstag” for mlât, since the word is only attested preceded by ym, ‘day’.

\textit{Archiv für Orientforschung} 50 (2003/2004) online version
single phrase: {by} could go with what precedes, {by ßnt} could belong together but not with {ml•t}, which need not be an adjective, or, as T. assumes, the three words could mean ‘in one full year’; (2) if the last interpretation of RS 3.334 be correct, there is no particular reason why the adjective ‘full’ and the noun ‘fullness’ should have had the same base (cf. Hebrew /mlº(≥)/, ‘fullness’); (3) judging from the later West-Semitic languages, syncope is unlikely in adjectives (or, to put it more precisely, the /*-at/ form, e.g., Hebrew -āh, is far more regular in adjectives than in nouns).

— p. 52 (§21.341.21c), p. 229 (§42.12), p. 665 (§75.534), pp. 693-94 (§76.346b). T.’s “n[eue] L[esung]” of RS 24.326:3 (KTU 1.143) as “k ypth yrḥ/yf ]1 hnd” is essentially correct. The transcription that I made available to him read: {(3) k ypṭḥ . yrḥ’ l (4) hnd} (with the word in line 4 written far to the right, under {yrḥ’ l}). The end of line 3 may only contain {ḥ} or {y} or that sign plus one other, and the most likely reading is, therefore {yrḥ’ l hnd}, ‘this month’. As regards the division of the first five signs, T. consistently represents this case as k ypṭḥ but in the case of the other liver omen containing a clause introduced by k (RS 24.312 [KTU 1.141]) he shows more ambivalence, dividing three times as k yqny but once, p. 665, indicating as certain both the reading ky qny and the analysis of qny as /QTLa/ 3 m.s. (That this analysis was preferred at some point in the preparation of this grammar is also revealed by the absence of yqny in its place on p. 662 in the listing of III-weak /YQTLu/ forms.)

— p. 53 (§21.341.22a), p. 144 (§33.115.42), p. 204 (§33.442). Though {mnḥyky} may represent a rare mater lectionis, or, more likely, an historical writing or even a noun of the form {mnḥ} + {y}, its vocalization as “minḥika” cannot be accepted without query, nor the omission of this word from the discussion of the form of the plural of nouns of the types /qat/, /qitl/, and /qutl/ (pp. 295-96 [§53.322.1-3]). Both etymology and the Hebrew forms (sing. /minḥā/, pl. /m’nḥāḥ/) show the noun to be of the /qitl/ type, with an expected plural base form /minaḥ-/ (i.e., with the disyllabic base characteristic of “segholate” nouns in Hebrew).

— p. 53 (§21.341.22b), p. 602 (§74.626.3d). The facts that šlyt is attested three times on two different tablets (RS 02.[014]+ iii 42' [KTU 1.3], RS 02.[022]+ i 3, 29 [KTU 1.5]) and that the form without {y} does not occur make of these occurrences totally unconvincing examples of {y} as an internal mater lectionis. Furthermore, if. T. is convinced that the orthography represents a /qat(t)iil/ form, why is this example not listed below, p. 261 (§51.43c) or p. 264 (§51.44i)?126 More plausible is the analysis suggested on p. 602, according to which šlyt would be a Š-stem verbal adjective (or noun, I would think) of a root LYT/LWT˚ of which the basic meaning would be ‘wrap, envelop’ perhaps referring to the serpent’s ability to coil up.127

126 In truth, T. has no category /qattil/ per se: §51.44i is in fact “[qa/iitil].”

127 The suggestion goes back to J. Gray, The Legacy of Canaan. The Ras Shamra Texts and Their Relevance to the Old Testament (SVT 5; 2d, revised ed., 1965) 31 n. 3 (in the first edition of this work, p. 27, Gray had adopted the traditional view that the root of šlyt was SLT). It is also possible that the term means something like ‘quick, active’ (cf. Arabic layyāt-, with that meaning).
— p. 54 (§21.341.23a). T. offers no proof in support of the assertion that the second [y] in personal names ending in two [y]s is a mater lectionis for short /i/. In a grammar as discursive as this one, simple assertions of dubious categories appear out of place. How do we know that these are not simply by-forms of the shorter names? Why is the graphically similar phenomenon of place names spelled with and without [y] at the end\(^{128}\) not discussed in §21.341.23?

— p. 54 (§21.342.1a), p. 63 (§21.355.1b), p. 155 (§33.135.2), p. 444 (§73.233.3). The interpretation of {trğnw} in RS 24.244:61 (KTU 1.100) as containing {w} mater lectionis which is preferred on p. 54 becomes “sehr wahrscheinlich” on p. 444. I can hardly accept the latter judgment for a text as replete with scribal error as this and when an alternative morpho-syntactic analysis of the verb has been proposed that removes the opportunity for the mater lectionis.\(^{129}\) Note that T.’s analysis also requires an aberrant root derivation, RGN ← RGM (p. 155). All this to avoid a textbook case of dittography—this verb is followed by the conjunction w.

— p. 54 (§21.342.1a), p. 426 (§73.121.1b). In the first section cited, the signs {ïtrhw} in RS 15.007:6 (KTU 2.15) are given as a possible example of [w] used as a mater lectionis ([hw] would be an objective suffix attached to {ïtr}, which would, perforce, be a verb); it is admitted that [hw] could be the 3 m.s. independent pronoun. In the second section cited, the context in which {ïtr} appears is said to be too obscure for analysis. As there are no convincing examples of [w] used as a mater in Ugaritic, it is hardly this passage that will provide the “smoking gun.” Because of other irregular usages of the {ï}-signs in RS 15.007, with at least one case of [i] for [á] ([itn], ‘I will give’, in line 4), I propose that {ïtr} here stands for standard [âtr] and [ïtr hw] means ‘wherever he may be’, lit. ‘(in whatever) place he may be’ (/ïïtra huwa/ or, if the spelling represents only an aberrant usage of the {ï}-sign, /âtra huwa/).\(^{130}\)

— p. 54 (§21.342.1b). Instead of simply taking [kwt] in RS 20.010:6 (KTU 4.691) as the same word as kt and the [w] as a mater lectionis, one may consider that kwt is not the same word as Akkadian kütu, but an Egyptian loan-word\(^{131}\) or that kwt, with consonantal [w], is a dual form distinct from the singular kt.\(^{132}\) Finally, the possibility that Ugaritic had a singular noun kw of which kwt would be a feminine variant must be considered (see remark below to p. 390 [§§69.123, 69.124.1]).


\(^{130}\)Context III (2002) 108; the full argument for this interpretation will be found in Les textes épistolaires (in preparation).


— p. 55 (§21.342.3), p. 57 (§21.352.1), p. 66 (§21.362), p. 503 (§73.627). As seems clear from the descriptions on pp. 57, 66, of the problems of the correct reading of {šrnâ} in RS 2.[003] iv 50 (KTU 1.14) as šrmn, and from the grammatical analysis on p. 503, the notation {š³rûn³} on p. 66 is incorrect (on p. 919, the use of the exclamation mark is defined as marking the correction of an ancient mistake, not the ancient mistake itself). On the correct reading of this word, see below remark to p. 66 (§21.362).

— pp. 56-65 (§21.35). In his introduction to this section on miswritings of various kinds, T. claims to include only “relativ gesicherten Schreibfehlern” (p. 56). In the following remarks, I point out epigraphic data that are at variance with some of these proposals and query proposals for emendation that I consider to be uncertain and which do not bear the question mark.

— pp. 56-67 (§§21.35-38). Two general remarks to these sections devoted to various types of scribal error. (1) Too significant a portion of the examples listed are either taken over from KTU or CAT or consist of new readings proposed by T. Because of the nature of the data, particularly those that relate to relatively minor differences between signs, to ancient corrections, overwritten signs, etc., the validity of the examples may only in many cases be determined by autopsy. Because such was, in most cases, the method neither of CAT nor of T. himself, one must be more suspicious of T.’s lists of this type of phenomena than of his grammatical analysis of forms of which the reading is clear. (2) Several of T.’s examples of scribal errors belong to the category of error that would have been owing to misperceptions of the eye, i.e., to copyist’s errors in the narrow sense of the phrase. Up to the present, however, there is virtually no data indicating that the scribes of Ugaritic texts copied from one cuneiform tablet to another. The surest such evidence would be duplicate texts, of which there are virtually none. Indeed, not a single scribal error is cited here from the one genre for which we have quasi-duplicates, that is, the hippiatric texts, of which four are presently known. Though such negative evidence does not, of course, prove that the practice did not exist, it does indicate that such explanations belong to the realm of speculation to a greater degree than in cultures where multiple copies of a given text are well attested.

— p. 56 (§21.352.1 {t} für {n}). In RS 10.090 (KTU 4.53:7) the word in question is written {bn} not {bt} (in the other text in this list that I have collated, viz., RS 11.844:13 [KTU 4.98], the reading {bt} is correct). The question remains, of course, whether the true cases of {bt} are mistakes or represent bt, ‘daughter’.

— p. 57 (§21.352.1 {t} für {m}). With regard to the proposed correction of {ddt} to {ddm} in RS 15.080 (KTU 6.19), the spelling {ddt} is attested another time, in the letter RS 17.139:31 (KTU 2.34:33). It cannot be considered certain that two examples of the

---


135{ddt} instead of {ldt}, which T. elsewhere interprets as meaning ‘giving birth’: cf. CAT, p. 176, n. 5, and here below, remark to p. 257 (§51.41h).
spelling establish the existence of a plural in -t alongside the commonly attested ddm, 'jars', but the occurrence of the two examples should be noted.

— p. 57 (§21.352.1 [t] für [m]), p. 66 (§21.363), p. 352 (§62.202.5), p. 353 (§62.25). The first section cited here contains the proposal to emend {'śrt} to {'śrm'} in RS 19.165:7 (KTU 4.658); the second asserts that the {t} of the full number formula in that line, viz., {hmšt . 1 . 'śrt}, has been crossed out by the scribe; the third asserts that the latter is the correct analysis of the situation;136 the fourth that the emendation proposed in the first cited paragraph is correct.137 Only the first proposal is accompanied by a question mark. T. knows his Ugaritic, but has garbled his presentation here: only one or the other of the two proposed readings represents standard Ugaritic, i.e., the phrase should be either ḫmšt ‘śrt, ‘fifteen’ (pp. 66, 353—i.e., it is the t that was faulty and was erased by the scribe), or ḫmšt l ‘śrm, ‘twenty-five’ (pp. 57, 352—i.e., it is the final -t of the last word that is faulty). What is really disturbing is that the epigraphic solution proposed on pp. 66 and 352, which, if correct, should decide the issue, is ignored on pp. 57 and 353; in neither section where the epigraphic solution is put forward is it indicated as being in any way hypothetical.

— p. 57 (§21.352.1 [a] für [n]), p. 64 (§21.355.1c). The reader has difficulty accepting that ápānk (RS 2.[019] ii 5 [KTU 1.21]) is correctly identified as a relatively certain scribal error for {āpnnk} (p. 57) when the same form is classified as having an extraneous {ā} on p. 64 (“ap«a»nk”);138 the two entries are not cross-referenced and neither bears a question mark.

— p. 57 (§21.352.1 [a] für [n]). In RS 1.018:20 (KTU 2.4:19), the scribe added a tiny third wedge to the left side of {bāš}, apparently in order to correct the word to {bnš}; the original error pointed out by T. was thus corrected in antiquity.

— p. 57 (§21.352.1 [a] für [n]), p. 223 (§41.221.52c), p. 502 (§73.623), p. 589 (§74.622.3). In an “Anm[erkung]” to the category “[a] für [n]” on p. 57, T. debates whether the fifth sign of {tšḥānn} (RS 3.322+ iii 45 [KTU 1.19]) should be emended to {n} or actually read as [n]; in the other sections cited, he assumes the reading [n] to be correct with or without emendation. The original editor’s copy shows no ambiguity as to the reading of {ā},139 but I have not had the occasion to collate the tablet. T. assumes that the form denotes awaking from sleep (vHT) without bothering to refute the idea that it may express the act of causing harm (vHT’);140 the reason is apparently that the verb should be plural, for which the correct writing would be {tšḥānn}, i.e., /tašāṭi‘ūnannu/. The writing with {ā} would only be correct if the subject were singular or dual, which does not appear to be the

136“… die richtige Lesung lautet ḫmšt [[t]] / ‘śrt, d.h. ‘15 (Schekel).” One will note that “[t]” is normally the representation for an erased sign, not one that has been canceled by “cross-strokes” (§21.363 Durchstreichung).

137”hmšt l ‘śrt … ist zu … hmšt l ‘śrm‘ zu korrigieren.” One notes here the previously unmentioned correction of {hmšt} to ḫmš, perhaps simply a mistake on T.’s part as the construction with ḫmšt is presented as regular in §62.41b (p. 355).

138T. indicates extraneous signs by « ».

139It also does not show a word-divider after the {ā}, though the form is read as {tšḥānn} in KTU/CAT.

case (the parallel verb is \(t'\)pn from \(\sqrt{t}\)W)P and the subject is n\(\text{srm}\), apparently referring to raptors in general, not just to just two of them).

— p. 57 (§21.352.1 \{h\} für \{i\}), p. 60 (§21.354.1b). A new syllabic text containing a list of divinized kings that partially overlaps with RS 24.257 (\textit{KTU} 1.113) shows \{ma-ap-i\} in the slot where the alphabetic text appears to read \{\(\text{I}^{\prime}\)mph\}.\footnote{D. Arnaud, “Prolégomènes à la rédaction d'une histoire d'Ougarit II: les bordereaux de rois divinisés,” \textit{SMEA} 41 (1999) 153-73.} Thus, though the status of the \{\(\text{I}^{\prime}\)\} in the Ugaritic text remains uncertain, the generally accepted correction of the latter text to read \{\(\text{I}^{\prime}\)mph\(<r>_\text{pi}\)} loses in credibility. All in all, the reading of a partially preserved \{\(\text{I}^{\prime}\)\} appears to be faulty and the royal name is consequently \{mph\}, perhaps derived from the root PHY “to see.”\footnote{See Pardee, \textit{Ritual and Cult} (2002) 209-10 nn. 37, 44; Bordreuil and Pardee, “Un nouveau membre de la famille royale d'Ougarit?,” \textit{De la Tabilla a la Inteligencia Artificial. Homenaje al Prof. Jesús-Luis Cunchillos en su 65 aniversario} (eds.A.González Blanco et al.; Zaragoza: Istituto de Estudios Islámicos y del Oriente Próximo, 2003) 31-39, esp. pp. 37-38.}

— p. 57 (§21.352.1 \{h\} für \{i\}). \textit{KTU} 1.145 (24.328A) no longer exists: it is (correctly) indicated in \textit{CAT} as having been joined to \textit{CAT} 1.103 (RS 24.247\(^{+}\)); there is no \{hn\} for \{in\} in line 2 of \textit{CAT} 1.103 + 1.145.

— p. 57 (§21.352.1 \{s\} für \{l\}). RS 2.[004] vi 42' (\textit{KTU} 1.17) does not provide an example of this error, for the \{l\} of the first token of \textit{iy} in fact consists of three wedges. (Statements regarding fine details of writing based on observation of photographs of this particular tablet are particularly dubious, for the script was fine and lightly impressed; since the tablet was inscribed, the surface has suffered erosion. The correct reading can often be determined only by examining the original under magnification and even then a number of uncertainties remain.)

— p. 57 (§21.352.1 \{s\} für \{l\}). The correction of \{tdḥṣ\} to \{tdḥl\} in RS 16.379:21 (\textit{KTU} 2.32) cannot be considered relatively certain\footnote{Elsewhere, T. quotes the passage with (p. 52 [§21.341.21a], p. 729 [§77.392], p. 883 [§94.21]) and without (p. 315 [§54.214d]) an exclamation mark attached to the \{l\} of his emended form \textit{tdḥl}.} when DHL is not attested elsewhere in Ugaritic and a not totally implausible Arabic etymology exists for DḤṢ.\footnote{Pardee, “Ugaritic. Further Studies in Ugaritic Epistolography,” \textit{AfO} 31 (1984) 213-30, esp. pp. 225, 226.}

— p. 57 (§21.352.1 \{l\} für \{u\}) and p. 58 (§21.352.2 \{b\} für \{s\}). The first reference should have a cross reference to the second, for no basis for the proposed emendation is provided at the first mention, while four reasons are outlined on p. 58.

— p. 57 (§21.352.2 \{a\} für \{l\}) and p. 58 (§21.352.2 \{l\} für \{s\}). There is no reason to emend the first sign of \{āšlū\} in RS 17.139:31 (\textit{KTU} 2.34) to \{t\} (though the third sign should indeed be emended to \{s\}): the text may be interpreted with the writer’s speech present in both lines 31 and 33.\footnote{Idem, ibid., pp. 227-28. Below, pp. 623 (§75.237d) and 640 (§75.517e), T. himself parses the form \{āšsū\}.}

— p. 57 (§21.352.2 \{h\} für \{t\}). The scribe of RS 19.016 (\textit{KTU} 4.609) indeed first wrote \{ḥḥbm\} in line 20 but realized his error and effaced the lower wedge of the second sign to

\footnote{141D. Arnaud, “Prolégomènes à la rédaction d'une histoire d'Ougarit II: les bordereaux de rois divinisés,” \textit{SMEA} 41 (1999) 153-73.}


\footnote{143Elsewhere, T. quotes the passage with (p. 52 [§21.341.21a], p. 729 [§77.392], p. 883 [§94.21]) and without (p. 315 [§54.214d]) an exclamation mark attached to the \{l\} of his emended form \textit{tdḥl}.}


\footnote{145Idem, ibid., pp. 227-28. Below, pp. 623 (§75.237d) and 640 (§75.517e), T. himself parses the form \{āšsū\}.}
correct it to {t}; however, he did a sloppy job of rubbing out the wedge and the left edge remains visible. I believe, nonetheless, that we may legitimately respect his wishes by transcribing [ḥt bm], instead of [ḥt¹ bm].

— p. 58 (§21.352.2 {h} für {p}). Though the emendation from {h} to {p}, i.e., from three wedges to two in the same configuration, is certainly the epigraphically easier solution in RS 17.063:8 (KTU 5.10), emending to the conjunction w is lexically more plausible for p is very rare in lists.

— p. 58 (§21.352.2 {y} für {h¹}). p. 222 (§41.221.52b), p. 627 (§75.42), p. 661 (§75.532). The emendation from {ysynh} to {yshnh}, “er riß es heraus,” can hardly be counted as relatively certain when an interpretation of the extant text has been proposed that not only is semantically plausible but also conforms better to the pattern of phonetic mirroring of verb and noun visible in the other elements of this poetic unit. On p. 661, the extant reading of {ysynh} is parsed, but the emendation is still mentioned as an alternative.

— p. 58 (§21.352.2 {d} für {l¹}). The situation in RS 4.474:10 (KTU 1.65) is not as simple as T.’s listing of this line as a case of emending {b’d} to {b’l¹} would lead the reader to believe, for neither the upper nor the lower row of wedges consists of three wedges.

— p. 58 (§21.352.2 {d} für {b}). The first example, which reads {ālnb} in RS 24.261:21 (KTU 1.116), should have been back in §21.352.1 on p. 57, i.e., under “[b] für {d},” as T.’s own corrected reading as “alnd” shows.

— p. 58 (§21.352.3 {w} für {r¹}). By my collation, RS 2.[014]+ iv 30 (KTU 1.3) does not contain the only example of this scribal error indicated by T. Rather the second set of horizontal wedges of the {r} of ārs has suffered damage with the result of giving the superficial appearance of {w}. A closer examination reveals traces of two wedges.

— p. 58 (§21.352.3 {r} für {w}). The Hurrian element /iriri/ appears established and the emendation of {irîrîm} to {iwîrîm} in RS 16.402:2 (KTU 2.33) cannot, therefore, be counted amongst the relatively certain scribal errors.

— p. 58 (§21.352.3 {û} für {d¹}). Since the editio princeps, the proper name dnil in RS 2.[004] ii 8¹ (KTU 1.17) has been read as having {û} in place of the expected {d¹}. T. follows that reading tradition here. The surface here is, however, worn and cracked and examination with a binocular microscope reveals traces of at least the second head of a lower horizontal wedge. Though ’Ilîlimilkû was certainly capable of error, this one may, therefore, be removed from the list.


151 Ch. Virolleaud, La légende phénicienne de Danel (BAH 21; MRS 1; Paris: Geuthner, 1936) 196 and pl. VI.
— p. 59 (§21.352.4). At the end of the first paragraph of this section, T. refers to the scribal error that consists of writing \{‘nn\} for \{‘nt\}, but without a reference. In recent years, the most discussed text where some have considered \{‘nn\} to be erroneous is RS 22.225:1 (KTU 1.96), but there seems to be a growing consensus that the writing in that text is correct, that the reference is to the evil eye, not to the goddess Anat.¹⁵²

— p. 59 (§21.352.5), p. 163 (§33.152c), p. 164 (§33.153c), p. 200 (§33.323.4b,c). Because \{bwtm\} is only attested once (RS 24.249:9’ [KTU 1.105]) in place of \{bhtm\}, ‘houses’, it is difficult to accept that it represents a “phonet. Variante” (p. 164) of the regular form. Though one may accept that \{btm\} is a true variant of the plural \{bhtm\} in RS 1.019:4 (KTU 1.48), because of other aberrant writings in that text that may reflect a non-standard dialect or at the very least non-standard scribal usage, it is unlikely that the same categorization is valid for RS 18.079:14 (KTU 4.370), where the form appears in the common phrase ‘house-builders’ (normally \{hr$btm\}) in a text not characterized by spelling peculiarities; the latter instance appears therefore to be simple scribal error. As for the regular plural, T. hypothesizes that /bahat-/ is secondary for /bayat-/ but, as /h/ is a common triconsonantalizer in Ugaritic (examples p. 163 [§33.152c]) as well as in the other Northwest-Semitic languages (cf. /‘māḥôt/, ‘maids’, in Hebrew, or /‘bāḥāḥ/, ‘fathers’, in Aramaic), there is no particular reason to take the form with /h/ as a simple substitute for a theoretical plural form wherein the etymological /y/ would have been retained.¹⁵³

— p. 59 (§21.352.5 \{btw\}), p. 61 (§21.354.1c \{wm\}), p. 157 (§33.141.1 \{wm\}), p. 159 (§33.142.1 \{wm\}), p. 159 (§33.142.21 \{btw\}), p. 164 (§33.153b \{lw$\}), p. 226 (§41.222.4 \{lw$\}). Two words in RS [Varia 14] (KTU 3.9) show \{w\} for expected \{h\}: \{btw\} for \{bth\}, ‘his house’, in line 4, and \{lw$\} for \{ll$m\} (or \{lk$m\}), ‘to them’ (or ‘to you’), in line 6; \{wm\}, ‘and if’, for \{wm\} (or \{wh$m\}) in line 6, is similar, though not identical.¹⁵⁴ T. explains the first two instances on similar grounds and categorizes the third as intervocalic elision of /h/ (the three examples are explicitly compared on p. 164). When one compares the three cases with each other and with all other words in the text, one realizes that \{btw\}, ‘his house’, and \{lw$\}, ‘to them’, represent the only possible cases of /-Vhu-/, while \{wm\}, ‘and if’, represents the only example of /-a$’i-/ or /-ahi-/. The text seems, therefore, to reflect a non-standard and quasi-phonetic spelling that must be ascribed to the particular scribe who wrote this text: as T. proposes, the vowel following the /h/ in the first two cases colored that

¹⁵²See recently, J. Ford, “‘Ninety-Nine by the Evil Eye and One from Natural Causes’: KTU² 1.96 in its Near Eastern Context,” UF 30 (1998) 201-78, with due credit to the originator of the basic interpretation, G. del Olmo Lete; I have adopted this basic interpretation in Ritual and Cult (2002) 161-62.

¹⁵³Oddly enough, T. does not adduce the highly idiosyncratic Hebrew plural /b$â’$tim/ in any of the paragraphs cited, though he does cite it elsewhere (p. 167 [§33.171.3]) as proof of the assimilation of a weak consonant: the Hebrew form would be derived from **bay(a)$tîm” as /‘a$‘hû’mâ/, ‘brothers’, may have been derived from /‘a$‘h(V)wûmâ/!

¹⁵⁴CAT indicates \{lk$m\} and \{km\} as certain readings in line 6, but the photographs of the West Semitic Research Project that accompany Friedman’s article on this text in Maarav 2 (1979-80) leave no doubt that the sign in question is each time \{w\}. Color photographs of this tablet provided by the West Semitic Research Project have now appeared in Bordreuil and Pardee, Manuel (2004), text 40 in the Choix de textes.
consonant (the scribe heard, or himself pronounced, /bêtiwu/ and /lêhumu/ as [lêwumu]). The third case /wa≥imma/ or /wahimma/, is less clear, apparently involving intervocalic elision of /∫/ or /h/.\footnote{With unfortunate though characteristic ambivalence, T. represents the proto-form three times as {w•m}, but once (p. 164) speaks of intervocalic elision of /h/.} The end result was, however, in all likelihood [wimma] not “/wim/” (p. 157), for there is no reason to believe that final short vowels also dropped in this scribe’s dialect (idiolect?).\footnote{T. either indicates the conditional particle without /-ma/ or with the /-ma/ in parentheses: p. 152 (§33.131.1.3: “hm /him/ ← /si/ im /…/ im /im(ma)/ ← *him(ma) ← *si im(ma),” p. 159 (§33.142.1 [-ma in parentheses]), p. 162 (§33.151c [ditto]), p. 173 (§33.214.22): “/…/him/ /im/ /him/,” Because he takes the particle as ultimately cognate with Akkadian ūmma (p. 152) and because the /m/ almost certainly represents enclitic /-ma/, there is no reason to believe that the Ugaritic particle did not end in /-ma/.} The point being made here is that these three phenomena do not represent haphazard ‘errors’ in this text but systematic representations of phonetic realizations. T. would probably accept the latter formulation, for, in each individual case, he sees these writings as representing phonetic change, rather than simple error; he does not, however, address the question of the orthographic ‘system’ visible in this text nor does he state whether he believes that they represent how Ugaritic was ‘really pronounced’ whereas the vast majority of texts would represent formal scholastic orthography—a debatable proposition that nonetheless deserves airing—as opposed to one scribe’s view of things.

— p. 59 (§21.353), p. 166 (§33.161c), p. 644 (§75.522), p. 789 (§83.122b). In all these sections, T. accepts the standard reading of /ynl/ in RS 2.[004] i 5 (KTU 1.17) though he once suggested that the reading should be /yfâll/ or /yfnl/.\footnote{AfO 42-43 (1995-96) 270.} The /n/ is in fact reasonably well preserved and there is no doubt about the reading.

— p. 59 (§21.353), p. 257 (§51.42a). Though attempts have been made to justify the emendation from /drk/ to /dkr/ in RS 1.005:6 (KTU 1.43), it certainly cannot be counted among the relatively certain emendations (p. 59) and hence as an example of the noun dkr, ‘male’ (p. 257).\footnote{On the problems of the passage and for an attempt to interpret the text without emendation, see Pardee, Les textes rituels (2000) 234-36.}


— p. 59 (§21.353). An explanation would have been in order for why /śgtgy/ is preferable to /śgty/ in RS 18.005:1 (KTU 4.321).

— p. 60 (§21.354.1a), p. 204 (§33.441). To my mind, it is implausible that the examples of haplography of /t/, /m/, /r/, and /n/ at the end of words listed on p. 60 would be ‘correct’ “Pausalformen.” Because most such forms would have had a case or a mood vowel (the only clear exception is /YQTLØ/ verbal forms), it is necessary to accept that this could involve the elision of an entire syllable. It is far more likely, to the extent that the examples are valid, that we are dealing with simple haplographies, as in the cases of other omitted consonants listed on p. 60. Are we really to believe, for example, that ‘qšr was purposely

— p. 59 (§21.353), p. 166 (§33.161c), p. 644 (§75.522), p. 789 (§83.122b). In all these sections, T. accepts the standard reading of /ynl/ in RS 2.[004] i 5 (KTU 1.17) though he once suggested that the reading should be /yfâll/ or /yfnl/.\footnote{AfO 42-43 (1995-96) 270.} The /n/ is in fact reasonably well preserved and there is no doubt about the reading.

— p. 59 (§21.353), p. 257 (§51.42a). Though attempts have been made to justify the emendation from /drk/ to /dkr/ in RS 1.005:6 (KTU 1.43), it certainly cannot be counted among the relatively certain emendations (p. 59) and hence as an example of the noun dkr, ‘male’ (p. 257).\footnote{On the problems of the passage and for an attempt to interpret the text without emendation, see Pardee, Les textes rituels (2000) 234-36.}


— p. 59 (§21.353). An explanation would have been in order for why /śgtgy/ is preferable to /śgty/ in RS 18.005:1 (KTU 4.321).

— p. 60 (§21.354.1a), p. 204 (§33.441). To my mind, it is implausible that the examples of haplography of /t/, /m/, /r/, and /n/ at the end of words listed on p. 60 would be ‘correct’ “Pausalformen.” Because most such forms would have had a case or a mood vowel (the only clear exception is /YQTLØ/ verbal forms), it is necessary to accept that this could involve the elision of an entire syllable. It is far more likely, to the extent that the examples are valid, that we are dealing with simple haplographies, as in the cases of other omitted consonants listed on p. 60. Are we really to believe, for example, that ‘qšr was purposely

\footnote{With unfortunate though characteristic ambivalence, T. represents the proto-form three times as {w•m}, but once (p. 164) speaks of intervocalic elision of /h/.}
written without the {r} in RS 24.244:33-34 (KTU 1.100) because it was pronounced ‘/aqša/, viz., without a case vowel and without the final consonant, in “pause,” when the word appears ten other times in this text, in the identical position in the sentence and in the verse and it is always elsewhere written {‘qshr}—in a text in which there are several certain scribal errors? Or that words at the end of sentences in prose texts (several examples here—see following remark) were pronounced without the final syllable because they were in “pause”? — p. 60 (§21.354.1a), p. 204 (§33.441), p. 674 (§75.62a), p. 711 (§76.524.5a). The case of {ytb} in RS 3.322+ iii 2, 17 (KTU 1.19) is more delicate than the ones just criticized because it occurs twice. The context, where the act of breaking (√TBR) recurs several times and is actually repeated in the fulfillment of the request made in lines 2 and 17, appears to call for the emendation to {ytb<r>}, a notation that appears on p. 711 with no reference to either of T.’s other two explanations. In the first two sections cited, T. cites these examples as reflecting pausal pronunciation, and, since the verbal form is each time plausibly /YQTLØ/, the proposal may be said to be somewhat more reasonable than in cases where an entire syllable would have disappeared. T. also proposes an alternative etymology, a root TBB that would mean ‘to cut’ and that would in each verse appear in parallel with TBR (pp. 60, 674). Against both of these latter explanations must count the fact that TBR is used in both passages which recount the fulfillment of the command, both times in repetitive parallelism with another token of TBR (ll. 8-9 {ytbr} // {tbr}, ll. 22-23 {tbr} // {tbr}) and both times in the same prosodic slot as {ytb} in lines 2 and 17. — p. 60 (§21.354.1a), p. 287 (§52.5g), pp. 475, 476 (§73.426), p. 841 (§91.22). One would have wished for more clarity in the presentation of the case for the emendation from {bq≤} to {bq≤<t>} in RS 16.399:21 (KTU 4.247). In essence, three interpretations are considered, with no cross-referencing: (1) the absence of the {t} is a scribal error; (2) the writing without {t} reflects pausal pronunciation (see p. 204 [§33.441] and previous remarks); (3) the “Emendation” is not required because the feminine morpheme is unnecessary (only on p. 841 is this option considered). Moreover, the author’s position is not made any clearer by citing the form as “bqt,” i.e., without the sigla for emendation, on p. 287. — p. 60 (§21.354.1a), p. 204 (§33.44), p. 848 (§91.321c). The literary form of RS 24.255:16-23 (KTU 1.111) is so different from that of the other ritual texts that the latter may be said to provide a poor basis for emending {‘tt<r> šd} to {‘tt<r> šd}. Though certainly not easy, the text may be interpreted as it stands.\footnote{Pardee, Les textes rituels (2000) 623, 627.} — p. 60 (§21.354.1a), p. 627 (§75.42). With regard to T.’s emendation of {tgr} in RS 18.134:5 (KTU 2.44) to {tgr<k>}: a second example of the phrase {tgr tšlmk}, ‘may they (the gods) guard, may they keep you well’, is now attested (RS 94.2545+:16) and one must ask whether the emendation to obtain the more common form of the formula with the pronominal suffix on both verbs is necessary. — p. 60 (§21.354.1a), p. 61 (§21.354.1d), p. 238 (§43.2d). Emending the text of RS 18.079:2 (KTU 4.370) in order to do away with the so-called ‘undeclinable variant’ of the relative pronoun (viz. {d tåršn} to {d<t> . tåršn}, is arbitrary as the form written {d} is well
attested, particularly in prose (see below, remark to pp. 234-38 [§43]). The case of {d<t>.
 tb‘ln} in RS 15.022 iii 8 (KTU 4.141) is perhaps more plausible because the form {dt} is
attested in the same verbal phrase in lines 6 and 10. But, if the two forms were used in free
variation, there is no particular reason to emend even this one.

--- p. 60 (§21.354.1a), p. 144 (§33.115.42). In the first section cited, T. notes in a remark
that the word ḫswn is extant in RS 11.[913]:2 (KTU 4.60) whereas, in the second, he
proposes “ḥsw<n>” in the same text as an example of final /n/ assimilated to the first
consonant of the following word (on this putative phenomenon, see below, remark to p.
144). The text does in fact read [ḥswfn] (in agreement with T.’s reading indicated on p. 60,
but against the editio princeps¹⁶¹ and KTU/CAT), and does not, therefore, provide an
example of consonant assimilation at a word-boundary.

--- p. 60 (§21.354.1b), p. 662 (§75.532). T. lists {yphn} in RS 3.322+ iii 14 (KTU 1.19) as a
simple scribal error without considering that what is plausibly an orthographic error (one
finds {yphn} in line 29) probably reflects actual pronunciation: [yippanna] for /yiphanna/,
‘he saw’. This is indicated by what has been recently taken as a three-fold spelling of this
verb without the [h] in RS 22.225 (KTU 1.96): tp in line 2 and tpnn in lines 5 and 6.¹⁶² See
further below on tp, remark to p. 199 (§33.323.2b), etc., and on tpnn, remark to p. 579
(§74.511b), etc.

{ydb} for y‘db in RS 24.224:12 (KTU 1.100) might have represent a phonetic change and
devoting a paragraph to that possibility (p. 161), T. concludes at the end of this last
paragraph that, because the form in question is written with the {‘} everywhere else in the
text, its omission in l. 12 is probably owing to scribal error. In favor of this explanation is the
presence of many other scribal errors in RS 24.244. Taking this text and the Kirta text (RS
2.[003]+ [KTU 1.14]), as a laboratory for scribal error (on account of the extensive
repetitions that both texts contain), a number of the ‘grammatical’ phenomena that appear in
this volume could have been confined to the section on scribal errors and thereby eliminated
from consideration in the properly grammatical sections.

such as this the correction without a question mark of {ygbhd} to {y<r>gbhd} in a list of
divine names that can only be qualified as mysterious (RS 24.246;15 [KTU 1.102])¹⁶⁴
constitutes a blatant case of the maximalist approach described above.

--- p. 60 (§21.354.1b), p. 197 (§33.322.42b), p. 273 (§51.46h). In the first paragraph cited,
T. indicates the emendation {t’h<t>yt} in RS 24.247+:32‘ (KTU 1.103); in the others, he
indicates both the emendation and the “alt. Lesung” {thl’yt}. None of my collations has

¹⁶¹Herdner, CTA (1963) 232 (text 143A).
¹⁶³Something has been garbled in this entry, where “… y<r>gb (1.102:15), y<r>gbhd (1.102:15)” cites in
different ways the single entry {ygbhd} contained in this line.
shown any trace that could be taken as a misshapen {t} and there is simply no space for the sign between the {h} and the {y}.165

— p. 60 (§21.354.1b), p. 311 (§54.133.2b). In the first paragraph cited, T. accepts, by implication, the reading of RS 24.248:21 (KTU 1.104) that was indicated in the transcription of the text that I made available to him, for he emends {mbt} to {m<® >bt}, an emendation that is much less plausible if one accepts that there was a word-divider after the {m} (in CAT one finds the reading {wm . bt ilm} vs. my {wmbt•lm}, which was also the reading in KTU). In the second, the reading of CAT is assumed, for he lists this passage as one in which bt, ‘house’, appears in the adverbial accusative. Though the necessity of emending this passage is far from certain, the presence of mḥbt, ‘dwelling/sitting places’, in conjunction with the number ‘four’ and multiples thereof in other passages, inspire at the very least serious thought.166

— p. 60 (§21.354.1b), p. 266 (§51.45e), p. 301 (§53.34), p. 302 (§53.36c). Two attestations of {mrḥqm} (RS 16.402:3 [KTU 2.33] and RS 24.277:27 [KTU 1.127:32]) render dubious the proposal (p. 60) to emend to {mrḥq<tt>m} in the first instance. In addition, there is no reason to analyze the form in the second text cited as a plural (pp. 301, 302), rather than as a singular used adverbially,167 which is indubitably the function of {mrḥqm} in RS 16.402 because it fills the slot of the standard mrḥqt|m in the epistolary prostration formula and corresponds to istu rūqīš in Akkadian.

— pp. 60, 61 (§21.354.1b). Though the reading {‘bmlk} in RS 19.016:15 (KTU 4.609) is superior to the editor’s {‘b[d]mlk}168—because the lacuna after the second sign is too narrow to restore a {d} there—the proposed emendation to {‘b<d>mlk} is unnecessary because the second sign is to be read as {d}, rather than {b}. The first element is not, therefore, ‘bd, ‘servant’, but the reasonably well-attested ‘d, ‘precious thing’.169 The name ‘dršp is attested in line 13 of RS 19.016 while the very name ‘dbʾl is attested in RS 23.037:4 (KTU 4.723); these attestations make T.’s proposal to emend that example to {‘<b>dbʾl} implausible. This and the following example show that, in the absence of a reliable prosopography, emending personal names is always a risky business.

— pp. 60, 61 (§21.354.1b). The name ‘ṭty is now attested a second time (RS 94.2383+:3) and the emendation of the previously known example in RS 17.063:1 (KTU 5.10) to {‘ṭ<tt<r>y} must be qualified as unlikely.

166 Ibid., pp. 566, 567, 572-73, 1279.
167 Ibid., pp. 713, 715, 726.
I agree with many of the readings proposed by T. and J. P. Vita for RS 6.216 (KTU 4.34)\footnote{“Epigraphische Bemerkungen zu ausgewählten ugaritischen Wirtschaftstexten,” UF 29 (1997) 675-81 (not included by T. in the present bibliography).}, but that of {ā w ‘s[x]m} may not be sustained. The reading {āfrî<q> slp’lm} is reasonably clear and, in its context, lends itself to the emendation {āfrî<q> slp’lm} that is here rejected by T.\footnote{See my study of this text “Épigraphie et structure dans les textes administratifs en langue ougaritique: les exemples de RS 6.216 et RS 19.017,” Or n.s. 70 (2001) 235-82.}

Many, including the present reviewer,\footnote{AfO 31 (1984) 216.} do not find the emendation of {‘ly} to {<b>‘ly} in RS 16.402:26 (KTU 2.33) to be self-evident, and T.’s listing should at the least have included a question mark. (See further below, remark to p. 635 [§75.512]).

Neither the emendation of {mtt} to {<t>mtt}, in a letter from the king of Tyre to the king of Ugarit (RS 18.031:13 [KTU 2.38]) that contains no other certain scribal errors,\footnote{See second following remark for another implausible suggestion by T. The first word of (®®b . ±nk}, “I have returned (transitive),’ in line 23 is usually parsed as an infinitive rather than corrected to {®®b<q>}. makes any sense. The text says that some ships had been caught in a squall off Tyre and to have ‘died’ (mtt), that the ‘master of wreck’ (rb tmtt) had rescued their contents, and that the ships, though stripped of their sails (≤ryt), had made it to Acco (see further below, comments to p. 110 [§32.144.12b], etc., to p. 195 [§33.322.2b], etc., and to p. 250 [§51.3f]). That {<t>mtt} does not function as the subject of the verb nškh in line 15 (T. so translates the passage on pp. 535 and 702) is clear for two reasons: (1) the verb is not marked for feminine gender whereas {tmtt} would bear the feminine morpheme; (2) the preceding sentence would say that the Ugaritian ships are in Tyre whereas the last part of the letter says that they are in Acco. Rather, nškh is 3 m.pl. with unnamed subject (cf. the suffix -hm in the following text) referring to the persons who manned the ships (see comment below to p. 534 [§74.32]).

It is truly astounding that the emendation of {d‘ . d‘} to {d‘ . <y>d‘} in RS 19.011:13 (KTU 2.61), another text without clear scribal errors, in which the emended element is taken as a ‘paronomastic infinitive’, should be indicated on p. 61 without a question mark and without any reference to the superficially obvious interpretation as a repeated imperative form, ‘know! know!’ . This is all the more true as the syntagm d‘ yd‘, ‘know indeed!’ , is unattested. The far more convincing interpretation as a reiterated imperative is indicated on p. 635.

The second emendation proposed to RS 18.031 (KTU 2.38) (see previous remark) is not only implausible but based on what is probably a false reading. T. proposes to read {w <k>lklhm} in line 21 when in fact the text reads {w .

\textsuperscript{170}“Epigraphische Bemerkungen zu ausgewählten ugaritischen Wirtschaftstexten,” UF 29 (1997) 675-81 (not included by T. in the present bibliography).\textsuperscript{171}See my study of this text “Épigraphie et structure dans les textes administratifs en langue ougaritique: les exemples de RS 6.216 et RS 19.017,” Or n.s. 70 (2001) 235-82.\textsuperscript{172}AfO 31 (1984) 216.\textsuperscript{173}See second following remark for another implausible suggestion by T. The first word of (®®b . ãnk), ‘I have returned (transitive)’, in line 23 is usually parsed as an infinitive rather than corrected to {®®b<q>}.
— p. 61 (§21.354.1d), p. 601 (§74.626.3a). The emendation of {t<}<b> to {t<}<b> in RS 24.250+23 (KTU 1.106) is far from certain; indeed, in my estimation, it is unnecessary, for {tb} may be analyzed as a G-stem infinitive. 175
— p. 61 (§21.354.1e), p. 395 (§69.133.32a). The emendation of {hm<} to {hm<} in RS 16.268:3, 8 (KTU 4.226), is placed on p. 61 under the heading "Quasi-Haplographie." In each case, however, the preceding and following signs ({ß - ß} and {ß - k}) are different enough from {t} to make a reference even to "quasi"-haplography difficult to accept as a cause for the omission of that sign.
— p. 62 (§21.354.11), p. 161 (§33.144). It is hardly likely that the absence of {h} in {lqt} RS 11.872:17 (KTU 2.13), if indeed the emendation be correct, may be accounted for on a phonetic basis (as T. himself admits in §33.144).
— p. 62 (§21.354.2c). RS 1.003:13 (KTU 1.41) may be explained without the emendation {rßp <dqt>}. 176
— pp. 62-63 (§21.354.2c, d). In section c, T. correctly notes that {ktr} is missing at the end of the line in RS 1.001:14 (KTU 1.39). This is indicated by the parallel deity list RS 24.246 (KTU 1.102), where {ktr} is found between the two deities named here in line 14 and at the beginning of line 15 ({yr<} and {<mrn>}), and confirmed by the empty space that the scribe left at the end of line 14—for reasons not easily explainable today, the scribe knew that a divine name belonged here but apparently did not know which one. Since both criteria apply equally well to line 16 (deity missing here as compared with the list of RS 24.246, long space left blank by the scribe), it is uncertain why T. did not include this case among his examples of missing text. Because it is likely that both the divine name and the term designating the sacrifice were omitted from this line (see CAT and contrast with KTU177), this example could have been included under section d of §21.354.2 "Fehlen mehrerer Wörter." The comparison of these two texts with the partial duplicate texts RS 1.003/RS 18.056 (KTU 1.41/1.87) also shows that T.'s emendation of RS 1.003:13, wherein he inserts {dqt} before {<srp} (following CAT), is unnecessary: if one be willing to admit that the order of mention of the deity and the sacrifice were inverted in RS 1.003/RS 18.056, a common occurrence in the ritual texts, {dqt} may be reconstructed as occurring at the end of RS 1.003:12. 178

176See ibid., pp. 146, 174, and the following remark here.
177More detail regarding the reconstruction at the end of line 16 and the reading of {<s>r} at the beginning of line 17 are provided in ibid.
178Indeed, a thought that had not occurred to me when writing Les textes rituels, RS 1.001 and RS 1.003/RS 18.056 could be harmonized by filling the blank space in RS 1.001:16 with {dqt <srp} instead of with {<srp
— p. 63 (§21.354.2d). It appears a bit simplistic to say that zbl b‘l árš is missing “am Textende” in RS 24.245:19 (KTU 1.101:18) when the corresponding text from the Baal Cycle is much longer and the end of this tablet was left uninscribed. It is true that in some sense the three words indicated are missing from the end of the text, but because we have no way of knowing what the end of the poetic composition may have been or what caused the scribe to break off the writing so abruptly,¹⁷⁹ that observation hardly belongs in a list of scribal errors.

— p. 63 (§21.355.1a), p. 243 (§45.121).¹⁸⁰ The first {h} of the form {hmhkm} in RS 29.095:14 (KTU 2.71) may not be considered a certain scribal error; indeed, it appears more likely that this is an alternate form of the indefinite pronoun mhkm (see below, remark to p. 229 [§42.0], etc.). The word may also be functioning as an adjective in this phrase ( {dbr hmhkm} ) as it follows the noun it modifies (T. takes it as “substantivisch gebraucht,” but his translation is not terribly convincing: “die Angelegenheit von irgendetwas”).

— p. 63 (§21.355.1a), p. 778 (§82.421). Nor may {lpßpß} in RS 17.117:4 (KTU 5.11) be considered a certain error (T. holds that the first {p} should be elided). In a text of which the diction is as uncertain as in this one, it is necessary to leave open the possibility that the signs are to be divided l p špš and interpreted as ‘according to the mouth (i.e., pronouncements/orders) of the Sun (i.e., the Hittite king),¹⁸¹ There is no cross-reference on p. 63 to the mention on p. 778 of this interpretation of the signs as a possible one.

— p. 63 (§21.355.1a), p. 476 (§73.426), p. 518 (§74.224.3). {ḥṭú} in RS 2.[009]+ ii 23 (KTU 1.6) is not a “n[eeu] L[esung]” but a rejection of the proposal in CAT to emend to “<ŋ>ḥṭu.”

— p. 64 (§21.355.1b), p. 179 (§33.231.22b), p. 185 (§33.243.15), p. 284 (§52.5a), p. 297 (§53.322.4). On p. 64, the possibility is broached that {mrddt} is a scribal error for {mrdt} in RS 34.124:28 (KTU 2.72), rather than an instance of the feminine plural morpheme having been added to the singular stem plus the feminine singular morpheme (/mardatu/ > /mardåtu/) instead of the expected /mardåtu/). The same two options must be left open in the case of tått in RS 24.247+:1 (CAT 1.103+). Though T. is certainly correct in adopting this reading and in interpreting the word as meaning ‘ewes’ (as opposed to the various readings proposed by the authors of KTU/CAT¹⁸²), he should also have mentioned tått on p. 64 as a

dqt}, i.e., with a text reflecting the same inversion of the order of mention of the deity and the sacrifice as one attains by reconstruction in RS 1.003/RS 18.056.

¹⁷⁹Pardee, Les textes para-mythologiques (1988) 119, 151-52. Since this publication, it has become clearer to me that, with very few exceptions, tablets from Ugarit and most other sites have been preserved because they were baked in a fire that destroyed a house, a section of a town, or an entire city. This tablet may, therefore, have been unfinished by the scribe simply because that was what he was doing when the time came to abandon the house and the city of Ugarit.

¹⁸⁰In this second citation, the form appears to be incorrectly transcribed, viz., as “/h/ḥkm” (the sigla for extra signs are normally «X» while { } are used for graphemes and { } for morphemes).


¹⁸²See overview of the various readings proposed by Dietrich, Loretz (and Sanmartín) in Pardee, Les textes rituels (2000) 536-37, 553.
possible case of dittography instead of treating it uniquely as the 'correct' plural of ṭāt in Ugaritic. Since this word does not bear the double feminine morpheme in the plural in the other languages where it appears, it is necessary to await another attestation of the form in Ugaritic before being sure of that analysis. Furthermore, by his own etymological derivation of ṭāt on pp. 185 and 297 (singular "*ṭa’awat-," plural *ṭa’watāt) and by his analysis of {ṭatt} as showing a double feminine morpheme (p. 297), this word should not have been included among "Grammatische Feminina ohne Feminininendung" (p. 284).

— p. 64 (§21.355.1b), p. 830 (§89.24e). On p. 64, two options are presented for dealing with šlmm in RS 16.268:10 (KTU 4.226): either emend to {šlm} or else take the final {m} as enclitic. On p. 830, the first option is not even mentioned while the second is formally rejected in favor of the analysis of šlmm as a m.du. G-passive participle.

— p. 64 (§21.355.1c). The emendation {trḥt«t»} indicated for RS 24.255:19 (KTU 1.111:20) is taken from KTU/CAT (albeit with a question mark) but is linked directly with the misreading of a word-divider at the end of this line. Since the head of the word-divider is at the bottom if read with the verso, there can be no doubt that it belongs to the end of line 6, which extended around the edge of the tablet onto the verso. With that word-divider properly accounted for and considering that the last sign of line 19 was written up against this word-divider from line 6, one must conclude that this word is incomplete, that it is completed by the first sign of line 20, a {š}. These purely epigraphic considerations lead to the conclusion that the words in question are to be read {trḥ tāš}, ‘you/she are/is to demand the bride-price’. (T. implicitly admits this on p. 568 [§74.422] and p. 616 [§75.217].)

— p. 64 (§21.355.1d). As the editor saw, there is no extraneous {b} in the word kd in line 12 of RS 24.256 (KTU 1.112); T. has here accepted uncritically a false reading in KTU that has been perpetuated in CAT.

— p. 64 (§21.355.2b), cf. p. 60 (§21.354.1a). The emendations {tn «km» nhšm y ḫr<n>}, indicated without a question mark for RS 24.244:73 (KTU 1.100), cannot be considered relatively certain when other emendations, equally or more plausible, have been proposed. For this emendation to win the day, T. would have to propose a stichometric analysis of the passage that would be superior to those proposed to date.

183In UF 31 (1999) 742, T. presented both forms as certain examples of the double feminine morpheme.
186No satisfactory explanation of this verse has yet been proposed that does not resort to emendation. An example which incorporates the elision of {km} and the addition of {n} to provide the divine name Ḥōrānu but which requires yet another in the next line, for a total of three instead of the single emendation in the text that I adopted in Les textes para-mythologiques, is visible in S. B. Parker apud Parker, ed., Ugaritic Narrative Poetry (SBL Writings from the Ancient World 9; Atlanta: Scholars Press, 1997) 222-23. As the resulting verse is not preferable on formal structural grounds (though it is a perfectly good 'staircase'), the high rate of emendation makes it questionable at best. For the arguments in favor of the single emendation, viz., the insertion of <mhř(y)> in line 73, see Pardee, JANES 10 (1978) 100-101 (the proposal goes back to M.C. Astour, “Two Ugaritic Serpent Charms,” JNES 27 [1968] 13-36, esp. p. 26).
— p. 64 (§21.355.2b). The ritual texts are so uncertain of interpretation that the identification of {ṣ}, i.e., ṣ, 'a ram', in RS 24.249:11' (KTU 1.105) as extraneous may not be accepted as certain.¹⁸⁷

— p. 64 (§21.355.2c). T. considers that the repetition of the same colon in RS 2.[003]+ ii 20-21 (KTU 1.14) was erroneous on the part of the scribe, an analysis that would be accepted by many scholars. But then he goes on to argue that it is the first colon that is to be deleted, because the second is preceded by ṻ. This may make some kind of sense for the modern scholar working with a computer, but I do not understand what it means in scribal terms. Does he mean that the scribe mistakenly omitted the ṻ when he first wrote the line, then, instead of erasing that line and re-inscribing it, simply wrote it again, this time with the ṻ? If so, such a procedure surely does not represent normal scribal procedure: if the omission of the {w} was noted before the next line was inscribed (as would be the case in the eventuality under consideration), the scribe would normally have erased the line and re-inscribed it with the {w} correctly in its place.

— p. 65 (§21.361). In the first paragraph of this section, T. states that an erased area may either be filled in with new signs or left uninscribed, but he makes no attempt to explain why one procedure or the other would have been followed. In general, the principles are these: if the scribe noticed his error immediately, the error was erased and the correction made in its place. But in cases where the error came to the attention of the scribe only after one or more lines were completed, two general possibilities exist: if the error consisted of something that was correctable (e.g., metathesis of signs) then the correction could be placed over the erasure, but if the error consists of extra signs, then the only possibility was to leave the erased area blank. Both possibilities may be illustrated by examples from RS 2.[004] i (KTU 1.17). (1) In lines 8' and 15', the scribe 'Ilîmilku left out the last sign of a word (the {ṣ} of qdṣ and the {h} of mîzrth, respectively) and inserted it later either after erasing the word-divider separating that word from the following or by simple squashing the word-divider with the new sign (in the case of the {h}, one clearly sees traces of the word-divider, but it is impossible to say whether it was imperfectly erased or just replaced by the {h}). In both cases, the correction could be and was inserted in the space available. (2) An example of an erasure left uninscribed in line 11' is cited by T. though without any details. This erased area was not reinscribed because the scribe mistakenly wrote šbʾ, 'seventh', immediately after tdlʾ, 'sixth', finished at least this line before noticing his error, then erased the erroneous word.¹⁸⁸ Because that word was extra, there was nothing to put in its place and the space was for that reason left uninscribed.

— p. 65 (§21.361a). What the authors of KTU/CAT took as an erased {h} at the end of RS 2.[004] v 30' (KTU 1.17) is in fact just an indentation in the broken edge of the tablet: it is

¹⁸⁷For an argument in favor of accepting the text as it stands, see Pardee, Les textes rituels (2000) 583.

¹⁸⁸The editor correctly read the imperfectly erased signs but included them as part of the final text (Virolleaud, Danel [1936] 186, 188). The authors of KTU/CAT thought they saw here the word ym, a reading once criticized by T. (AF 42-32 [1995-96] 270). In fact, traces remain of the signs {šbʾ}, but the scribe's intent was clearly to erase them.
too far from the last letter of the line and located too low on the plane of writing to be taken seriously as a genuine remnant of an erased sign.

— p. 65 (§21.361a). What the authors of CAT identify as an erased {k} at the end of line 31 of RS 18.038 (KTU 2.39)\(^\text{189}\) has more wedges than a {k} and can only with great difficulty be attached to /yiklû/, because (a) the wedges in question are aligned with the horizontal line separating lines 30 and 31, not with line 31, (b) the end of line 16 extends just below the end of this dividing line, leaving insufficient room for the scribe ever to have inscribed the end of line 31 here, and (c) there are no traces of any other erased signs between the last sign of line 31 and the erased sign in this narrow space. The simple fact of the alignment of these traces with the horizontal dividing line leads to the hypothesis that they represent the last sign of an entire line of writing that the scribe would have erased because he had forgot to place the dividing line. These traces would not have been fully erased because the sign was on the edge of the tablet.

— p. 65 (§21.361a). The remaining traces of the erased sign to the right of the last {l} in RS 34.124:28 (KTU 2.72) do not correspond to the form of {m}, i.e., it never had a vertical wedge to the right. Rather, the {l} itself appears to have been written over two erased signs, the first of which was {≤} while I have been unable to identify the second. This is not, therefore, a case of the scribe writing the first sign of a word at the end of a line, realizing that the space is insufficient for the entire word, erasing the sign, and starting the word over on the next line (line 29 here begins with {mlk}). That scenario is highly unlikely in this case, in any case, for the {l} is already on the right edge of the tablet.

— p. 65 (§21.361b). It must be judged uncertain whether the erased sign in RS 29.095:2 (KTU 2.71) was {m}. It is true that, of the erased sign, the head of a horizontal wedge remains on the left side of the depression caused by the erasure. But identifying the erased sign as {m} on that basis begs the question of why the scribe would have made a large erasure mark to erase the vertical of {m}, left the head of the horizontal wedge, then impressed the {g}, made up of a single vertical wedge, over the erased vertical of the putative {m}. Nevertheless, however irrational such a sequence of events may appear, it may not be ruled out by the remaining traces.

— p. 65 (§21.361c). I found no erased {l} in RS 24.250+:27 (KTU 1.106), only a partially damaged word-divider. Given that this is a new reading in CAT (i.e., absent from KTU), one must look upon it with great suspicion. What might have led to the new reading is a bit of breakage on the left upper edge of the following {t}, which might on a photograph be mistaken for remains of the heads of vertical wedges.

— p. 66 (§21.362). The assertion that the third sign of the first word in RS 2.[003]+ iv 14 (KTU 1.14) is {i} placed over an original {ā} is simply incorrect. Though the sign has suffered damage and though earlier editors, thinking that the scribe should have written the

\(^{189}\)Again T. has accepted uncritically a reading from CAT. The facts that the erased sign was assigned to line 30 in KTU and that the new line assignment in CAT was not based on collation should have incited him to caution in this case.
same form as in ii 33, actually read {ṣbî} here, the authors of KTU/CAT were correct in reading {ṣbâ}.\footnote{190T.’s proposal here is a modification of an earlier assertion, viz., that the reading should be simply {ṣbî} (A\textit{uO}r 13 [1995] 233).}

— p. 66 (§21.362). As has been observed above (remark to p. 55 [§21.342.3], etc.), the notation {ṣr mầ} for the word šrnn in RS 2.[003]+ iv 50 (KTU 1.14) is incorrect even by T.’s standards. However that may be, the reading {ṣrn} is certain and no exclamation remarks are required. Collation shows that the {ṣ} was written over a poorly erased sign that can no longer be identified. Here T. claims that the scribe had originally written {ṣ’t} at the beginning of the line, but the traces left after erasure are scanty and do not conform to this proposal. (The authors of KTU thought that the scribe had mistakenly written the entire word s’t here, but in CAT only {ṣ} is said to have been present. What one finds on the tablet is a trace of a vertical wedge to the right of the {ṣ} and under the {r} two horizontals in the form of the lower wedges of {b}. Though the former could belong to {ṣ}, the latter have nothing to do with {ṣ’t}.) The final sign was always a three-wedged {n}—it was the first {n} that was originally written with only two wedges, but the this error was corrected by the addition of a wedge to this sign at some time after the final sign had been inscribed.

— p. 66 (§21.362). The assertion that the {r} of the word škrn in RS 2.[004] i 30’ (KTU 1.17) was written over a {n} may not be accepted. If such were the case, the scribe would have taken care to write a more typical {r}. In fact, the sign is an uncharacteristically narrow {r} of which the center is damaged.

— p. 66 (§21.362), cf. p. 70 (§21.412l). It is not quite correct to say that the {n} of {l˘m˘n} in RS 24.247+:6 (KTU 1.103) is written over a vertical wedge. More precisely, the head of the {n} touches the right side of the tail of a vertical wedge of which the head is lost in the lacuna above {l˘m˘n}. Because we are dealing with only the tail of the wedge and because the scribe placed the {l} and the {n} on either side of this tail, this case does not belong to the category treated in §21.362, viz., individual signs that have been overwritten for the purpose of correcting a mistaken writing.\footnote{191For the problems posed by this line segment, see Pardee, \textit{AfO} 33 (1986) 122-23.}


— p. 66 (§21.362). My collation did not reveal that the {â} of {âl˘dn} in RS 17.117:8 (KTU 5.11) was inscribed over two vertical wedges (for T. the first two verticals of a {d} mistakenly inscribed in anticipation of the following sign), but only that it has “suffered slight surface damage.”\footnote{192\textit{AfO} Beiheft 19 (1982) 48.}

— p. 67 (§21.38). Though T. avoids using proper names to derive grammatical categories, in the matter of phonetic versus phonemic orthographies, examples such as {ṣdkn} and {ṣtq˘n} for {ṣdqn} or of {ṣtq˘slm} and {ṣtq˘slm} for {ṣdq˘slm} could have been cited as illustrations.\footnote{193See C. H. Gordon, \textit{Ugaritic Textbook} (Rome: Pontifical Biblical Institute, 1965) 472 (§19.2147).} These and similar forms are mentioned below, p. 98 (§32.134), p. 140
(§33.112.52), and elsewhere in the section on phonology, as examples of dissimilation or assimilation of various features of articulation; their contribution here would have been to illustrate phonetic writings.

— p. 68 (§21.412c), p. 226 (§41.222.5b). Because of the broken contexts, *hn* in {铿-铿 . hn} (RS 16.402:9 [KTU 2.33]) may be the presentative particle rather than a pronominal suffix and *hm* in {any . hm} (RS 18.113A+B:25 [KTU 2.42:24]) may be the conditional conjunction. In the case of RS 18.140:20, where *KTU/CAT* 2.45:21 indicate {ytn . hm}, I found no word-divider on the tablet.

— p. 68 (§21.412c), p. 210 (§41.112.7), p. 211 (§41.12), p. 226 (§41.222.5a), p. 231 (§42.4), p. 571 (§74.432). On pp. 68 and 226 {b . hn} in RS 18.113A+B:35 (KTU/CAT 2.42:23) is translated “bei ihnen”; on p. 571, the same reading is translated “hinsichtlich dieser Dinge(?);” on pp. 210, 211, and 231, it is proposed that the line should be restored as {b . hn .[hmt]} and translated “über die be[treffenden (Leute) …]” (the complete restoration and translation are to be found only on p. 231, where *hnmt* is presented as a demonstrative pronoun). In my preliminary edition of this text, I restored the last visible trace in this line as a word-divider, and subsequent collation confirms that this is the probable reading. One or the other of the explanations proposed by T. appears, therefore, to be plausible; the first, however, is to be preferred because of the probable word-divider after {hn}.

— p. 69 (§21.412e). The word-dividers claimed to exist between the noun/preposition and its suffix in *bnh* and *ly* in RS 24.263:3', 4' (KTU 1.117:4, 5) were both erased on the tablet and it is therefore improper to cite them as the (only!) examples of this particular orthographic practice.

— p. 69 (§21.412f). There is no word-divider at the end of the line in RS 15.098:13 (KTU 2.17:6): rather than the extant word-divider and two erased signs indicated in *KTU/CAT*, one finds on the tablet only traces of a word-divider and {‘}, both erased. (This provides, therefore, another example of the phenomenon treated above, p. 66 [§25.361a], viz., a word is begun, erased, and re-inscribed in full on the following line.) In RS 16.078+:24 (KTU 2.23), there is also no word-divider at the end of the line: the last sign, {y}, is only partially preserved and the lacuna, wide enough for one or two signs, is deep and followed to the right by an uninscribed space; {b‘ly‘l} is, therefore, in all likelihood the last word of the line and any word-divider that might have followed the {ly‘l} has completely disappeared in the lacuna. As concerns word-dividers at the end of lines in RS 16.394 (KTU 2.31), my collation agrees with that of *KTU/CAT* in finding only two, not the four claimed by T. I found no word-divider at the end of RS 11.795:16 (KTU 4.91): what Herdner and the

---


195 Ibid.


197 On the interpretation of lines 13-14 of this text, see below, remark to p. 312 (§54.133.2c).

198 CTA (1963) 231.
authors of KTU/CAT identified as such is only surface damage (in agreement with the editio princeps)

— p. 69 (§21.412f). Word-dividers placed at the end of lines in multi-columned tablets constitute a special case, for some of them have a special function, that of marking the division between a word that runs across the column dividers and the line of writing proper to the column on which the first word has impinged. This phenomenon is well illustrated by RS 2.[004] i 2', 12' (KTU 1.17). T. claims in this section that {ylhm}, the last word in each line and which in each case impinges on the second column, is each time followed by a word-divider. This is correct for line 2' but not for line 12'. The reason for the difference appears clear. In the first case, a line of writing on col. ii was inscribed at the same level as line 2' in col. i and the word-divider was useful to indicate the dividing line between the words belonging to different parts of the text. But in the case of line 12', the line in question on col. ii (l. 13') was not at the same height as i 12' and there was, therefore, no need for the word-divider. (The same is true, by the way, of col. i, lines 14' and 16': in both cases the last word of the line impinges on col. II and the word-divider was used in neither case because each of these ends of lines faces a space between lines in col. ii.

This explanation of the difference between the two passages seems to lead to the additional conclusion that the word-divider in such cases was not actually inscribed until its necessity became apparent in the course of the writing of the second column.

— p. 69 (§21.412g). There is no word-divider between the elements of the proper name 'bdšhr in RS 11.844:19' (KTU 4.98).

— p. 69 (§21.412g). The use of the word-divider in {mlk . ≤®trt} in RS 86.2235:17 (RSO XIV 39) does not belong to the same category as the other examples cited in this paragraph, viz., the appearance of the word-divider between the elements of compound personal names, because the phrase in question is not yet at Ugarit a compound divine name as it came to be in the first millennium. In the Ugaritic phrase, the divine name mlk is in construct with his seat of reign, the town of 'ttrt. Other texts leave no doubt on this score: in RS 24.251:42' (KTU 1.107) the deity is for literary reasons described as mlk b 'ttrt and in RS 24.252:1-2 (KTU 1.108) mlk, designated by the title ṯπū mlk 'lm, is described as il yḥb b 'ttrt, il ṭpt b hdr'y, the god who dwells in 'ttrt, the god who rules in hdr'y.

— p. 69 (§21.412g). In a remark at the end of this paragraph, T. asserts that the authors of CAT have incorrectly ("fälschlicherweise") classified the use of the word-divider between the elements of a compound personal name as mistakes ("Fehler"). It might be more useful


200In AuOr 16 (1998) 292, T. claimed that the last word of line 16' is followed by a word-divider, but this is not the case.

to describe the phenomenon statistically: T. has come up with only twenty examples, which must be placed against hundreds, if not thousands of examples, of the non-use of the word-divider in names of this type. On the other hand, it appears certain that this use of the word-divider reveals that the scribes understood the compound nature of the names in question. Viewed in this way, it seems likely that {dn·il} in RS 2.[004] ii 24' (KTU 1.17), the only case of this writing of 'Aqhatu's father's name with the word-divider, must at the very least be classified as an inadvertance on the part of the scribe, 'Ilīmilku, not as a purposeful showing off of his erudition, for his use of the word-divider is fairly regular.

— p. 69 (§21.412h). The argument that has just been made for {dn·il} may also be applied to {āp·hn} in col. ii, line 28', of the same text: because this compound particle is elsewhere in this text written without the word-divider, the example in line 28' must be classed as an irregularity on the part of 'Ilīmilku, probably not an intentional display of etymological erudition on his part, though not strictly speaking an 'error'.

— p. 69 (§21.412h). One may doubt both that there is a word-divider after the second sign of {'r'r} in RS 24.244:64 (KTU 1.100) and that, if it were there, it would mark the "lexematischen Bildungselemente" (§21.412g).

— p. 69 (§21.412j). Even the user of KTU/CAT will be astounded by the assertion that RS 24.246 (KTU 1.102) was inscribed without any word-dividers when there is one in line 3. In fact, as I have attempted to show, the one word-divider present is the only one that is to be expected to have been used in the text and it is highly meaningful.


— p. 70 (§21.412l). It is unclear why T. does not use the notation «.» in this section dealing with improperly placed word-dividers. For example, why not ‘«.»tttr instead of ‘‘.tttr” in RS 24.244:20 (KTU 1.100) or ‘q«.»šr instead of “q.šr” (same text, line 39)? In all examples, the vertical wedge, if correctly read, is within a word and may only be described as erroneously inscribed—such is certainly the case of the two examples cited (see my study cited in the previous remark).

— p. 70 (§21.412l). My collation of RS 2.[014]+ (KTU 1.3) did not confirm the presence of a word-divider after the first sign of the word klbì in col. iii, line 45'. The wedge in question is larger than the usual word-divider in this text and it appears more likely that we are dealing with a four-wedged {l}: there are several four-wedged {l}s in lines 44'-46' and the distribution of the various sized wedges varies considerably from one example to another.

— p. 70 (§21.412l). RS 78/04:12' (CAT 1.173) does not provide an unambiguous case of a misplaced word-divider: the scribe inscribed {š}, then a word-divider, then, realizing that the word should have been šm, ‘(two) rams’, he inscribed the {m} under the word-divider.

204 See the new photograph of these lines in Bordreuil and Pardee, Manuel (2004), text 2 in the Choix de textes.
This, therefore, is an example of a word-divider placed at the end of a line (§21.412f) and of a secondarily corrected text, but not of a word-divider placed within a word.

— p. 70 (§21.412). It is unclear why T. includes {‘.rt} as an example of a misplaced word-divider in RS 10.035:4 (KTU 4.46) but not {prt.wn} in the same line when he has himself identified the latter as an incorrectly placed word-divider.205

— p. 70 (§21.412m). Unless a case can be made for a proper use of double word-dividers, the second of two word-dividers placed side by side (§21.412m) should also have been indicated as «.» . T. proposes that the usage may have been intended in only one of the texts cited here: in RS 2.[008]+ v 42 (KTU 1.4) the two word-dividers may be the equivalent of a colon in modern punctuation.

— pp. 73-75 (§22.1-22.33). That the vertical stance of the lower wedges of {b,d} cannot be used alone as a criterion for deciding whether a text was written in the short alphabet, or was even under the “Einflüsse” of that tradition, is shown by RS 16.265 (KTU 5.9), for there the portions of alphabets written on the edges of the tablet show precisely that form of {b} and {d} whereas the rest of the text is indubitably written in the long alphabet.

— p. 78 (§22.81). T. proposes that in RS 5.197+ (KTU 4.31) the word kd denoting a liquid measure has been emitted before words for oil, but the translation contains “(Schekel).” Unfortunately, the state of the text is such that it is not certain whether the formulae “b + number” indicates a price in shekels or whether it is a quantity of oil that is introduced by b, nor, in the latter case, whether the measure is the kd or not. Even in regular Ugaritic (this is a text in the short alphabet), other measures are used for oil.

— p. 85 (§23.523), p. 186 (§33.25.1), p. 196 (§33.322.41a), p. 274 (§51.46k). Because there is no obvious reason why a field would be named “Ende, Schicksal,” one may doubt that {A.ŚÂ.MEŠ AH-ra-a-PI} in RS 15.085:18 (PRU III, p. 52) should be analyzed as providing the only example in syllabic writing of the /-y/ ending that is presented on p. 274 as indistinguishable from the nisbe ending (on which see below, remarks to p. 197 [§33.322.42c] and to pp. 273-74 [§51.46h-k]). It goes without saying that the ending /-y/-y/ here fits the category of geographical names bearing that ending (see remark to pp. 273-74 [§51.46h-k]) but that fact says nothing about the etymology or the “meaning” of the toponym.

— p. 88 (§31.2). It is remarkable that T. takes the syllabic orthography of the proper name Munahhimu which does not mark the gemination of the /h/ as an indication that this and other “gutturale Phoneme” may not in Ugaritic pronunciation have actually been geminated when (1) the syllabic orthography does not consistently represent gemination (nor non-gemination for that matter, i.e., one encounters cases of {VC-CV} sequences where the consonant was not in fact doubled) and (2) /h/ is one of the ‘weak’ consonants that maintained the possibility of gemination the longest in Hebrew, as is witnessed by so-called ‘virtual doubling’ in Hebrew grammar.

— p. 88 (§31.3), p. 143 (§33.115.41), p. 250 (§51.3h), p. 279 (§52.212). For the vocalization of ‘lady’, T. considers only the possibility of a derivation from ‘adn, i.e., /’adânt/- /’adatt-/, without mentioning the possibility that it may simply be the feminine

form of /'adu/, ‘father’. The problem—one not discussed by T.—is that ādn means only ‘father’ in prose\textsuperscript{206} and frequently shows that meaning in poetry as well, while ādt always means ‘lady’ in prose, indeed is often used in letters as a second title for a person also called ūm, ‘mother’. So did ādt develop directly from /'adu/ and secondarily take on the value of ‘lady’, or is it the feminine form of /'adānu/ and has it preserved what is plausibly the original meaning of that word, viz., ‘lord’ → ‘lady’? RH 78/25:11’ (CAT 2.83:5) furnishes the writing {ādnty}, but that, if not a mistake, may only represent a form of the same word with the long form of the feminine morpheme, viz., /'adānatu/. Unfortunately the condition of that text is too poor to allow a decision as to whether the word means ‘mother’ or ‘lady/ladies’.\textsuperscript{207} One may also debate whether {ād} ‘father’ and hence, plausibly, {ādn}\textsuperscript{208} had in the second millennium a doubled /d/\textsuperscript{209} Since the /d/ is not doubled in later dialects, I opt for that solution, but the data are not unequivocal.

— p. 89 (§31.42). On the basis of data from the other languages, Arabic in particular, the rule for accent in a word containing no long vowels is not that the accent is on the first syllable, but that it is on the third syllable from the end of the word unless a long vowel intervenes, in which case the syllable containing that vowel takes the accent.

— p. 93 (§32.123.22). As regards the etymology of {qβz} in RS 24.293:13 (KTU 1.133), I have pointed out the similarities, not mentioned here by T., between this passage and RS 2.022 ii 24-25 (KTU 1.5), where one finds {qβb}.\textsuperscript{210} The identification with Arabic QBĐ is thus uncertain.

— p. 94 (§32.123.31), p. 256 (§51.41e). In the first section cited, Ugaritic ģr, ‘mountain’, is compared with Semitic /zβrr-/, in the second, it is identified as a /qutl/-base noun from a hollow root, said to be vocalized /ũr/- ← /ũwr-/ ← /ũuwr-/.

\textsuperscript{206}Even in RS 18.038:5-6 (KTU 2.39)! On this new interpretation (as compared with Pardee, UF 13 [1981] 152), see Context III (2002) 95 n. 52, 103 n. 119, 114 n. 216. In the polyglot vocabulary entry from which we learn the vocalization of the word at Ugarit, [a-da-nu] has for equivalents words that mean ‘father’ (RS 20.149 ii 9 [Ugaritica V, Akkadian text 130]). T. glosses ādn only by “Herr” (p. 250, cf. p. 278 [§52.1]). /'adānu/ may consist of the noun /'adu/, ‘father’ (see following discussion and notes) plus the derivational suffix /-ān/- which would early on have had the meaning ‘lord’. If such was the development, the meaning of ‘father’ for /'adānu/ in Ugaritic would have been secondary. For such a development, compare the colloquial use of ‘governor’, abbreviated ‘gov’ or ‘guv’, for ‘father’ in the popular register of British English.

\textsuperscript{207}T. assumes that the form is singular (see also UF 31 [1999] 741) without taking into consideration the state of the text.

\textsuperscript{208}T.’s only remark on etymology is that ādn may be derived from a root ‘DN, rather than from a root ‘D + -ān ending (p. 250); no comparative evidence in favor of that formal possibility is adduced.


As T. recognizes implicitly on p. 560, there is no reason to take $y\div ly$ in RS 3.322 i 31 (*KTU* 1.19) as meaning 'to burn', i.e., as derived from a root with initial /s/. The meaning 'be low, droop' (etymological /GLY/) fits this passage well, as recent interpreters have seen\(^{211}\). The case of RS 2.[009]+ vi 17 (*KTU* 1.6) is complicated by the fact that the reading there is uncertain, but it makes thereby all the less likely a case for this irregular phonetic correspondence.

There is no particularly good reason to take QT˚(T˚) in RS 1.002:[14'-15'], 22'-23', 30', 39'-40' (*KTU* 1.40); RS 17.100:[28'], 38', [49'], 59' (*KTU* 1.84) as meaning 'to lie' and to derive it from $\sqrt{QT}$T. There are perfectly good etymological pegs with /h/ on which to hang the Ugaritic word; these indicate that it has to do with committing a moral turpitude.\(^{212}\)

M. Heltzer's (inherently dubious) proposal that the hrš q†n were 'makers of sickle-swords' (Akk. *k/qat(t)innu*),\(^{213}\) cited here by T. with a question mark, is shown to be inadmissible by the equivalence \{LÚ.MEŠ.NAGAR.SIG\} = \{hrš q†n\} in RS 94.2519:6, 19, where the use of \{SIG\} leaves no doubt that Ugaritic q†n is derived from the root QT˚N 'to be small'.\(^{214}\)

One may for two principal reasons consider that the original form of the place name \{tbq\}/[t bq] was the former, rather than the latter:\(^{215}\) (1) the phenomenon of assimilation of phonetic emphasis is more broadly attested at Ugarit than is dissimilation of same, and (2) no attempt was made by the scribes of Ugarit to register /h/ when writing the name in syllabic script. T. opts for /t bq/ on the basis of its being more commonly attested in alphabetic script.

Again entire paragraphs (pp. 98, 158-59) are devoted to a dubious grammatical category, in this case the assimilation of the glottal stop '{ }' to the preceding consonant: \{qš\} in RS 24.258:2 (*KTU* 1.114) need not be derived from a root QĐ', nor even from QŠY, the alternative suggestion; just as plausible as the latter and more plausible than the former is a derivation from the geminate root QŠŠ, which is a by-form of QSY.\(^{216}\)

Here dbb is cited without textual reference as meaning "Fliege" and as being derived from a root dBB. I know of no text, however, where that meaning is


certain (some give that meaning to {dbb}, not {dab}, in RS 2.[014]+ iii 46 [KTU 1.3]) whereas ddb in the meaning of ‘(sooth-)sayer’ is now well attested: on p. 676 (§75.65), T. correctly cites RIH 78/20:1, 9 (CAT 1.169) and RS 92.2014:9, 13 (RSO XIV 52) as having the meaning “Beschwörer.”217 On ddb as a proper noun, see below, remark to p. 278 (§52.12b).

— p. 101 (§32.142.32), p. 668 (§75.537d), p. 735 (§77.51b). I fail to see why in the first section cited, devoted to Ugaritic {d} ← /d/, T. cites Arabic HDW (!) as etymology for yḥdy in RS 24.277:27‘ (KTU 1.127:32). However that may be, this etymology really does not help to explain the Ugaritic text: the Arabic verb is used for driving camels on a march, whereas in RS 24.277 yḥdy is thought to mean ‘drive away (a goat into the wilderness like Azael in Lev. 16)’. The passage is better explained by the well-attested Ugaritic verb HDY, ‘to see’.218

— p. 106 (§32.143.41). bz in RS 15.072:4 (KTU 1.80) cannot mean “Euter, Mutterbrust,” for one does not ‘slaughter a ram from the udder’. bz is more plausibly derived from the root BZZ, ‘to take booty’ (the text reads {b bz ≤zm 219 ʃh˘}, ‘When the goats were taken as booty did he slaughter his ram’).

— p. 106 (§32.143.43), p. 288 (§53.121b), p. 356 (§62.432), p. 392 (§69.132), p. 757 (§81.11). It cannot be judged likely that Ugaritic ḫzr means “Hilfsarbeiter” nor that it is a loan-word from Akkadian ƙaẕiru (p. 106), and the translation “kHzr-Leute” (p. 288) is, therefore more prudent. The reasons for this negative assessment are: (1) judging from the distribution of usage in Akkadian, the word there probably represents West-Semitic ĀDR and the Ugaritic word would therefore have to be seen as a back-borrowing of a word that should have remained in the language in its native form; (2) /h/ in Akkadian loan-words normally is represented in Ugaritic by {g}; (3) ḫzr in RS 19.016:49, 50 (KTU 4.609) is used to describe all the personnel who have served during the month of ihtbnm, from the highest to the lowest, both those who have served in the palace and those who have furnished the rations for the latter,220 and ‘help-personnel’ simply does not describe so broad a grouping. I have no etymology to suggest, but, in this text at least, ḫzr cannot be more precisely defined than as a classifying term for all who were furnishing a service to the palace. This being the case, one may doubt that the b in the phrase b ḫzr in RS 19.062:2 (KTU 4.630) is partitive in function (p. 757), especially if the same formula is correctly reconstructed in RS 19.016:49, 50. The difference between the two texts is that in RS 19.016 the service categories are


218 For the argumentation, see Pardee, Les textes rituels (2000) 725-26. For the root “ḥdy/w,” ‘to see’, in Ugaritic, see T., §32.142.33.

219 Read {ṭbḥ} (see T., p. 57 [§21.352.2]).

220 For this interpretation of RS 19.016, see Pardee, Semitica 49 (1999) 30-58, on ḫzr in particular pp. 44-45, 57.
named whereas in RS 19.062 those who are b ḥzr are not further defined and are, by the
structure of the text, contrasted with four well-known professional terms (‘plowmen’, ‘house
builders’, ‘makers of small objects’, and ‘arrowhead makers’). If we allow RS 19.016 to
dictate the meaning of b ḥzr here, that term would designate all who were in immediate
service to the palace, whatever their function, whereas the other terms would designate
professionals not in immediate service but entitled to a ration of wool (in return for service
performed in previous months?).

— p. 106 (§32.143.43). Nor can it be judged likely that Ugaritic kzy is a “Fremdwort”
directly related to Akkadian kizû. Whatever the origin of the word, the consistent writing
with {y} in Ugaritic shows that that consonant was a part of the word in Ugaritic. Because
most loan-words do not show that consonant, the first presumption must be that it is a native
West-Semitic word.

— p. 106 (§32.143.44). It is not quite clear why the {z} in Ugaritic grbz is said “für akk. /s/
stehen” when there is no proof that the word was borrowed from Akkadian. The most that
can be said is that Ugaritic represents the word with {z} while the syllabic orthography
represents primarily /s/.

— p. 106 (§32.143.45). Against T.’s acceptance that tzg may be a Semitic word, the actual
distribution of usage shows it is in all likelihood a loan-word from Hurrian and that it
designates a category of offering rather than the content of an offering that would belong to
another specific category.221

— pp. 106-7 (§32.143.45). T. provides no reasons why the Hittite etymology for Ugaritic ztr
is “abzulehnen” other than that the Hittite word may not designate specifically the ‘sun-
disk’;222

— p. 107 (§32.143.53). The reading of the {t} of ḡt in RS 24.271:30 (KTU 1.123) is
uncertain (T. cites as his source for the reading “1.123,” but in KTU one finds {Īt1}, in CAT
{Īm1}—either is in fact possible). Moreover, it is far from certain that the reading {ḡĪt1} would
have the meaning “Grab(bau)” proposed by T.223

— p. 108 (§32.143.53), p. 277 (§51.7). ṣḥmn, ‘sesame’, is better classified as a Kulturwort
than as “entlehnt.” Moreover, there should be a cross-reference on the first page cited to the
second, for there the possibility is aired that ṣḥmn is a composite word (ṣamm-, ‘oil’, + ṣamm-,‘plant’). The explanation that ṣḥmn is a loan-word from Akkadian, where the composite

221 On these two points, see Pardee, Les textes rituels (2000) 500-2.
222 The etymological explanations indicated by Watson, UF 27 (1995) 542, are certainly not preferable.
223 See Pardee, Les textes rituels (2000) 692, 693, 705. In the recent study of “Der ugaritische architektonische Begriff ḡšt „Totenheiligtum“” by O. Loretz (UF 33 [2001] 377-85, esp. p. 381), the author cites his (and Dietrich’s) opinion expressed in TUAT III (1997) 1240 n. 110 that ḡšt should be read for ḡšm in RS 24.271:30’ and on pp. 381-83 he proposes the interpretation “Totentempel, Totenheiligtum” for that
text. Loretz bases this interpretation of line 30’ on his interpretation of il bt in line 29’ as “Götter des
Palastes.” In the ritual texts, however, only what is probably the singular divinity il bt, (‘tutelary) god of the
palace/dynasty’ is otherwise attested (see Ritual Texts, pp. 76-77) whereas a manifestation of ‘Ilu as il ḡšt, ‘solicitous ’Ilu,’ is attested in RS 4.474:9 (KTU 1.65). For these reasons, I have proposed that il bt in
line 29’ should correspond to the singular deity and [il ḡšt-l] in line 30’ (perhaps restored as il ḡšt) be
interpreted as ‘god of sollicitude’, i.e., an abstract formulation of il ḡšt (Textes rituels, p. 705).
word would first have seen the light, is also preferred there; this in spite of the fact that the best-attested form of the Akkadian word is šamaššammū, a form significantly different from the Ugaritic one.

— pp. 109-10 (§32.144.12b), p. 793 (§83.141.2b), p. 807 (§85.2b). As I have argued, there is no reason other than the weight of interpretational tradition to take tn in RS 3.343+ iii 19' (KTU 1.15) as meaning “ändern” or “verändern” and hence to represent a case of Ugaritic [t] for Semitic /ß/. Though on p. 109 T. presents the case with a question mark, he considers the interpretation plausible and addsuces other forms in its support (see following remark); on pp. 793 and 807, the interpretation is intensified by the prefixation of “ver-” and no question mark is attached to the translation. Also on pp. 793 and 807, he restores pr, ‘to break’, with a question mark, at the end of the preceding line as a parallel to tn, a completely gratuitous restoration if tn does not mean ‘to alter’.

— p. 109 (§32.144.12b), p. 298 (§53.331.2). In the first passage cited, T. asserts categorically that [šl’nn] in CAT 4.275:6 (RS 17.125) is a “Flüchtigkeitsfehler” for the regular Ugaritic tl’nn, ‘table’; in the second, he lists the reading with no indication of it being erroneous. I have not collated this tablet, but Virolleaud copied the sign in question as a perfectly preserved [t], and there is no reason to doubt this witness, for Virolleaud was not prone to this type of error, while CAT is replete with errors of all kinds.

— p. 110 (§32.144.12b). T. seems to be putting too much weight on the similarity of “ändern” and “anderer” in citing tn, ‘another (male person)’ (RS 2.[003] + ii 48 [KTU 1.14]), in support of the argument that /my/ and /šmy/ have fallen together in Ugaritic. In that context, in which a man hands his new bride over to another man, tn means ‘a second (male person)’, not ‘a changed (male person)’!

— p. 110 (§32.144.12b), p. 144 (§33.115.41), p. 286 (§52.5f), p. 322 (§54.315.2c), p. 324 (§54.323a), p. 346 (§62.122b), p. 487 (§73.523c), p. 495 (§73.534a), p. 569 (§74.423), p. 786 (§83.113h), p. 910 (§97.94). T.’s interpretation of ttth in RS 15.128:4 (KTU 3.3) and RS 18.031:24 (KTU 2.38) as meaning ‘other’ may not be admitted. On pp. 144 and 346 he takes the word tt in this meaning as an unidentifiable form of the number ‘two’, unidentifiable apparently because he is not able to accept the grammatical interpretation proposed by the originator of the hypothesis for RS 18.031, viz., that it would be “un nombre cardinal … employé là où on attend un ordinal.” This is the grammatical analysis adopted recently

224 See CAD Š1, pp. 301-7, where it is argued that the Akkadian term actually denoted a type of flax rather than sesame.


226 Tropper and Vita have elsewhere declared [šl’nn] in this text to be a typographical error (UF 30 [1998] 699).

227 From T.’s presentation of a similar case for the masculine form tn (pp. 365-66 [§63.12]), it is clear that the distinction between the ordinal number adjective ‘second’ and an adjective meaning ‘other’ is artificial, not, apparently, based on differently vocalized forms in Ugaritic (he refers to the fact that CAD makes no distinction between šanû, “zweiter, nächster,” and šanû, “anderer”), and ultimately useless for the interpretation of Ugaritic texts. An ‘other’ entity is a ‘second’ entity.

228 Hoftijzer, UF 11 (1979) 385 n. 19.
also by del Olmo Lete and Sanmartín. As T.'s belabored attempt at interpreting ‘rb … b ḫbbth hwt ṭḥ (RS 15.128) as meaning « falls er in ein anderes Land flieht » (p. 322; cf. p. 487) shows, such an interpretation of ṭḥ simply cannot hold: usage of the directive/locative morpheme -h in all the languages where it appears shows that it should be attached to the noun, not the adjective in a phrase such as this. T.'s explanation of the placement here as having “veiliecht allein orthograph. Gründe” after the real morpheme had disappeared from hwt (“/ḥuwwatâ/ ← */ḥuwwatah/) ← /ḥuwwataha/”) finds no basis in Ugaritic orthographic practice, for there is no usage there of -h as mater lectionis and no reason to believe that the /h/ of the directive/locative morpheme ever quiesced (on this, see below, remarks to pp. 151-52 [§33.131.1], etc., p. 215 [§41.221.11b], etc., and pp. 320-25 [§54.3]). In both texts, ṭḥ is better explained as derived from the doubly-weak verbal root TWY, ‘to halt, to be a guest in a place,’ as the editor originally proposed for RS 18.031. New texts (RS 94.2592 and RS 96.2039) show that ḦBT/ḤPT means ‘to serve as ḫuptu (for someone),’ that it need not involve military service, and that it does not designate flight per se. RS 18.031:24-25 (w ányk ṭḥ by ‘ky ‘ṛyt) is, therefore, to be translated ‘your ships have found haven in Acco, (they are) stripped (of their sails)’; while RS 15.128 may be rendered ‘(such-and-such persons) have guaranteed (so-and-so) as regards his performing his ḫuptu-service (/ḫi ḫabāṭihu/) in the land of his residence’ (the land is not identified in the text). This latter formulation regards the performance of service by the guaranteed person, perhaps as a debtor, not his flight to another land.

— p. 110 (§32.144.12b, §32.144.14). ṭph in RS 1.019:2, 13 may well mean ‘family’, i.e., correspond to ṣph in standard Ugaritic, and ṭps in line 7 may correspond to standard Ugaritic ṣps, “Ṣapṣu, the sun.” T. cites the two forms in separate paragraphs and does not observe that these are the only examples in this text of {ṭ} before /p/. This may explain the apparent shift of /š/ to /ṭ/ in these words. However that may be, the multiple difficulties of


230Virolleaud, GLECS 8 (18 novembre 1959) 65; idem., PRU V (1965) 83. In PRU II (1957) 189, Virolleaud took the ḥ of hwt ṭḥ in RS 15.128:4 as a personal pronoun, remarking that ‘L’association des mots hwt ‘la vie’ et ṭḥ ‘deux ou deuxième’ pose un difficile problème.’ This difficulty, it appears to me, is greater than the problem posed by the disappearance of both /w/ and /y/ in the Ugaritic forms (perhaps this was what kept T. from adopting, or even commenting on, Virolleaud’s explanation of RS 18.031:24). Note, as regards TWY, that one would not expect the /w/ to be present in the G-stem (it is attested in the D-stem in RS 3.325iv 44 [KTU 1.16], where the meaning is ‘to receive someone as a guest’—see remark below to p. 669 [§75.537d], etc.) and that third-radical /y/ often disappears; thus the vocalization of {ṭṭ} may have been either ṭāṭ/ ← ṭāt ← ṭāwiyaṭ/ (3 f.s. /QTLa/) or ṭāṭu/ ← ṭāwiyaṭu/ (f.s. stative part.).

231See below, remark to p. 137 (§33.112.31), etc..

232Pardee, Context III (2002) 94. On ‘ṛyt, see remark below to p. 195 (§33.322.2b), etc.

233Hoftijzer and van Soldt, UF 23 (1991) 189-91, avoid this problem by taking b ḫbbth hwt ṭḥ as the beginning of a new phrase. That syntactic analysis, possible but somewhat awkward, becomes unnecessary once ḫbbth and ṭḥ are correctly identified. On the analysis of the phrase b ṣyḥ[m] in RS 19.066, see below, remark to p. 226 (§41.222.4a), etc.

interpretation posed by this text may be taken as indicators that it was not written by a scribe trained in one of the standard schools of Ugarit.235

— p. 111 (§32.144.16), p. 170 (§33.211.1), p. 203 (§33.425). In the first section cited, {ūryn}, “Kronprinz,” is said to be plausibly a Hurrian loan-word; in the second, the etymology of the word is said to be unclear; in the third, the word is cited among “Fremdbzw. Lehnwörter.” There is no cross reference in any of these sections to any other, and only the first two pages are indicated in the index of Ugaritic words.

— p. 112 (§32.144.17), p. 159 (§33.141.6). Since the element {ištr} of the personal name {ištrmy} is Akkadian, as T. observes (p. 112), why should the second not be as well? If so, the name does not illustrate quiescence of /≥/ in syllable-initial position (as T. holds, p. 159), but only represents the Akkadian pronunciation without /≥/, viz., /(≥)ißtarumm≠/.  

— pp. 113-14 (§32.144.231). The case for {mÿrn} meaning “rain’ (√MT˚R) in RIH 78/14:13’ (CAT 1.163:6) being very strong and that for {thbÿn} (line 10’) being a form of the root elsewhere attested as HBT being at the very least plausible (one cannot say more because the word appears in a broken context), it appears necessary to classify this text with RS 5.194 (KTU 1.24) as texts in which {z} is written in the traditional form and in which the sign {t} does not appear. Remove, therefore, the question mark that T. places at the head of each of these entries.

— p. 114 (§32.144.24). It is astounding to me that the two cases just discussed, of which one is contextually clear while both fit into a known graphic system, are marked as questionable, while the two alleged cases of {z} for /s/ are indicated as “wahrsch.” when neither of these criteria applies: (1) there is no constraining interpretational reason to analyze {zm} in RIH 78/20:7 (CAT 1.169) and {npÿl} in line 15 of the same text as containing etymological /s/ and (2) the phoneme /s/ is elsewhere represented in this text by {ṣ}.236

— p. 120 (§32.145.22), p. 556 (§74.412.28), p. 894 (§96.242b)  In these three entries, T. uses “n[neue] L[esung]” in the sense of ‘new reconstruction’, for his reading of {qßt yqb [yd]rk} in RS 2.[004] v 35’-36’ {KTU 1.17) differs from that of CAT only in the restoration of {d} instead of {b} as the last sign in line 35 (no restoration of the end of line 35 was proposed in KTU). In this respect, the parenthetical remark placed after “n[neue] L[esung]” on p. 894 to the effect that “am ende von Z. 35 ist nur für zwei Zeichen Platz” is potentially misleading, for the authors of CAT also proposed the restoration of only two signs. In this section, which deals with juxtaposed sentences, stating who has and who has not restored a w before the partially restored verb would have disambiguated the juxtaposition of phrases.

— p. 122 (§32.146.221). It is improper to list DHL, “Angst haben,” as Ugaritic when the root is not yet attested in Ugaritic. No reference is given at this entry, but only two instances of the root have been claimed for Ugaritic: RS 15.008:12 (KTU 2.16), where the reading is in fact (tw hern) (see remark below to p. 545 [§74.412.14], etc.), and RS 16.379:21


236See Pardee, Les textes rituels (2000) 875, 877, 885. Ford (crediting C. Cohen with the idea) has recently suggested that the root of the first word is ZM’, ‘to be thirsty’ (UF 34 [2002] 176-177 ).
(KTU 2.30), where the reading is {tdhš} (see above, remark to p. 57 [§21.352.1 {š} für
{l}] ; T.’s index contains no references to the latter passage).237
— p. 123 (§32.146.23a). As {šmh} in RS 24.293:16 (KTU 1.133) would be the only
instance in Ugaritic of this writing for {šmh}, ‘rejoice’, as this text contains no other aberrant
spellings, as one of the standard Ugaritic texts with no tendency to peculiar spellings
contains {šmhy} (RS 2.[022]+ ii 25 = KTU 1.5), and as one may not admit that “der Kontext
der Form [in RS 24.293] spricht eindeutig zugunsten der vorgeschlagenen Interpretation,”
one must seek another etymology for the word.238
(§64.1), p. 372 (§64.21), p. 373 (§64.24), p. 375 (§64.62), p. 410 (§69.223.11), p. 411
(§69.223.3), p. 413 (§69.233), p. 419 (§69.322.3), p. 420 (§69.52), p. 832 (§89.28). It can
be admitted neither that the lth-measure is “offenbar im Volumen eines halben Homer (ug.
hmr)” (p. 123) nor that mlth means “Hälfte” (pp. 267, 371, 372, 413, 420, 832). The volume
of the lth, assigned by comparison with Hebrew letek (!), may be correct, but that is far from
‘obvious’.239 Indeed, to the extent that the lth is definable by the mlth, at least two texts
speak against it. (1) In RS 18.024:26-27 (KTU 4.337) ‘a talent and a mlth of asa foetida’ are
said to go for 7 1/2 shekels (of silver). As the price of seven shekels per talent is
reasonable, the half-shekels of silver seems to correspond to the mlth, which would make the
value of the latter 1/14 of a talent, which if rounded off to 1/15, would correspond to a round
number, i.e., 200 shekels, or about two kg.,240 much less than half a homer.241 (2) T.’s
interpretation of RIH 83/12:7-8 // RIH 84/8:11-14 (CAT 4.778 and 4.782), which is identified
as a “n[eue] L[esung]” (pp. 372, 413), simply will not hold water. The passage reads kd
šmml mlthm šsr ‘l ʾābn šw and T. translates “ein kd Öl abzüglich der Hälfte davon (sc.
der Hälfte des zuvor genannten ṭt-Masses oder eines kd) zu Lasten von PN 1, Sohn des
PN2.” Each entry of this text, however, is independent of the others and the interpretation of
mlthm must, therefore, be comprehensible within the text as quoted; moreover mlthm is most
plausibly taken as a dual242 (the use of enclitic-ßen is infrequent in economic texts;243 if the
word were plural in a text of this nature, the number of units would have been specified).

237The second passage is the only one listed for this root in del Olmo Lete and Sanmartín, Diccionario I
239As T. appears implicitly to recognize by three times (pp. 372, 373, 375) mentioning Heltzer’s hypothesis
according to which the ltk was 1/10 of a kd (UF 21 [1989] 200-2); but T. considers this possibility as only
“denkbar” (while at the same time expanding the meaning to include the dd-measure).
240For the argumentation, see Pardee, Syria 77 (2000) 54. In my study of RS 1.012 (AuOr 20 [2002], chart
on p. 170), I mistakenly indicated the lth as having a volume of 1.5 liter instead of a weight of 1.9 kg.
241It is generally assumed that the Ugaritic homer would have been the equivalent of the Mesopotamian
one, about 100 liters (cf. del Olmo Lete and Sanmartín, Diccionario I [1996] 100), though there is no
internal proof for this of which I am aware.
242Bordreuil, CRAI 1987, p. 294. This interpretation is mentioned by T. as an alternative on pp. 372 and
413 but it is in fact the only one that makes sense in the passage.
243T. only identifies this specifically as his interpretation on p. 832.
For these reasons, there can be no doubt that the \textit{mlth}-measure was less than a half (since two of them are subtracted from\textsuperscript{244} a single \textit{kd}-measure)\textsuperscript{245} and probably considerably less than a half (otherwise one wonders why the debt would have been expressed in terms of a \textit{kd}-measure of which a major portion was missing). The value of 1/15 derived inductively from RS 18.024 fits these requirements well.\textsuperscript{246} One must, it appears, deal with the possibility that Ugaritic \textit{lt} and Hebrew \textit{letek} are in fact ‘false friends’. As for the Ugaritic homer-measure, first mentioned in §32.146.23a, then in §51.41e, §69.223.11, §69.223.2, and §69.322.3 (though it is absent from the index of Ugaritic words), one must judge it unlikely that that word appears in RS 1.012 (\textit{KTU 4.14}), for ‘fifty homer-measures’ (perhaps some 5,000 liters) of an unknown commodity written \{\textit{skm}\} is far too great a quantity for that text. It is more plausible that the word \textit{hm}r in that text refers to actual donkeys.\textsuperscript{247}

\textsuperscript{244}On the form and meaning of \textit{ḥsr}, see below, remark to p. 258 (§51.42c), etc.

\textsuperscript{245}T. argues that the \{-m\} of \textit{mltm} must be the enclitic morpheme and not the dual morpheme because “‘zwei Hälften’ inhaltlich sinnlos ist.” (What is known in English as being between the rock and the hard place.)

\textsuperscript{246}If \textit{mlth} is correctly interpreted as designating 1/15th of a talent in RS 18.024, it appears likely that it took on the general value of 1/15th and was used in that sense with other measures (the various attestations are cited by T., p. 372). \textit{mlth} in the one well-preserved text that clearly indicates the surface measures of fields RS 17.246:14 [\textit{KTU 4.282}], if it does indeed designate a fraction, may indicate the amount of land normally sown by a \textit{mlth}-container of grain (rather than ‘a half’ of the \textit{šd} as T. believes: p. 267 [§51.45f], p. 372 [§64.21]).

\textsuperscript{247}See Pardee, \textit{AuOr} 20 (2002) 175-76.


{y} is not sufficient to rule out the traditional interpretation by ‘LY, ‘to ascend’,\textsuperscript{250} and a geminate root may also be considered.

— p. 126 (§32.146.33a), p. 661 (§75.532). There is no need to take \{[y]‘nynn\} in RS 2, [004] vi 32’ (\textit{KTU} 1.17) as showing an irregular correspondence with Arabic GNY: it may be the widely attested root ‘NY meaning ‘to answer, respond’.\textsuperscript{251}

— p. 137 (§33.112.31), cf. p. 255 (§51.41e). New attestations of \{ḥbt\} vs. \{ḥpt\} confirm T.’s identification of the basic form as /ḥbṭ/ with /ḥpt/ arising by devoicing by devoicing of the /b/ in contact with the /y/. Moreover, a new text appears to show clearly that the two forms are not randomly distributed but that the devoicing only occurs when the /b/ is in immediate proximity with the /y/. RS 96.2039:8-9 reads \textit{w ḥt ḥn bnš hw b gty ḥbṭ}, ‘and moreover, this servant was working in my farm’. \{ḥbt\} must here be a verb and hence be vocalized something like /ḥabata/. RS 94.2592 shows that the two forms may appear in the same text, \{ḥbt\} as a verb (II. 4’-5’ and 6’-7’: \textit{kI bnšm dt ḥbṭ tnn}, ‘all the servants who have been working there’), \{ḥpt\} as a noun (II. 12’-14’ w ḥpt p mnm hw b ṣpr štn, ‘and [each] worker, no matter who he is, put [his name, lit. ‘put him’] in a document’). This type of distinction is clear also for all but one of the examples cited by T.\textsuperscript{252} The apparent exception is RS 18.050:8 (\textit{KTU} 4.360), where one finds the phrase \{āḥd ḥbt\}, ‘one worker’. Either this is an exception to the rule of proximity or else \{ḥbt\} here is not the common noun, but the G-stem participle reflecting the verbal usages just cited (i.e., /wa ‘āḥḥadu ḥābiṭu/, ‘and one is performing ḥuptu-duty’) or, perhaps, a /qattāl/ \textit{nomen professionalis}. This distribution of forms makes it not unlikely, I would judge, that the forms \{lbš\} and \{lpš\}, meaning ‘garment’, reflect different bases of which only in the second would the original /b/ have been in immediate proximity with the /š/ (perhaps something like /lubšu/ and /lipšu/ ← /lšbšu/).

On this same basis, it is unlikely that \{mqb\} and \{mqp\} represent variants of the same word if that word was a \textit{m}-preformative noun from a \textit{I}-\textit{n} root (p. 138 [§33.112.22]). T. states immediate proximity as the rule in §33.112.34 (p. 138) and accounts for what he considers to be truly variant forms as owing to some forms being written purely phonetically, others according to a “phonematisch-etymologisch ausgerichteten Orthographie”. It appears clear, however, that, in the case of certain lexemes, the number of the latter may be reduced. Indeed, it is not impossible that \{ḥbt\}/\{ḥpt\} and \{lbš\}/\{lpš\} may actually have been lexicalized according to their (etymologically secondary) pronunciation. Finally, I observe with regard to ḤBT that, in both of the new texts cited, it is \textit{bnšm}, i.e., ‘(royal) servicemen’, who are said to be ḤBT and the service takes place on royal farms (explicitly in RS 96.2039, etc.).

\textsuperscript{250}Pardee, \textit{Les textes rituels} (2000) 715, 723 (I would now vocalize, however, /ya‘lû/, see remark below to p. 656 [§75.531e]).

\textsuperscript{251}Pardee, \textit{Context I} (1997) 347 n. 43; Wyatt, \textit{Religious Texts} (1998) 273-74 n. 112. My suggestion to restore \{[l]‘nynn\} would require that the verb be parsed as D-stem, for the /’/ appears to assimilate to the /’/ in the 1 c.s. of the G-stem (see T. p. 157 [§33.141.2], p. 432 [§73.223.1], p. 492 [§73.531.2], p. 661 [§75.532]).

\textsuperscript{252}On RS 15.128:4, see above, remark to p. 110 (§32.144.12b), etc. \{ḥḥṭ\} in RS 15.098:8 (\textit{KTU} 2.17:1) illustrates that ‘segholate’-type nouns (i.e., /qatl/qitl/qutl/) had a dissyllabic stem in the plural, just as in the other Northwest-Semitic languages (on this, see T. pp. 251 [§51.41] and p. 295 [§53.322.1a]).
by implication in RS 94.2592). In these texts, and probably in many of the administrative
texts cited by T., .Prot has none of the three meanings that he indicates ("Entflohenener;
Freigelassener; Soldat"); as a verbal noun "Freikommen, Fliehen"); rather it designates
laborers on one of the royal farms or elsewhere. In other cases, e.g., RS 18.050:8 (KTU
4.360), .Prot appears to stand in opposition to ³’, another type of worker (← P’L), perhaps'textile-worker' in particular. The form there is singular, plausibly a G-stem participle (/haibi/) rather than the common noun, as proposed above. One also encounters the plural
.Prot in RS 15.098:8 (KTU 2.17:1), where the context appears to favor the interpretation as the plural of /u̱pt/, viz., /u̱ubatima/ (the word is in the oblique case there) with the expected /b/ when there is a vowel between that consonant and the /l/. Finally, it should be noted
that, at least in prose texts, there is no reason to take .Prot as a collective noun; indeed,
in one text, 3 m.s. pronouns, both independent and suffixal, are used in reference to .Prot (see
RS 94.2592:13-14’, cited above this note). Thus the proper interpretation of ³m tirops .Prot
hndn (RS 34.124:10 [KTU 2.72]) is not "Warum schickst du denn diese .Prot-Truppe" (p. 230
[§42.2]; cf. pp. 548 [§74.412.22], 701 [§76.427b], 733 [§77.413], 789 [§83.122d]) but
"Pourquoi envoies-tu ce .Prot-soldat-ci … ?" though the new data render questionable
whether this .Prot's assignment was specifically military.
administrative texts that has just been described may serve to elucidate an enigmatic poetic
passage. RS 3.343+ i 5’-6’ (KTU 1.015) reads ³tr’ tzg l ‘gh / bn .Prot l unthm, 'As' a cow
 lows to its calf, sons of .Prot to their mothers …'. When translating this passage, I have had no
idea what to do with the phrase bn .Prot and neither does T. An interpretation based on
the use of .Prot near the beginning of the Kirta epic, where that word is used to describe one
of the elements of a šbu, an 'army', is on the right track. It may be inferred from the

253References in T. cited above, remark to p. 110 (§32.144.12b), etc.
254Cf. M Heltzer, The Internal Organization of the Kingdom of Ugarit (Royal Service-System, Taxes, Royal
(translating forms spelled {hht}-); M. Astour, ‘The ³hapiru in the Amarna Texts. Basic Points of
Controversy,'' UF 31 (1999) 32: ‘‘ … the lowest and most oppressed class of the native population’’
(defining ³hapšu).
255Pardee, Semitica 49 (1999) 56-57. One should note, by the way, that this ³’ cannot be explained as
owing to voicing owing to immediate contiguity of the /p/ and the /l/: whether the form be /b’a’lul/ or
/ba’alul/, the two best attested nomina agentis, there would have been a vowel between the original /p/
and the /l/.
256Bordreuil and Pardee, Une bibliothèque (1991) 144. On the interpretation of the verb tlika, see below,
remark to p. 220 (§41.221.2). Observing the proper interpretation of .Prot shows that the nuance expressed
here by the D-stem is not that of the plurality of the object, a possibility aired on p. 548 (§74.412.22).
258In the first two sections cited, he lists this passage among those where the substantive meaning
‘Entflohenener; Freigelassener; Soldat’ would appear; on p. 487, he proposes seeing in .Prot a verbal noun,
‘Jungtiere, die sich verlaufen haben (w.: Jungtiere der Verirrung ?)’.
légendes (Littératures Anciennes du Proche-Orient 7; Paris: Cerf, 1974) 536-37 note d.
parallel with ‘gl, ‘calf’, that the word bn in the phrase bn hpt is not to be taken as a category marker leading to the translation “soldiers.” 260 The lines should be translated: ‘As a cow lows for her calf, as sons of servicemen261 (make a comparable human noise) for their mothers, so the Udmites will mourn for her’. The situation is well illustrated by RS 96.2039 (cf. preceding remark), which illustrates the plight of spouses separated by their masters. The poet may be using hyperbole by referring to very young males parting for warfare and hence separated from their mothers.

— p. 137 (§33.112.32), p. 410 (§69.223.11), p. 477 (§73.431a). hrmr in RS 20.010:7 (KTU 4.691) is interpreted as ‘donkeys’ in the first and third references cited, as the hmr-measure in the second, i.e., in one of the sections dealing with numeral expressions, weights, and measures. The word also appears in line 5 of this text, where there is little doubt that it expresses the homer-measure (p. 411 [§69.223.3], p. 419 [§69.322.3]).

— p. 138 (§33.112.33), p. 266 (§51.45e), p. 404 (§69.172a). On p. 404, mitm mqp in RS 14.129:2 (KTU 4.127) is indicated as a “n[eeu] L[esung].” The reading of the second word is, however, given as {mqpm} in the first reference cited and that form is there explicitly identified as a plural. On p. 266, the possibility of the two readings is indicated by placing the [m] in parentheses. The editor read {[…]itm . mqp . m[…]} (PRU II 145) and the authors of KTU indicated {[…]itm . mqp . m[…]}; in CAT one finds {[…]itm . mqp . m[…]}, but the {i} for {i} is probably just one of the numerous typographical errors by which this edition is marred, rather than a new reading, for the two signs have no physical resemblance and the editor copied a clear {i}. If there is a new reading from T., therefore, it is on p. 138, where the word-divider which is read by all witnesses after mqb appears to have been disregarded.

— p. 139 (§33.112.35). Though I have myself vocalized nbt, ‘honey’ as /nubtu/, following Hebrew nºpet,262 the presence of the {b} may indicate that in the Ugaritic form there was a vowel between that consonant and the /t/. T. observes that the Hebrew form with /p/ may be owing to devoicing in proximity with the /t/ and that the base form may have been /nubt-. He does not, however, remark that forms such as hpt and lps (see remark above to p. 137 [§33.112.31]) indicate that proto-Ugaritic /nubt/- may be expected to have become /nupt/- in Ugaritic. One should perhaps postulate, therefore, that the Ugaritic form was /nubtu/, i.e., the ‘long’ form of the feminine morpheme would have been retained in Ugaritic because of the long vowel in the stem.263


261 hpt is not necessarily a collective here: the plurality of the phrase is provided by bn (masc. pl. constr.) and the two plural elements of the last word (‘mothers’ and ‘their’).


263 Arabic shows a form nūbīm, ‘bees’, which is considered by the traditional lexicographers as the plural of nāyibīm but of uncertain etymology (see Lane, p. 2863). For Akkadian, CAD indicates the late form nūbu, meaning “honeybee.” By comparison with this hypothesis for Ugaritic, Hebrew nºpet would plausibly have come from proto-Hebrew /nūba/ which became /nuptu/ through loss of the feminine /a/, shortening of /ū/ because the syllable was now closed, and devoicing of the /b/ in proximity to the /t/; this proto-Hebrew
Archiv für Orientforschung 50 (2003/2004) online version

— p. 139 (§33.112.35). ‘ps in RS 18.148:20 (KTU 2.47:17) is probably not a nomen rectum governed by the preceding word,²⁶⁴ for two lines lower one finds ḥrdk ‘ps, where the pronominal suffix precludes that syntactic analysis. Bordreuil and I have proposed that ‘ps in both cases, as well as in RS 88.2159:14, is a verb²⁶⁵ (an analysis that T. accepts here for the last case only).

— p. 139 (§33.112.36). The four signs after the word-divider in RS 29.095:12 (KTU 2.71) are [p'pr], not [t'pr]. T. proposes that [t'pr], taken from KTU/CAT, may be interpreted as though from ‘BR. On the interpretation of the passage, see below, remark to p. 270 (§51.45w), etc.

— p. 140 (§33.112.52), p. 313 (§54.133.2d), p. 351 (§62.202.4). One may grant that T. has thrown more light on the usage of kbd in administrative texts than anyone since Liverani²⁶⁶ (see above, introductory remarks with note 6) while remaining skeptical that the usage requires an etymology separate from KBD, ‘to be heavy, important, etc.’—T. would have this term derive from a root KPT, ‘to put together, to add’. Indeed, the two roots show sufficient similarity in form and meaning to be identified as having descended from the same root. However that may be, it appears very likely that T. is correct in analyzing the morpho-syntax of kbd in Ugaritic as that of an adverbial accusative (p. 313) the function of which is to indicate the joining of a smaller number to a larger.

— p. 140 (§33.112.52), p. 558 (§74.413.2), p. 561 (§74.414.2), p. 757 (§81.11). T. takes šrd, imperative in RS 2.[003]+ ii 24 and /QTLa/ in col. iv l. 6 of the same text (KTU 1.14), as cognate with Hebrew/Punic fiRT, ‘to serve’. He claims that the analysis as Š-stem /QTLa/ forms of the root YRD, ‘to descend’, hence ‘to bring down’, to be less likely, but does not say why the explanation that involves a phonetic discrepancy is more likely than the one that does not. The object of the verb in both cases is the divinity Ba‘lu, who appears to the hero, Kirta, later in the text and who must, therefore, have descended from his lofty dwelling-place.

— p. 141 (§33.112.8), p. 304 (§54.112.3), p. 519 (§74.232.1), p. 527 (§74.232.22). Given the contraction to ‘/ammîṭar-/’, there is no reason to hesitate on the original form of the name Ammistamru as having been ‘/ammîyiṭamvr-/ (T. will only go so far as ‘/ammV-yiṭamvr-/), for the loss of the /y/ is most likely owing to contraction of homorganic elements, viz., ‘/iyj/.²⁶⁷ The name is written ‘/muṭmr/ on the king’s own seal but took on the secondary form ‘/ammîṭamru/, which probably arose at least in part because the first element bore the 1 c. s. pronominal suffix: ‘/ammîyiṭamuru/ > ‘/ammîṭamru/, i.e., the three homorganic elements /i/, /y/, and /i/ coalesced to a single /i/ vowel, short because the syllable /qult/ form would by the normal processes have become nōpet in Biblical Hebrew as we know it from the Massoretic tradition.

²⁶⁴T. does not translate the phrase ḥrd ‘ps but lists it among examples of the noun ‘ps and appears to be following van Soldt, UF 27 (1995) 486 n. 5, who translated “border troops.”


in which it is found after the coalescence is closed. The name does not, therefore, quite mean “Ammu hat beschützt” (p. 527), but ‘my divine uncle (viz., an unnamed divinity, plausibly Ba‘îţn268) has protected (this child). On the other hand, T. has gathered together in §33.112.8 so many examples of phonetic assimilation of various kinds that our explanation of the standard Ugaritic writing of the name with [t] as owing to influence from a long Akkadian tradition becomes less plausible: here it would be a simple case of assimilation of non-voicing, ‘ammiĎtamru/ > ‘ammitamru/ (so T. on pp. 141 and 527)—the writing of the name with signs representing a sibilant in Akkadian is simply the regular representation of Ugaritic /ʃ/.

— p. 142 (§33.114.11), pp. 348-49 (§62.182b), p. 526 (§74.232.21), p. 572 (§74.432), p. 594 (§74.622.3). T. divides with an even hand the readings [tmtmn] or [ttmmm] in RS 3.343+ ii 24’ (KTU 1.15), the former on pp. 348, 572, the latter on pp. 349, 594 (on p. 142, the form is only identified as Š-stem of vTMNY).

— p. 142 (§33.114.11), p. 377 (§65.131b), p. 587 (§74.621), pp. 604-5 (§74.632). Though in the fourth section cited here T. calls his interpretation of RS 24.248:15-20 (KTU 1.104) “sehr unsicher” and refers to the possibility of a word-divider in the sequence of signs that he presents as [tθdmn] (l. 17), he apparently considers the transliteration in CAT without the word-divider preferable to mine with the word-divider269. He argues on the basis of the writing [tθdmn] that the /ʃ/ of the Š-stem becomes /ʃ/ before a root containing /ʃ/ in third root position. Because the reading without the word-divider may not be retained,270 one of only two examples of the phenomenon is thereby eliminated. Because the other example cited (presented here on p. 588 [§74.622.3]) occurs in a broken passage of an obscure text (RIH 77/18:8 [tθbl-În]271 [CAT 1.175]), the grammatical category must for the time being be considered highly dubious. It should be remarked in this context that the only example of a II-ʃ root showing /ʃ/ for the consonantal phoneme of the Š-stem is ŹTB, ‘to sit’, where the /ʃ/ ← /w/ is no longer present in the Š-stem (/ʃawʃab-/ > /ʃotab-/); it is clearly from this secondary form that the Ugaritic form [tθb] = /ʃotab-/ developed (so T., who takes the only attested form as a passive of the Š-stem: p. 604 [§74.632]).


269The reading was [t̮ n] in the transliteration that I provided both to the authors of CAT and to T. The word-divider was placed in half-brackets because of its damaged condition, but, considering the place of the traces in the sequence of signs, those traces can only be those of a word-divider. Not only did the original editor of the text read a word-divider here (Herder, Ugaritica VII [1978] 39-41), but T. himself once pointed out that the reading was plausible: Der ugaritische Kausativstamm und die Kausativbildungen des semitischen. Eine morphologisch-semantische Untersuchung zum Š-Stamm und zu den umstrittenen nichtsibilantischen Kausativstämmen des Ugaritischen (Abhandlungen zur Literatur Alt-Syrien-Palästinas 2; Münster: UGARIT-Verlag, 1990) 26, n. 10.

270I indicate my interpretation of this passage below, remark to p. 211 (§41.12), etc. In his review of my treatment of this text in Les textes rituels, T. equivocates on this point, admitting that it appears to be there but not changing his interpretation of [tθdmn] (UF 33 [2001] 690).

271T. accepts the reading of CAT, [tθbn], as certain, but the reading of the fifth sign is in fact unclear (on the tablet, one sees a horizontal wedge overlying what may originally have been a [t]).
The formulation “/’ah(h)att-/ ← * ’ahad(a)-’eine’” (p. 142) is questionable on two grounds: (1) as T. himself holds in §33.171.2 (pp. 166-67) and §62.111.1 (p. 344), there is no particular reason to doubt that the /h/ was geminated in Ugaritic, as it was in Hebrew:272 if this was true for the two languages, it may have been true for proto-Northwest Semitic (this is only a hypothesis, of course, for the basic Aramaic form /had/ has lost the first syllable, whether that was ./’a/ or /’ah/); (2) as T. himself states in the introductory paragraphs to the first section cited (§33.115 and §33.115.1), the assimilation of the root consonant /d/ to the feminine morpheme occurs only when the two consonantal morphemes are in immediate contiguity; the proto-form in both languages must therefore have been /’ahhadat-/ and indicating that the hypothetical earlier form may have been /’ahhadat-/ (p. 142) is of no relevance in this case.273 For these reasons, listing ’ahd as a /qatal/ form on pp. 257, 343 and āht as a /qatalat/ form on p. 258 may be questioned. Furthermore, the explanation of the Hebrew form ’eḥād (← /’aḥhād-/), cited on p. 256, as owing to “Vortongemination” may not be accepted for proto-Hebrew because the construct form shows the same gemination: ’ahad, with pataḥ in the first syllable, must have come from /’aḥhād-/ (the same is true of the feminine construct form ’ahat). It is thus clear that the /h/ was doubled in proto-Hebrew, though that this was also the case in Ugaritic may only be a working hypothesis. Assuming that the accentual systems of proto-Ugaritic and proto-Hebrew were similar, both languages may have had a form /’ahhadu/ of which the feminine was /’ahhātu/ ← /’aḥhādut/274 a hypothetical earlier form /’aḥhādatu/ would have belonged to a proto-form of the two languages.

— p. 142 (§33.115.11), p. 143 (§33.115.12). Three passages in two new texts, RS 94.2563:22’ {[…]m. w. ‘t. šmn}, line 26’ {[…]t. ‘t. šmn}, and RS 94.2600:14 {[kršu. w. ‘t. šmn]}, illustrate the connection between the ‘t’ measure and oil (T.’s definition is “ein kleiner Behälter, zugleich ein Mass (für Öl)’’). RS 94.2600:14 shows that the ‘t’-measure is probably smaller than the krsu-measure, for the larger normally precedes the smaller in such a formulation. There are no new data regarding T.’s indubitably correct identification of ‘t as a feminine singular form of a noun from the root T ˙≤D of which the plural is ‘dt, i.e., the

272The masculine absolute form ’eḥād alongside the construct form ’ahad and the feminine pausal form ’eḥāt alongside the absolute and construct form ’ahat leave no doubt that both forms showed /ḥḥ/ in proto-Hebrew, for the seghol-dissimilation in pretonic position occurs only when the following guttural was historically doubled.

273Compare the following entry (commented upon in my following remark), where the proto-form is correctly indicated as “*tā’idtu.” [āht] in RS 88.2159:4, 23 (RSO XIV 51) is also indicated as coming from “*ahadta” and [yrt] in RS 2.022:4 i 6 from “yaradtā” (the long vowel here is, however, questionable—see following remark). On the other hand, [ylt] in RS 2.002:53, 60 (KTU 1.23) is said to derive from “*yalad(a)t,” [yrt] in RS 2.004:4 41’ (KTU 1.17) from “*yālidat-,” and the noun št, ‘lady’, from “*šVw/šd(V)t-.”

274On pp. 166-67, T. cites the feminine form as an example of “Vortongemination,” but one may doubt that the accent preceded the /h/ in the feminine form (see above, remark to p. 89 [§31.42]).
singular form shows assimilation of the third root consonant /d/ to the feminine morpheme /t/.275

— p. 143 (§33.115.11). The /ã/ of “yaradtã” should be indicated as short or, at the very least, as anceps, i.e., /ã/ or /ã/ (on p. 465 [§73.331.3], both the masculine and the feminine forms are first given as anceps then normalized with the short vowel). Even the latter formulation, however, is highly questionable since there is no reason to believe (1) that the 2 m.s. pronoun had /ã/ in proto-Ugaritic nor (2) that /a/ would have become /ã/ in this position (see more detailed remark below to p. 207 [§41.1]).

— p. 144 (§33.115.42), p. 535 (§74.331), p. 536 (§74.333), p. 615 (§75.215). Everywhere but p. 536 T. assumes that tãdm in RS 2.[003]± ii 9 (KTU 1.14) and RS 3.322± iv 42 (KTU 1.19) and yãdm in RS 2.[003]± iii 52 are N-stem /YQTL/ forms of the root ‘DM meaning ‘to redder oneself’, i.e., ‘to apply red cosmetic to oneself’. On p. 536, he suggests that the form may in fact be D-stem and mean ‘to apply red cosmetic’. Since the D-stem would have to be factitive and since no object is expressed in any of these passages, this alternative analysis may not be given serious consideration. (The West-Semitic languages do not show a category of “internal D-stem” similar to the “internal causative” [to use traditional terminology], viz., cases in which the causative expresses a state of the agent, rather than the transmission of a state to a patient, e.g., hirḥîq in Biblical Hebrew which, according to context, means either ‘to be far away’ or ‘to cause to be far away’.)

— p. 144 (§33.115.42), p. 626 (§75.42), p. 656 (§75.531e), p. 697 (§76.421a). In the first section cited, T. vocalizes yh (RS 2.[012] i 35 [KTU 1.12]) as “yahhî,” in the third as “yahhhû” (no vocalization is indicated on pp. 626 and 697). In the first section, the root is indicated as “nhw’y” (the vocalization indicated is thus appropriate, according to T.’s standards, to the non-preferred root form), in the third as “nhw,” in the fourth as “nhy.”

— p. 144 (§33.115.43). T. proposes here that the five cases of {n} missing at the end of a word (of which only four are valid—see remark above to p. 60 [§21.354.1a] ḥswn) may be owing to assimilation of the /n/ to the first consonant of the following word. The explanation may not be taken seriously, however, for two reasons: (1) the behavior of /n/ is not remarkable, for other letters are omitted at the ends of words by simple scribal error (p. 60 [§21.354.1a]); (2) three syntactic relationships between the two words in question are visible in the four valid examples and there is no reason to believe that the case vowel would not have been present in all these cases and impeded the assimilation. On the other hand, the occasional writing of {b} for {bn} in Canaanite/Phoenician patronyms of which some forms are cited here may well represent assimilation of the /n/ in pronunciation, as many scholars have thought—if it be accepted that the case vowel was no longer present.

— p. 144 (§33.115.43), p. 823 (§89.11a). One of T.’s examples on p. 144 of /n/ at the end of a word assimilating to the first consonant of the following word (see preceding remark) is b ġlmt in RS 2.[008]± vii 54 (KTU 1.4). On p. 823, nothing is said about this hypothetical assimilation and it appears to be assumed, more properly, that the alloforms of the preposition b need not appear in the same form in a given bicolon.

275For the correlation of the new and old data, see Pardee, Syria 77 (2000) 59.
T.devotes a third of page 145 to trying to figure out why the /n/ is present in the writing of tnśn (RS 3.367 iv 17', 26' [KTU 1.2]), which he takes to be a G-stem /YQTL/ form of a 1-n root, only mentioning at the end that the form may be N-stem or D-stem. The form apparently presents such a problem to his mind because forms of this same verb with assimilated /n/ are attested elsewhere. Given that verbs often appear in more than one stem in the various Semitic languages, that G-stem intransitives often show N-stem variants in the various Semitic languages, surely parsing the form as N-stem presents far less difficulty than trying to come up with a phonological explanation for why the /n/ did not assimilate in a G-stem form (T. fixes on an explanation by position of the accent, a variation that does not produce this result with any frequency in any of the other West-Semitic languages).

Given the two possible derivations of {mnd‘} suggested by T. ("entweder aus *mi‘a‘nu ‘ida‘ ‘was weiß ich?’ oder aus *man(nu) yida‘ ‘wer weiß?’ ’), there is no obvious reason to believe that in Ugaritic (as opposed to the later languages in which forms are attested in which the /n/ has been assimilated) the /n/ was in immediate contiguity with the /d/ but nevertheless remained unassimilated.276 It would appear that, in this case, T.’s thinking has been too much affected by the later forms. For Ugaritic, one may surmise that the vocalization was /mannida‘/ (←/man‘ida‘/- or /manyida‘/-) if hypothetical T.’s derivations be accepted. Perhaps even more plausible would be /mannuda‘/ (←/manyuda‘/), /mannadū‘/- (←/manyadū‘/-), or, perhaps even more plausibly, /mahnōda‘/ (i.e., /mah/ plus the N-stem),277 all three meaning ‘what is known’? For these possibilities of derivation from passive forms, compare Hebrew maddū‘, plausibly from /mahyadū‘/. In the various Aramaic forms cited by T., a form similar to the Ugaritic form would have occurred, followed by eventual reduction to Ø of the vowel between the interrogative element and the /d/ of YD‘ and the assimilation of the /n/ to the /d/.

T. glosses the word mnd‘ discussed in the previous remark as “vielleicht, gegebenenfalls.” Though this appears indeed to be the value of the word when it is used alone, as in RS 3.325+ ii 24 (KTU 1.16), in the epistolary text RS 17.139:10-11 (KTU 2.34) the word appears twice in what is most likely a correlative construction, and the translation is, therefore, ‘whether… or’.

Though it may be granted that it is “fraglich” whether the word for ‘year’ was pronounced with or without a murmured vowel in the early 12th c. B.C.,278 one may at least say with some confidence that “had the

276T. puts only two words in this paragraph, mnd‘ and gngn (with the perhaps related toponym hlb gngnt). While one may accept the validity of the second form, which is a reduplicated form (/C1vC2vC1vC2v/), there is no clear why the first, which is formed from two different morphemes, should be expected to have developed like the first. This appears even less likely when one realizes that even some /C1vC2vC1vC2v/ forms show assimilation of /C1/ to /C2/, including gngn, which appear once as {ggn}.

277On the basic stem /mah(a)/ for the interrogative/indefinite pronoun with impersonal reference, see below, remark to p. 239 (§44.12), etc.

278A statement that is at least more careful than T.’s earlier description of the /n/ as “vokallos” (UF 31 [1999] 741).
form /ßantu/ existed in proto-Ugaritic, it would, as in Samarian Hebrew and Phoenician, where the proto-form was clearly /ßantu/, have become /ßattu/.” 279 Thus the syllabic form providing the evidence for the pronunciation of this word in Ugaritic, viz. RS 20.189A+B:11 {ša-Ian1-tu4},280 must be interpreted in one or the other of the following ways: (1) the phonetic realization of proto-Ugaritic /ßanatu/ was [ßan@tu], not /ßantu/, or (2) the rule that /n/ when contiguous to a following consonant must assimilate to this consonant was no longer operative when /ßanatu/ became /ßantu/, or (3) the syllabic writings are not to be taken at full face value (see remark below to p. 182 [§33.242a], etc.) and the actual pronunciation was /ßanatu/. The same must be said, of course, of [... Ša-Ian1-ti] in the cuneiform text from Ashqelon published by Huehnergard and van Soldt.281 There, the very presence of an /n/ shows that the “Canaanite” in question is of the southern type, rather than the Phoenician type (cf. p. 280 [§52.214]), where the /n/ of /ßantu/ would have assimilated long before. Because the southern Canaanite form eventually became /ßanā/, it appears necessary to assume that the word was there pronounced either /ßanatu/ or /ßan@tu/, not /ßantu/. These considerations lead me to doubt seriously that the phonetic realization of the Ugaritic word should be indicated as “/ßantu/.” Similar conclusions are to be drawn with regard to the plural of ‘segholate’-type nouns in Ugaritic (see above, end of the remark to p. 137 (§33.112.31), etc., and below, remark to p. 251 [§51.41], etc.).

— p. 146 (§33.115.44.5). It is not quite correct to say that in RS 19.022 (KTU 2.62) “sind nebeneinander die Formen mdym (Z. 4) und mndym (Z. 7) bezeugt” for in the first case the line reads [[-(-)]mdym], i.e., at least one and perhaps two signs preceded the signs in question, and the resemblance between the two forms, may therefore, be only superficial.

— p. 146 (§33.115.5). Ten lines are devoted to dealing with the hypothesis that ḫpn and ḫlpn may be variant forms of the same word, meaning “Umhand, Mantel,” when there is no evidence in favor of the identification. In the end, T. proposes that the two words have separate Semitic etymologies, but he does not propose a meaning for ḫlpn. In the few attestations of ḫlpn, one encounters nothing contrary to the hypothesis that this word would not be the name of a specific garment but denote simply a swath of tissue.282

— p. 148 (§33.115.6), p. 276 (§51.5b). ššrt, said to be present in RS 24.266:21 (KTU 1.119) represents a false reading in KTU taken over uncritically here. The text in fact reads [§ š rp],283 ‘a ram (as) a burnt-offering’.284


280One fragment of this text was published by Nougayrol (Ugaritica V, text 132), another by Laroche (UF 11 [1979] 477-80); the reading {ša-Ian1-tu4} was proposed by van Soldt, BiOr 47 (1990) 731, 733.


283As the editor of the text had seen: Herdner, Ugaritica VII (1978) 34 (verso, line 4).

— p. 150 (§33.116.2), p. 204 (§33.445), p. 378 (§65.145), p. 379 (§65.147c). T. hypothesizes that the form ſb’ d, ‘seven times’, represents the assimilation of the / geomet to the / of the number noun (on the form and etymology of the iterative morpheme, see below, remark to pp. 377-78 [§65.142-43]). In favor of this analysis is the fact that the multiplicative affirmative particle without / appears only with ſb’ : attested with / are įnid, įtīd, įmnīd, 285 and ‘šnid. Against this explanation of ſb’ d may be counted: (1) the expression {µb’ d w ſb’ id} in RS 9.479A:8-9 (KTU 2.12), the only occurrence of { ſb’ id} but clearly an intentional one, roughly translatable ‘seven times this (way) and seven that (way)’, and one plausible intended to express two forms of prostration, perhaps something like Amarna Akkadian kabattum u širīma, ‘on belly and back’; 286 (2) the impossibility of taking T. seriously in his proposal that { ſb’ id} in RS 9.479A:9 would be a pausal form—because the precise order of the elements occurs in five other texts where the second form is always { ſb’ d}; 287 (3) the existence of two forms in Amarna Akkadian ( {VII- ſu VII-TA-a-an}), though only the former is used at Ugarit (usually with { ſu} instead of { ſu}); (4) the plausible presence of a vowel between the / and the / (/faba’ ida/); 288 (5) the fact that the assimilation of / to / would only occur at the end of a word in this formula; (6) the fact that at least one scribe perceived the particle as separable from the number noun (‘three times’ is written {tl t, id} in RIH 78/14:12’ [CAT 1.163:5]). These factors indicate that / id was certainly an affirmative particle 289 of which the function was to express repetition; -d may have been a true variant thereof without the { }. The short form could be used either correlatively ( ſb’ d w ſb’ d) or the long form could be used contrastively (as in RS 9.479A) in the epistolary prostration formula to express two forms of prostration; as the distinction between two forms of the multiplicative suffix was not observed in the Akkadian of Ugarit, the retention in Ugaritic of a form with { } could represent an archaism. T.’s argument based on distribution remains, however, a strong one in favor of the interpretation of { -d} as a simple phonetic variant of { -id}. Perhaps the best solution would be to say that T. is correct that ſb’ d represents the contraction of /faba’ a/ + /’ida/ and that the scribe of RS 9.479A consciously recreated the more archaic form by analogy with the other

---

285 In the unpublished text RS 94.2273:4.
286 T. adopts this interpretation explicitly for the expression įnid ſb’ d, ‘two times seven times’ (p. 379). Whether or not ſb’ d consists of an assimilated form of { ſb’ } + { id}, it appears likely to me that the scribe of RS 9.479A, by going against the trend of using the form ſb’ d and writing in its place the longer form, intended to express two forms of prostration.
287 In RS 18.040:6-8 (KTU 2.40) and in RS 92.2010:7-9 (RSO XIV 50), one finds ſb’ d (w) ſb’ d mrhtm qlt; in RS 19.102:18-20 (KTU 2.64:14-16), RS 29.093:9-10 (KTU 2.70), and RS 24.2391:5-6 (unpublished—see provisionally Pardee, Context III [2002] 105) one finds įnid ſb’ d mrhtm qlt/qnl.
288 T. indicates the base form of the word for ‘seven’ as /faba’-/ (p. 348 [§62.171]), but vocalizes the putative assimilated form as “/faba’’/ ‘ id- ← *faba’’ ‘ id- ← *faba’ ’ ida,” with no explanation of the reconfiguration of the stem; as an alternative, he proposes “/faba’ ida/ ← *faba’ a’ ida, with the /’ dropping by dissimilation with the /’ (p. 150). I’m not sure whether the dropping of both /a/ and /’ belongs properly to the category of dissimilation, but the proposal is certainly plausible, viz., it is the proximity of /’/ and /’/ that triggered the dropping of the /a/ and the /’a/. As we shall see, this suggestion bests fits the related Amarna data.
289 It is only attested once separated from the preceding word by a word-divider, in the text just cited.
multiplicative forms that ended in /-ˈida/. The question remains, however, of the correct vocalization of the contracted form. There are no data from Ras Shamra on this question, but there are forms in Amarna Akkadian, where the corresponding morpheme ends in /-n/, which indicate that the vowel after the nominal stem is either /i/ or /ê/ (i.e., the result of a contraction of the sequence /aˈi/). These are EA 215:6 {ši-ib-e-TA-an}, EA 196:4 {ši-BI-T[a-a]-an}, and EA 221:6 {ši-BI-TA-an}, which may represent either /šibˈidan/ or /šibˈēdan/ 290. T.’s vocalization /šabaˈida/ is internally implausible (see note 288) and does not match the Amarna data, but his second proposal (/šabˈida/) fits all the presently available data—if one be willing to admit that the writing with {e} in the first Amarna example cited is not definitive for determining /i/ê/ distinctions.

— pp. 151-52 (§33.131.1), p. 161 (§33.142.4), pp. 320-25 (§54.3). It appears highly unlikely that T.’s vocalization of the locative directive morpheme {-h} as /ah/ (p. 152) or as /āh/ (p. 320) may be considered the correct grammatical description—T. might have been willing to explore the possibility that the morpheme might have contained a vocalic element after the /h/. One must, in any case, object to the formulation according to which the Ugaritic particle “im Anschluß an den he. Befund … wahrscheinlich als /ª˜h/ zu vokalisieren ist” (p. 320). If proto-Hebrew had had /ā/, it would have become /ō/ in Biblical Hebrew. To the extent, therefore, that Ugaritic and Hebrew are to be compared in this respect, the vowel before the /h/ in Ugaritic can only have been /a/. As for the proposal that the morpheme consisted of vowel + consonant, it is rendered dubious by the case of {mrh}, ‘in Mari’, in RS 24.244:78 (KTU 1.100), i.e., one where a proper noun plausibly ends in a vowel, for the sequence /mariah/ is not permissible in West Semitic. 291 Since the accusative is the most commonly used case for the purpose of expressing the adverbial use of a substantive in West Semitic, this means that the suffix would normally have been affixed to a common noun in the accusative case in the singular, i.e. /STEM + a + h-/.

290 My thanks to Robert C. Hawley for pointing out to me the relevance of these forms.


292 T. makes a major place in this grammar for the locative case in Ugaritic (pp. 326-36 [§54.4]), and one might consider the possibility that the /-h/ was attached to nouns in this case. The fact that the Hebrew form of the particle is /-āḥ/ certainly speaks against such an hypothesis, however.

293 T.’s basic view of the morpheme as a true case historically related to the archaic dative -iš in Akkadian (a stance reiterated in UF 33 [2001], p. 626-29) is belied by Ugaritic, where the morpheme follows the -m of the plural (see below, remark to pp. 320-25 [§54.3]), and by the Hebrew data, where the morpheme is attached to a singular form ending in /-a/ (/’ārsāḥ/), to a dual form ending in /-aym/ (/misrāyamāḥ/), and to a plural form ending in /-im/ (/kašdimāḥ/). Of course he may choose to ignore these data (as he has done explicitly in UF 33 [2001] 628-29 n. 4) in favor of his hypothesis regarding the proto-Semitic origins of the form and specifically in terms of the Akkadian case system, but a more plausible reconstruction is possible (see continuation of this remark).
also, the vowel is a regular part of the /-ma/ element attached to both dual and plural nouns (pp. 289-90 [§53.213], pp. 293-94 [§§53.31-53.312]).

Examples are furnished by ārš, which does not consist of /'ārša + ah/, but of /'ārš + a + h(V)/, and by šmnh, 'to the heavens', which does not consist of /šamāma + ah/, but of /šamāma + h(V)/.

Because of other /š/ – /h/ correspondences between Akkadian and West Semitic, others before T. have been tempted to identify the Akkadian dative case marker with Northwest-Semitic -h, but it is necessary to remain dubious about the hypothesis for, as Gelb correctly remarked, “… the fact still remains that Hebrew ah is an enclitic which functions as a postposition placed after the inflectional elements, while Akkadian īš is a case …” T., on the other hand, has taken the Akkadian /-š/ morpheme as the model for West-Semitic /-h-/ without discussing the possibility that in proto-West Semitic the morpheme may have had a simple deictic origin. The only way that I can see to identify the morphemes in Akkadian and Northwest Semitic is to hypothesize that the Akkadian dative case was formed historically from the same deictic particle that came to be attached to Northwest-Semitic words after the number and case markers. If that is what happened, then the vowel after the consonantal element may have been /i/, for the dative morpheme on pronouns in Akkadian is /-ši(m)/.

This fact might account for the fact that the vowel dropped early in proto-Hebrew (as is seen by the facts that the /h/ has quiesced and that words bearing the morpheme are penultimately accented), for /i/ was more prone to drop than /a/, especially in word-final position. (I continue, however, to vocalize the particle as /-ha/ owing to the very speculative nature of the hypothesis and to the fact that deictic /ha/ is well attested in the Northwest-Semitic languages.) Finally, the fact that the accusative served as the adverbial case in West Semitic is sufficient explanation for the many locative nouns without -h. T. proposes on pp. 161 and 321-23 that in such cases the /h/ has quiesced. But the many ‘adverbial accusatives’ in Biblical Hebrew make the argument highly unlikely, for all these forms would also have once borne a consonantal suffix that has disappeared without leaving a trace—hardly likely, as is shown by the behavior of quiesced /'i/ in Hebrew (viz., it is usually retained in the orthography). It must be noted here that the Hebrew unmarked adverbs are all in some sense relics, since the case system had long since fallen from usage; all the more likely, then, that Ugaritic, where the case system was fully operative, should have made use of the device more extensively. T. does not discuss the phonetics of the form from which the /-h/ has putatively dropped, but, if it simply dropped with no associated change in the preceding vowel, the form would have in any case been identical with the simple accusative form

---

294 The evidence from syllabic writings of dual nouns occasionally shows /-mi/.

295 T. vocalizes this form /'āršah/, but this implies either that the /-h/ morpheme was attached to a vowelless nominal form (for which there is very little evidence in Ugaritic—see pp. 335-38 [§§54.5-6]), or that /'āršah/ and /ah/ have contracted to a single vowel (which may be expected to have resulted in a long vowel, for which there is no evidence whatever—Hebrew, with the penultimate accent on these forms, speaks directly against its proto-language having a long vowel between the nominal stem and the /-h/ morpheme).

296 Sequential Reconstruction of Proto-Akkadian (Assyriological Studies 18; Chicago, University of Chicago, 1969) 91-92.
(‘arṣah(V)/ > ‘arṣa/), the base form to which the locative form was originally added! For this reason it borders on the absurd to say that “der Term[inativ] im Ug. tatsächlich wohl häufiger belegt ist, als die Orthographie auf den ersten Blick vermuten läßt” (p. 323). It appears necessary to posit that the accusative case functioned as the adverbial case in West Semitic; that the deictic morpheme /h(V)/ affixed to such forms in Ugaritic and Hebrew marked this function more explicitly; that it could be attached to nouns of which the characteristic vowel was other than the accusative or to nouns in the genitive case in the very rarely attested cases of double adverbial marking; and that the use or non-use of the particle was a matter of style or emphasis.

— p. 152 (§33.132), p. 350 (§62.201). Ugaritic, Hebrew, and Aramaic all show a particular form of the number noun ‘ṣr, ‘ten’, that occurs only in certain of the cardinal numbers from ‘eleven’ through ‘nineteen’, viz., ‘šrh/‘ṣrh. As the vocalization with an /i/-type vowel in Hebrew and Aramaic (‘ēṣrēh) shows, there is no basis whatever for seeing in the [-h] of Ugaritic ‘ṣrh a secondary form of the feminine morpheme -t/. T. compares, of course, the Hebrew form, but limits himself to the consonantal skeleton, making no attempt to explain the šere before the /h/ in Hebrew. The origin of the form in these languages has yet to be elucidated, but claiming an irregular form in Ugaritic that does not correspond to the Hebrew form is hardly a viable solution. T.’s insistence on finding the Akkadian dative morpheme /iš/ in the Ugaritic adverbial {-h} (see preceding remark and below, remark to pp. 320-25 [§54.3]) might better have been directed to this form: at least Hebrew/Aramaic /‘eṣrēh/ and the Akkadian dative morpheme have an /i/-vowel in common, while the whole basis of T.’s analysis of adverbial {-h} as a dative morpheme is to be found in the apparent cases of West-Semitic /h/ = Akkadian /iš/ (pp. 151-52 [§33.131]). Furthermore, the exclusive use of ‘ṣrh with a number noun in the digits to express a number in the teens reminds one of the use of the preposition l in compound numbers (e.g., ārb‘ l ‘šrm, lit. ‘four to twenty’ = ‘twenty-four’). ārb‘ ‘ṣrh in this hypothesis would been ‘four to ten’, the latter relationship expressed by a relic of the true dative case.

— p. 153 (§33.134.1), p. 285 (§52.5c), p. 295 (§53.322.1a), p. 301 (§53.34). Whatever the solution may be to the intriguing similarity of Ugaritic pāmt, ‘times (i.e., repetitions)’, p’n, ‘foot’, and Hebrew pā‘am, which means both ‘time(s)’ and ‘foot’, the solution that T. proposes, viz., of taking Ugaritic pāmt as the plural of p’n, is insufficiently supported. Just as plausible as the hypothesis that the two Ugaritic forms represent a bifurcation of an original P‘M is the opposite view, that Hebrew represents a falling together of two distinct forms, P‘N and P‘M. The only argument of substance that T. adduces is that the dual of P‘M is not attested, that the phrase meaning ‘twice’ is tniid. That is correct, but three counterarguments to the conclusion that T. draws therefrom may be offered: (1) pāmt is attested only in the ritual texts, tniid only in letters; hence we do not know how the two-fold repetition of a sacrifice would have been expressed in ritual idiom; (2) that two forms of expressing repetition existed side by side is shown by the forms šb‘id (alongside šb‘d — see above, remark to p. 150 [§33.116.2], etc.) and pāmt šb‘/šb‘ pāmt, both meaning ‘seven times’, the

first in epistolary texts, the second in ritual texts;\(^{298}\) (3) in Hebrew, \(p\)‘\(m\) in the meaning ‘time (repetition)’ is attested in the singular, the dual, and the plural, whereas Ugaritic \(p\)‘\(n\) means only ‘foot’. The possibility must, therefore, remain open that Ugaritic \(P\)‘\(M\) could express both a single occurrence and a two-fold repetition. That being the case, the question of the ultimate relationship between \(P\)‘\(N\) and \(P\)‘\(M\) must also remain open.

— p. 153 (§33.134.1) contains a repeated datum: \{\(âbd\ . \(b\)‘\(l\)\} in RS 19.096:48 (\(KTU\ 4.635\)) is listed once in this form with the word-divider between the two elements of the personal name, again below in the same paragraph without the word-divider.

— p. 154 (§33.134.2), p. 251 (§51.41a). One of the more striking inconsistencies of this work is to be found in these two paragraphs. In the first \{‘\(b\)s\}/‘\(ps\} is said to come from a root ‘\(PS\) and not to be connected with \{â\(ps\\}, “Äußeres; Rand,” while in the second “‘\(b\)/\(p\)‘\(s\)” is said to be a “\(N\)eben\(f\)orm” of \{â\(ps\\}.

— p. 154 (§33.135.2), p. 251 (§51.41a). Because Ugaritic has two words, \(p\)‘\(n\) ‘foot’ and \(p\)±\(m\)-‘time’, as compared with Hebrew \(p\)‘\(a\)m, ‘time, footfall’ (see third preceding remark), one must remain dubious about \{\(p\)‘\(n\}\ having arisen as a simple dissimilatory phenomenon from \{\(p\)‘\(m\}\. It is in any case a bit simplistic to cite Hebrew \(p\)‘\(m\) as a cognate for the meaning ‘foot’ when the standard Hebrew word for ‘foot’ is \(r\)é\(g\)èl. One cannot help wondering if the Ugaritic lexical distinction is not older than the data attested in the other Semitic languages.

— pp. 154-55 (§33.135.2), p. 294 (§53.313), p. 301 (§53.34), p. 302 (§53.36). The only example of the masculine plural ending {-n} in alphabetic script claimed by T. is \{\(k\)\(\)m\(n\}\ in RS 17.106:4 (\(KTU\ 4.269\)), where the \{-n\} would represent dissimilation from the preceding \(/m/\). Because there are two other certain scribal errors in this text, \{\(m\)\(\)s\(\)m\(\)\} for \(m\)\(\)s\(\) in line 8 and \{\(s\)\(\)m\(\)\} for \(h\)\(\)m\(\)\ in line 35, it appears more likely that \{\(k\)\(\)m\(n\}\ is also a simple writing error without a clear phonetic basis.\(^{299}\) The error in line 8 consists of a dittography of \{\(h\\}, while that in line 35 has no clear basis, either graphic or phonetic, except in the sense that the third consonant of the word is \{-\}—hardly the basis for an explanation of the mistake as owing to assimilation, except in the very broadest of terms. It does not appear plausible, therefore, to explain \{\(k\)\(\)m\(n\}\ by a phonetic process, especially when one considers that the word in question is written \{\(k\)\(\)m\(n\}\ in its two other occurrences in this very text.\(^{300}\)

— p. 155 (§33.135.2), p. 255 (§51.41c), p. 272 (§51.46c), p. 290 (§53.214), p. 292 (§53.231), p. 327 (§54.415), p. 382 (§67.11). T.’s preferred explanation of the \(/n/\ in \(k\)l\(\)\(ā\)t\(n\)m\ as the consonantal element of the dual morpheme “\(ā\)m” which has undergone dissimilation from the following \(/m/\ is forced in the extreme. T. sees here the noun \(k\)l\(\)ā\(t\) + dual morpheme + locative suffix \(\(l\)\)\(\) + enclitic -\(m\). I see no reason to doubt that this is simply the noun \(/k\)l\(\)ā\(t\)’/
+ feminine morpheme /-at/) + afformative /ān/ + the standard dual ending, here in the oblique case, i.e., in the adverbal accusative construction: /kil’atānēma/, ‘in the pair (of hands)’.

— p. 155 (§33.135.3). In the sequence {gh’s m. lḥmd}, where {lḥmd} should be {mḥmd}, ‘choice’, the {l} is explained as dissimilatory: it has replaced the {m} because the two {m}s were “in unmittelbarer Umgebung.” There is no reason to doubt, however, that the first word ended in a vowel and the proximity was not, therefore, immediate. One may also doubt that a nonsensical word would have been created by dissimilation, for this consonantal sequence occurs many times, and the dissimilation occurs nowhere else.

— p. 156 (§33.137.1). The explanation of {qrwn} in RS 24.277:11’ (KTU 1.127) that is mentioned here only as an afterthought, viz., that it is a personal name, is, given the structure of this paragraph of RS 24.277, the most likely.301 That it would be a secondary form of a noun qrbn, ‘present’, the main burden of the paragraph in T.’s grammar, is highly unlikely.

— p. 156 (§33.137.2). One can only ask oneself what a paragraph on “*w > /b/ (??)” is doing in a grammar of Ugaritic when all examples are drawn from personal names of Hurrian origin and all examples are of the element that may be represented as either {iw} or {ibr}. The problem appears clearly to be one of representing a Hurrian phoneme that is embedded in particular names.

— p. 158 (§33.141.43a), p. 450 (§73.243.22a). As I observed in 1984,302 RS 16.402:16 (KTU 2.33) cannot contain a form of the verb ’HḥD, ‘to seize’: one indeed sees traces of what could be [z] or [ḥ], but those traces are followed by a {b}, not a {d}. The citation of this passage as containing the form {âḥd}, of which the {â} would in any case be entirely restored, thus has no textual basis.303

— p. 158 (§33.141.43a), p. 451 (§73.243.22c), p. 547 (§74.412.21), p. 613 (§75.212.13), p. 614 (§75.212.2). The problems of vocalizing the /YQTL/ forms of the verb ’RŚ, ‘to request’, are not easy to resolve, but one would wish at least for consistency of presentation, which one does not find in these sections. On p. 158, the 1 c.s. form {ârš} (RS 16.078+:18, 22 [KTU 2.23]) is confidently said to be /’ārūš/-/ ← /’a’rūš-/ but on the other two pages more caution is observed and the stem vowel is indicated as being /u/ or /i/ or is left unspecified. On p. 547, the imperfective and imperative forms of this root that are written with {i} are parsed as G-stem, whereas those that are written with {ā} are parsed as {D-stem}. The most pertinent aspect of this question is that the imperative form {irš} is well attested; this writing should represent neither the D-stem nor a G-stem imperative of the /qutul/ type, but /’iriš/.

Because, however, other /YQTL/ forms show writings appropriate for the D-stem (see pp. 451, 547, 613), this 1 c.s. form may be the same, viz., /’arrišu/ ← /’a’arrišu/. It must be

302 AfO 31, p. 218.
303 One finds the reading {z⟩b⟩} in KTU, which reflects well the traces on the tablet; that was changed to {⟩h⟩⟩} in CAT, a reading adopted uncritically by T. (on p. 158, he indicates by means of an asterisk that the form is partially reconstructed; on p. 450, he places the {ā} in square brackets but does not indicate that the notation in CAT identified the second and third letters as uncertain).
remarked at this juncture that the only possible occurrences of the 1 c.s. /YQTL/ forms of this root are those cited from RS 16.078+ and that the analysis there as a finite form is far from certain, for the independent pronoun ànk occurs in line 15, which opens up the possibility of parsing {àrš} as a G-stem participle. A further philological remark: T. treats {àrš} in RS 17.117:12 (KTU 5.11) as a certain case of an imperative form spelled with {â}, but that form may well be perfective\(^{304}\) (see further below on p. 425 [§73.121.1a]). Judging from vocalizations of the various imperfect forms offered throughout this work in which the {â} is interpreted as a mater lectionis for quiesced /ˈ/ (e.g., p. 458 [§73.273.2] “tārVšina”), this analysis of the use of the {â} is preferred by T., despite the fact that, in his principal discussion of the imperfect of this verb (p. 613), the most he can say for that solution is that it is “denkbar.” That he in fact prefers this solution is, however, further indicated by the fact that the root ‘RŠ is not even mentioned in the treatment of D-stem forms of I-‘ roots (p. 615 [§75.216]). I must say that T.’s orthographic solution barely deserves the qualification of “denkbar” and that the analysis of {i}-forms as G-stem and of /YQTL/ of forms of which {â} is the second sign as D-stem is far more plausible.

— p. 159 (§33.141.6). My collation\(^{305}\) confirms T.’s observation that {nqpt} in RS 19.017 i 11 (KTU/CAT 4.610 1 12) is a false reading for {nqpt}.\(^{306}\)

— p. 159 (§33.142.21), p. 226 (§41.222.4a), p. 244 (§45.122c), p. 822 (§88.24). As I observe below (remark to p. 533 [§74.32], etc.), nhtu in RS 4.475:10 (KTU 2.10) must be a finite form rather than a participle and inm is, therefore, most plausibly the negative particle in with reduplicated enclitic -m. T. explains this form in three ways: (1) in + -hm (3 m.pl. pronominal suffix) + -m (this order of the morphemes is stated explicitly on p. 822: “… das erste /m/ des PrS 3.m.pl. /-humu/ enthält’’); (2) a scribal error for inm; (3) inm (i.e. in + -m) + -m. Only the last of these explanations makes sense in context. T. correctly refutes the emendation, proposed in CAT, to {m<n>m}.

— p. 160 (§33.142.23), p. 540 (§74.35). In the first section cited, T. analyses wn árk in RS 3.362+ ii 20' (KTU 1.10) as containing the conjunction w expanded with -n, in the second as containing the N-participle of vN’R, with an unreadable sign after the {r}. In neither case is there a cross-reference to the other analysis.

— p. 160 (§33.142.3b), p. 201 (§33.412), p. 451 (§73.243.22b), p. 625 (§75.33). T. everywhere presents the G-imperfect and imperative forms of HLK, ‘to go’, as deriving directly from the root HLK, which, since the {h} is never present, involves irregular elision of that consonant. Judging from the corresponding Hebrew forms, which behave in every respect like /yaqtıl/ forms from I-Y roots (/yēlēk/ = /yērēd/, etc.), it appears more plausible to ascribe the differences to suppletive root forms, HLK and (Y)LK. (For a more detailed argument and a comparison of HLK and HLM, ‘to strike’, see below, remarks to p. 520 [§74.232.21] and to p. 625 [§75.332].)

---


\(^{305}\)Or 70 (2001) 251, 252, 260.

\(^{306}\)See also van Soldt, UF 28 (1996) 681 with note 229; Tropper and Vita, UF 30 (1998) 701.
— p. 161 (§33.145.1), pp. 774-75 (§82.411). In vocalizing the compound preposition bd, T. makes no attempt to explain why Old Canaanite had /a/ while the corresponding form in Phoenician/Punic had /º/. Unlike Rainey—and many other scholars—who normalize the Old Canaanite form /bād-/308 T. vocalizes /bad-. One may presume that he is thinking of the /a/ > /â/ > /o/ shift that is well known for Phoenician/Punic, but the last stage of that shift is normally attested in accented syllables.309 Does T. believe that the /bod/ element of proper names was accented? Whatever the case may be with the Canaanite form, one may argue on the basis of the proper name {bi-di-‘i-lu},310 which apparently consists of the elements /bi/ + /yadi/ + /‘il-/ and may have been pronounced something like /bîdi‘ilu/, that in Ugaritic the compound preposition was /bîdi/.

— p. 162 (§33.151a), p. 510 (§74.222.2), p. 511 (§74.222.2), p. 612 (§75.212.12), p. 615 (§75.213). In the first section cited, {tûsl} (RS 1.009A:5′ [KTU 7.41], RS 24.250+25 [KTU 1.106]) is presented simply as reflecting a root ‘SL where Arabic has WSL; in the second and fifth, the analysis of the writing as the use of {û} as a mater lectionis for /û/ is put forward (the form would be /tûšalu/ from /tušalu/, a G-passive /YQTL/ form); also in the second the second possibility is considered of analyzing the form as active, “tôšVlu ← “tašulu,” meaning “sie trifft ein”; in the third and fourth sections cited the form is simply said to be “G-PK.” The analysis as a mater lectionis must be judged highly unlikely because it is unrepresentative of the Ugaritic writing system and, in the hypothesis as an active form, assumes the shift of /a >/ to /û/. Either the {û}-sign may be considered occasionally to represent syllable-final /û/ preceded by /u/, as I assumed in my vocalized text of RS 24.250+ (i.e., it would be a G-passive and vocalized /tušalu/)311 or else this form represents another case (see above, remark to pp. 33-35 [§21.322.1]) of a secondary vowel introduced in harmony with the stem vowel (i.e., it would be 2 m.s. G-active and vocalized [tašulu]).312 T. would not, of course, accept the latter analysis as valid because he rejects the existence of 2nd person volitive forms in the ritual texts (see below, remark to p. 211 [§41.12], etc.).

— p. 163 (§33.152a), p. 164 (§33.154a), p. 249 (§51.3e), p. 250 (§51.3l). On T.’s hypothesis that the base form of the word for ‘water’ in proto-Ugaritic would have been /mâ‘-, see below, remark to p. 250 (§51.3l).

— p. 163 (§33.152b), p. 276 (§51.5c). T.’s very complicated reconstruction of nhmmt as a “/[qataltVI]” form appears unnecessary in light of the consideration that forms produced by reduplication of the third radical exist alongside those showing reduplication of the second and third radicals. T. argues against the latter on the basis of the /h/, which one may agree is secondary as the root was N(W)M, for, he believes, the /h/ shows that the form had two /a/

307 The Old Canaanite datum is {ba-di-ú} in EA 245:35.
309 Ibid., pp. 40-41 (§78a-c).
vowels, which would not be the case of a “qVtlVF” form. One may grant that /qatlal/ may have become /qitlal/ already in Ugaritic (though there is no proof of this of which I am aware) without admitting that the /h/ of the form in question arose uniquely in the word nhmmt owing to the presence of two /a/ vowels: NHM may be a Ugaritic by-form of the more common N(W)M, ‘to slumber’.

— p. 163 (§33.152c), p. 200 (§33.323.4b), p. 252 (§51.41a). Rather than seeing in {bhtm}, ‘houses’, a linear development from /bayat-/ (i.e., the hypothetical dissyllabic plural base of /bayt/) it is at least as plausible to see the function of the /h/ as effecting the triconsonantalization of the secondary form /bêt-/, for /h/ appears in that role elsewhere in Ugaritic and in the other Northwest-Semitic languages (Ugaritic {âmht}, ‘maidservants’, Aramaic /³bâhât/, ‘fathers’, /³râhât/, ‘to run’, etc.).

— pp. 166-68 (§33.17). Syllabic writings provide a notoriously precarious basis on which to establish theories regarding consonantal gemination, especially when there are conflicting writings, as in the case of ≤i¬-, “wood’ ({i¬-¬u(-[ma]} vs. {i-¬ú}). See above, fifth general remark.

— p. 169 (§33.181.2), p. 260 (§51.42j). The first paragraph cited is expended on the outside possibility that {pu-“ur DINGIR.MEŠ} in RS 20.024:28 might be Ugaritic. As several other entries in this text show (e.g., l. 4 {DIM be-el ḪUR.SAG.ḥa-zi} or l. 12 {dša-sú-ra-

313 T. does not appear to be particularly convinced of the dissimilation in formations of this general type, for he vocalizes šmal, ‘left’, as “/ša/im ‘al” (p. 276 [§51.61b]).

314 The other example cited by T., {qym} in RS 2.[024] i 5 (KTU 1.22), is no more convincing than this one, as T. recognizes on pp. 642-43 (§75.521d).

315 My colleague T. van den Hout has kindly told me of the existence of a place name spelled {se-e-ra}, a mountain located in Kizzuwatna where three texts describe the hisuwa- festival as being carried out. Because according to RS 18.040:15-16 the king of Hatti has gone to this place to offer sacrifices (w ūmny ydbh), the identification of SYR with that mountain appears plausible (Pardee, Context III [2002] 104).


317 Nougayrol, Ugaritica V (1968) text 18.
tu₄), the preferred strategy was not transliteration,³¹⁸ but translation. That posed no problem here, as Ugaritic and Akkadian had an identical word for ‘assembly’ and the translation of Ugaritic pʰr i_lm into Akkadian thus involved using the Akkadian, rather than the Ugaritic, inflection for the first word and representing the second logographically. The second paragraph cited then lists Ugaritic /puʰur-/ as a secondary /qutul/ base, derived from /qutl/. As there are no data from Ugarit in support of the other example cited in that paragraph (see below, remark to p. 260 [§51.42 j bkr]), the /qutul/ base for this word is at best purely hypothetical.

— p. 169 (§33.181.2), p. 254 (§51.41c). In the first section cited, {ši-i-ru} in RS 20.149 ii ³³¹⁹ is analyzed in two ways, as /ši≥ru/ (i.e., with syllabic /r/) or simply as /ši’ru/; in the second section, a third possibility is considered, that the syllabic orthography reveals a true disyllabification of the stem, viz., /ši’iru/. The two paragraphs are not cross-referenced. The Ugaritic orthography {šiir} allows any of the three vocalizations. Hebrew /š³ ’ēr/ does not speak against the analysis as a simple /qitl/, for II- ’segholate’ nouns sometimes show final forms with re-syllabified ’alif: the development may have been /ši’r/ → /ši(≥)r/ → /š(≥)r/ → /š@≥´r/.

— p. 169 (§33.182), p. 258 (§51.42b), p. 303 (§54.111). T. proposes that the Ugaritic place name {nḥl}, which is written {na-ḥa-li} in a syllabic text (RS 16.251:7),³²⁰ should mean ‘palm grove’, with cognates in Hebrew and Arabic that denote specifically the ‘date palm’. The area of Ras Shamra has never been known, however, for date production and one may doubt that a town was named after its grove of date palms. T. does not state why he prefers ‘palm grove’ over ‘stream’ (√N‡L) as an etymology (he correctly rejects nḥl, ‘to inherit, take possession of’, as being at the origin of this name—there is no reason to doubt the West Semitic origin of the name and Akkadian /ḥ/ is, in any case, usually represented by alphabetic {g} in loan-words). On the interpretation of tumrym as designating inhabitants of the town of [tmr], rather than date-palm workers, see below, remark to p. 273 (§51.46h).

— p. 169 (§33.183), p. 260 (§51.42h), p. 485 (§73.521). The classification of (ri-gi-mu) in RS 20.189A+B:³²¹ as a /qitil-/ form is open to the objections voiced above in the fifth general remark; /qitil-/ being a rare nominal type in West Semitic, it may be judged more likely that this combination of signs in fact represents /qitl-/ and that the word in question is in fact a common noun (‘word, speech’) rather than a verbal noun (‘speaking’; cf. Akkadian rigmu). (On this word in the context of problematic syllabic writing of Ugaritic words, see below, remark to p. 182 [§33.242a], etc.)

— p. 170 (§33.211.1). T. does not indicate that ūṯḥt, “Räucherbecken,” occurs in Ugaritic only as a divine name.

---

3¹⁸Even the case of {d.giški-na-rù} (l. 31) is not without ambiguity for, though kinnāru is not the standard East-Semitic word for ‘lyre’, it may once have been and in any case appears in western peripheral syllabic usage (see Archi, Eblaítica 1 [1987] 9; Conti, Quaderni di Semitistica 17 [1990] 160).

3¹⁹Nougayrol, Ugaritica V (1968) text 130.


3²¹Laroche, UF 11 (1979) 477-80.

Archiv für Orientforschung 50 (2003/2004) online version
— p. 170 (§33.211.1). T. fails to point out that the only certain occurrence in alphabetic script of the “phonet. Variante ištfr(y)” for the divine name “uštfr(y)” occurs in the Hurrian section of a bilingual text and that it is in fact the Hurrian form of the name, since it here bears the Hurrian dative suffix (RS 24.261:21 [ištfrd] [KTU 1.116]). For that matter, in Ugaritic texts the variant without [y] of the divine name normally written [uşqr] occurs only in the compound divine name uštfr ḥlmz (RS 24.260:2, 121). In the other text cited for the writing with [i] (RS 24.266:14 [KTU 1.119]), only the [i] is fully extant and the partially preserved following sign could be [l] rather than [§]. The upshot is that /ištfrara/ is Hurrian (and Akkadian), but not standard Ugaritic, and that the standard Ugaritic form of the divine name that corresponds to syllabic [DINGIR.ış-ḫa-ra] is [uşqr].

— p. 170 (§33.211.2), p. 202 (§33.23b), pp. 426-27 (§73.122), p. 540 (§74.342). §33.211.2 is another wasted paragraph: two examples of prosthetic /'/ before dental and sibilant consonants are cited here but neither is valid. The reading /itml/ is virtually impossible in RS 24.266:19 (KTU 1.119), while the analysis of /iṣn/ in RS 34.126:18 (KTU 1.161) as an N-stem imperative is to be preferred, and by far, over that as a G-stem, for the G-stem imperative does not show prosthetic /'/ in Ugaritic (neither of the other two examples cited pp. 426-27 is any more convincing than is that of /iṣn/ and the fact that the form would appear with different /'/-signs does nothing to help T.’s case—on /ibky/, see below, remark to p. 195 [§33.322.2c], etc.; on /uqr/, remark to p. 202 [§33.423a]

— p. 171 (§33.212c), p. 223 (§41.221.52c), p. 449 (§73.243.21). Two remarks are required with regard to T.’s reading of RS 17.434+:51 (KTU/CAT 2.37:9) as “ib’r.nn” (pp. 223, 449) and to his interpretation thereof. First, the reading is in fact /[...]. ib’r [.] nn. [...]/; therefore, however likely the reconstruction as verb + object suffix may be, it may not be presented as certain. Second, the meaning of B’R is not always clear, even when the context is well preserved, and the meaning of ib’r is uncertain in this and the other passage, also badly broken, where it occurs (RS 16.394:59 [KTU/CAT 2.31:55]). One would not guess this from the translation of RS 17.434+:51 (p. 223: “ich werde ihn/es vernichten”), though more uncertainty shows through in that of RS 16.394:59 (p. 449: “ich werde/will


324 Virtually the entire lower edge of a horizontal wedge is extant, with no trace of the small vertical necessary for [i] (see Pardee, Les textes rituels [2000] 663, 1285). Even if the reading were accepted, one wonders what the word for ‘yesterday’ (T.’s interpretation, cf. p. 202) would be doing in a prescriptive ritual text.


326 As we will see in the remark to /uqr/, the argument that the /ū/ is owing to vowel harmony with the stem vowel is implausible.

327 On T.’s system of reference to RS 17.434+ in relation to the presentation of that text in CAT, see above, remark to p. 10 (§19.2).
Another case of indecision. On B'R, see above, remark to p. 52 (§21.341.21a), where other translations are quoted of another passage.

— p. 171 (§33.213.1a), p. 257 (§51.41g). It is implausible to evoke "Diphthongkontraktion" in the case of assumed proto-Ugaritic /ḥawwatu/ represented in syllabic script as [ḥu-wa-tu₄].

for /ww/ does not behave like /wC/; ditto for proto-Ugaritic /ḥayyūma/ as [ḥe-yu-ma].

for neither does /yy/ behave like /yC/ (see T. p. 134 [§32.221.1], p. 172 [§33.213.3]). The first explanation offered here, viz., that the syllabic writing represents vowel coloring under the influence of the following consonant, is more plausible, as is even one that would posit developments from non-geminate forms. Without more data, however, it is impossible to determine whether the three examples of such vowel coloring cited in this paragraph are allophonic variants of the time of the scribe of the polyglot vocabularies or earlier and broader phonetic changes. For the case of iy, 'where?', see next remark. Finally, T. suggests (p. 172) that the retention of the third-weak consonant in forms such as {‘ly} or {‘rwt} reveals similar vowel coloring. The example of Hebrew, however, where the possible forms /qataya/qatiya/qatuya/ were clearly leveled out on the basis of the second form (/bânīyti/), offers the possibility that similar processes may have occurred in Ugaritic, i.e., {‘ly} may have been /‘aliya/, not /‘aleyə/, whereas {‘rwt} is probably a ghost form.

The end result would have been similar to that envisaged by T., but the process would have been one of retention of older forms rather than of production of new forms.

— p. 171 (§33.213.1b), p. 330 (§54.423a), p. 752 (§81.61a). For {iy}, 'where', two explanations are again offered (see preceding remark). Here, however, it is more likely, since we are dealing with a particle, that the Ugaritic form was actually a composite one. Because so many particles consist of a basic particle which has been expanded in various ways, it is perfectly plausible to explain the Canaanite and Akkadian particles cited on p. 752 as consisting of /‘ay + ya .../, as is clearly shown by Arabic /‘ayna/, i.e., /‘ay + na/. Thus proto-Ugaritic would have had a basic particle /‘ay/, which monophthongized to /‘ê/ and which was written [i] in Ugaritic. This particle was then secondarily expanded by the

328 Nougayrol, Ugaritica V (1968), text 137 ii 10' (not line 4', as is indicated on both pages cited).
329 See also p. 255 (§51.41d) and p. 301 (§53.34). The form is attested three times, once with {ḥe}, twice with {GAN} = {ḥe|hê}; again one of the references is incorrect: the line number in Nougayrol, Ugaritica V (1968), text 137, col. 1, should be 20', not 3'.
330 It is hardly plausible, however, to classify this vowel coloring under the rubric "[qat] ← *qatf" (p. 257 [§51.41g]), for "[howwat]" would not have been and would never have been /ḥuwwat-/, i.e., if the hypothesis according to which the syllabic writing represents the actual pronunciation of the word is correct, [howwat-] is a realization of /ḥawwat-/, not of /ḥuwwat-/. The same is true of {ḥe-yu-ma}: on p. 255 (§51.41d), this form is presented as /qitl/ ← *qat, but [ḥeeyy]' would not in fact have been /qitl/ but the realization of /ḥayy-/ as heard and represented by the scribe.
331 On p. 257, T. indicates a more plausible form of the hypothesis of diphthong contraction: /ḥawy-/ → /ḥêy-/ → /ḥêw-/; i.e., the contraction would have been possible because not blocked by the gemination of /w/. A parallel development for {ḥe-yu-ma}, viz., a proto-form /ḥayw-/, is not considered on p. 255.
332 On the place of {‘rwt} in T.'s system and the unlikelihood of the reading, see below, remark to pp. 653-54 (§75.531b), esp. note 1178.
333 On p. 199 (§33.323b) he mentions the possibility that the III-w forms may have been /qatula/.
affirmative particle /ya/, pronounced /êya/ and written {•y}. This explanation could account for the difference between this particle and ãy meaning ‘indeed’ (or ‘whatever’) more plausibly than T.’s suggestion that the difference is owing to the two words having been accented on different syllables.\textsuperscript{334} If {•y} represents /’ayy-/ and {±y} meaning “indeed’ (or “whatever’), more plausibly than T.’s suggestion that the difference is owing to the two words having been accented on different syllables.\textsuperscript{334} If {•y} represents /’ayy-/, it would, as expected, have retained the /a/ in the sharpened first syllable. (For another explanation of ãy and a criticism of T.’s explanation thereof as an indefinite pronoun/adjective, see remark below to p. 172 [§33.213.2], etc.) At the present state of our knowledge, the long form /’ayy-/ of the interrogative pronoun meaning ‘where?’ is found in Ugaritic only in the old Amorite personal names ãyãb and ãyãh, /’ayya’abã/ and /’ayya’aãh/, ‘Where is the father/brother?’, birth names that are given when the father or older brother is deceased at the time of a child’s birth. It seems plausible that ïy also appears in Semitic proper names, the clearest case being ïyb’l, ‘Where is Ba‘lu/the master’, to be vocalized /’êyaba‘lu/, and interpreted as a properly Ugaritic name built from the particle discussed here above.

— p. 172 (§33.213.1b), p. 195 (§33.222.2b). T.’s attempt to explain {âliy(n)} as representing a /’aqtal/ form with /ay/ going to /’ey/, whence the writing with {i}, is apparently based on the rarity of the /’aqtil/ nominal base in West Semitic. Considering that the standard proto-Ugaritic development is /ay/ > /ê/, it appears more likely that the form is /’aqtãl/, perhaps a secondary development from /’aqtil/ under the influence of the following /y/—though this is not a necessary hypothesis, for one cannot discount the possibility of an original /’aqtil/ form.\textsuperscript{335} As for the form posited by T., the very existence of /e/ as a short vocalic phoneme must be described as highly dubious. Note in this respect that, on p. 172, T. transcribes the form in question as “/’al’eyn,” as containing the phoneme, not the phone, /e/. The only proven form of the vowel in Ugaritic is the long /ê/ that results from the monophthongization of /ay/.\textsuperscript{336} For that reason, when transcribing names of foreign origin that are thought for comparative reasons to contain a vowel of the /e/ family, it is best to represent them as /ê/,\textsuperscript{337} i.e. as a vowel that is long but not the result of inner-Ugaritic diphthong reduction.

— p. 172 (§33.213.1b), p. 199 (§33.222.3b), p. 614 (§75.212.3), pp. 653-54 (§75.531b). It cannot be judged likely that átwr (RS 2,[008]+ iv 32’ [KTU 1.4]) represents a pronunciation [’atowa(t)]. As with /e/, there is no reason to suspect the existence of /o/ in Ugaritic and no reason to doubt that Ugaritians were capable of pronouncing the triphthong /awa/ with two tokens of /a/.

— p. 172 (§33.213.1b), p. 200 (§33.222.4c), p. 666 (§75.535). ‘ly in RS 2.[008]+ i 23’ (KTU 1.4) is presented in the first section cited as a case for the realization of the 3 m.s. /QTLa/ form as [qateya]; on p. 200, the phonemic representation is /’alaya/; on p. 666, the

\textsuperscript{334}As the /y/ would have been geminated in both words, according to T.’s hypothesis, the reason for a difference in accentuation is not obvious.

\textsuperscript{335}As is shown by Hebrew, where there is no /’aqtil/, but /*’aqtil/ and /*’aqtal/ exist alongside /*’aqtal/.

\textsuperscript{336}Brokelmann, Grundriss I (1908) 373-74, cites /’iqtil/ forms but no /’aqtil/.

\textsuperscript{337}See Bordreuil and Pardee, RSO XIV (2001) 374 n. 12.
preferred analysis is as a m.s. participle /‘āliy(y)/. (Apparently because of this decision, this reference is not included under the heading of /‘alay/ 3 m.s. /QTLa/ on p. 664 [§75.534], where the vocalization is given as /‘alay/.) In context, the analysis as 3 m.s. /QTLa/ is to be preferred\(^\text{338}\) and, as observed above, there is no basis for the assumption that the realization of the form was with /e/, a phone(me) for the existence of which in Ugaritic there is no evidence.

— p. 172 (§33.213.2), p. 241 (§44.31), p. 244 (§45.13). T. analyzes the two-fold āy in RS 2.002:6 (KTU 1.23) as an indefinite pronoun derived from the same basic form as the interrogative adverbs i and īy, ‘where’ (on these, see remark above to p. 171 [§33.213.1b], etc.), but provides no argument for why an indefinite pronoun (or adjective) would have developed from an interrogative adverb having to do with location. His description is as follows: “Als adj. Int[errogativ]p[ronomen] (der Personen- und Sachklasse) ist aus sprachvergleichenden Gründen an sich āy /‘āyy-/ zu erwarten. Diese Form ist im Ug. tatsächlich bezeugt …, fungiert aber als Indefinitpronomen” (p. 241). §45.13 is entitled “Das adjektivische Indefinitpronomen” and the passage is there translated as “Eßt von jeglicher Speise, trinkt von jeglichem gegorenen Wein!” ([lhm . b lhm 1. āy […] w šty . b ḫmr yn āy]). T. does not provide any comparative basis for his expectation that /‘ayy-/ should serve as an interrogative adjective/pronoun referring to persons or things, though he is apparently thinking of Arabic /‘ayy-. The cognate in Hebrew, on the other hand, has to do with location only. At least until more data appear illustrating Ugaritic usage,\(^\text{339}\) it appears preferable to take Ugaritic āy as an interjection cognate with later Hebrew /‘ōy/hōy/, viz. /‘āy(a)/.\(^\text{340}\) This same interjection may be seen in the name āymr, which appears as a nom de circonstance in the text recounting Ba‘lu’s battle with Yammu (RS 3.267 iv 19 [KTU 1.2]), which may be vocalized /‘āyamr̲i/, ‘Yes! Expel’, or, if the verbal form was /QTLu/, in an assimilated form which may have been something like /‘ayamrû/ (← /‘āy + yamrû/), ‘Yes! It will expel!’\(^\text{341}\)

— p. 172 (§33.214.1). I believe that skepticism is required with regard to T.’s acceptance at face value of syllabic signs with {e}, e.g., {me}, as proving allophonic pronunciation of /i/ as [e] in the environment of a resonant, /l/, /m/, or /n/. A total of seven examples are cited, each attested only once; one appears in a broken context and the first sign is damaged (\{1e\}-la-yi

---


\(^{339}\)It is clear at least from poetry that the interrogative pronouns could be used adjectivally: in RS 2.[014]\(^+\) iii 37’ (KTU 1.3), one finds mn ib yp’1 b’1 (mīna ‘ēbu yapa’a lē ba‘li/) ‘Who is the enemy who has arisen against Ba‘li’ (mn = the personal interrogative pronoun /mī/ ‘who’ + enclitic -na). Cf. below, remark to pp. 242-44 (§45.1) on the indefinite pronominal forms.

\(^{340}\)J. Gray, Legacy of Canaan\(^2\) (1965) 28 n. 8. The polarity of the particle is not exclusively negative (‘Woe!’), as the lexicographers have seen for Biblical Hebrew, and it appears likely that the tendency towards negative polarity is a secondary phenomenon.

\(^{341}\)Given the social function of the personal names āyāb and āyāb (see above, remark to p. 171 [§33.213.1b]) it appears unlikely that āymr is of the same formation (T., p. 752 [§81.61a]): ‘Where is the expeller?’ as a rhetorical question is the only possible analysis that would make any sense and even that is hardly satisfactory because the two names ygrṣ and āymr in this passage have the literary function of expressing positively the role of the weapons in the story.
[RS 17.147:5 = PRU VI 29]); in another case, the Akkadian word being glossed has almost entirely disappeared (RS 16.086:4 read by the editor as {⋯ du-r[i(?)]: ḫa-mi-ti} = PRU III, p. 138); the interpretation of another is open to question ({{le-e}—see above, remark to p. 52 ([§21.341.21c]); only in this case and in one other would the /e/ value be affirmed by the use of a following sign that is /e/-specific ({{HI-en-ni-šu} in one of the polyglot vocabularies, RS 20.123 ii 20' = Nougayrol, Ugaritica V, text 137). That the values of the syllabic signs were not unequivocal, especially when representing non-Akkadian words for which an orthographic tradition or local convention did not exist, is well known.

— p. 172 (§33.214.1), p. 249 (§51.3b). In view of the dubious cases just cited, it must be judged equally dubious that the writing {me-te} for 'two hundred' (RS 16.145:14 [PRU III, p. 169]) represents “[mê( ‘)t]” instead of /mî’t-/.

However that may be, T. decides at a later point (p. 289 [§53.212], p. 291 [§53.221], p. 358 [§62.51], p. 418 [§69.321]) that the Akkadian writing does not represent the Ugaritic form at all but is probably Middle Assyrian and hence useless for deriving therefrom Ugaritic phonology, morphology, and syntax.

— pp. 172-73 (§33.214.21), p. 249 (§51.3d), p. 256-57 (§51.41f). Given Akkadian ummu, 'mother', and Aramaic šum, 'name', it is difficult to accept that the like vocalization of these two words in Ugaritic would represent a tendency in that language for /i/ to become /u/ before /m/. The only other example cited is {rûm}, listed because of Hebrew rɔzem and Akkadian rîmu. Given the common variation in nominal vowel patterns from one Semitic language to another and the Ugaritic counter-example cited in §33.214.22,342 it is difficult to see these words as reflecting a tendency of the Ugaritic language. The case of šm, to which the entirety of §51.3d is devoted, is particularly weak if Testen's hypothesis that this is one of a few biconsonantal words that did not have a characteristic vowel in proto-Semitic is correct,343 for the secondary realization in the various languages was to be expected to show variation from language to language. Given the forms with /u/ in Ugaritic, Aramaic, and Akkadian, it is Hebrew, with its forms šêm, šêm, and rɔzem, that is the odd language out and it would, therefore, appear more plausible to posit a sound-shift in proto-Hebrew from /u/ to /i/ than the opposite in Ugaritic.

— p. 173 (§33.214.23), p. 175 (§33.215.21b), p. 260 (§51.42g), p. 808 (§85.4). Why should the original form of {im[t]} /ˈimitta/, 'of a verity', have been “*amitta’? The hatef-seghol in Hebrew /ˈemet/ indicates at least the possibility of a proto-form /ˈimitt-/ in that language as well.

— p. 173 (§33.214.3). Evidence for the "Lautwandel" of /i/ to /a/ before /r/ consists of two forms /ma‘šar-/ 'tenth', and /mamšar-/ 'sword(?). The first is supported only by Hebrew /ma‘ašër/, the second is paralleled by Akkadian /namšar/.

---

342T. actually cites two counter-examples, but the second does not qualify: in 'Anatu's title ybm l̲imm, the second word in the singular (an eventuality mentioned by T.) and consists of the divine name Lîμu (Pardee, Context I [1997] 251). The word for 'people' may or may not have been vocalized /lu‘mu/: as T. points out, the only datum presently available is the word {lùmm} in RS 24.257:2, where the division of the signs is uncertain (Pardee, Les textes para-mythologiques [1988] 168).

When Akkadian, Aramaic, and Hebrew all show the proto-form */immir-/ for ‘lamb’, it appears overbold to include Ugaritic {imr} with the certain examples (it is listed in the ‘certain’ category but with a question mark!) of /qV1ttV2:l/ going to /qV2ttV2:l/. Not only is there no direct evidence that the second vowel was long, but the shared form */immir-/* in four Semitic languages indicates that the vowel shift in the first syllable, if indeed the proto-Semitic vowel was /a/, may not have been an inner-Ugaritic phenomenon. T. argues that, because the Ugaritic words for ‘pig’ (”/hinnîšu/*”) and ‘bull’ (”/’îbbîru/*”) were both derived from /qattîl/ forms, so should be the word for ‘lamb’. But the only evidence cited for the other two words is Hebrew /’abbîr/, that does not mean ‘bull’, but ‘powerful animal (including the bull)’. It appears equally plausible, at least on the basis of the data adduced, to argue that the three animal names were all originally /qiṭṭîl/ in Ugaritic. If such is the case, there is not a single certain case of the putative sound change (the other claimed examples are the land designation /siṭbîr-/*, of uncertain meaning and etymology, and {ulp}, on which see second following remark).

Because of the broken state of the text, it cannot be considered certain that {±mrm} in RS 3.348 i 10 (KTU 1.20) means ‘lamb’. The object of the verb {litdlbh} may have preceded it, not followed it.

In the first two sections cited, {ulp} in RS 1.002:19 et passim (KTU 1.40) is classified as a noun of the type /qattîl/ → /qiṭṭîl/, in the third as a compound consisting of a conjunction, a preposition, and a noun (/’ô lê pî/*, ‘or according to the declaration of’). On p. 778, the interpretation as a common noun is mentioned, but only as preferred by ”viele Autoren,” not as so classified above in this grammar. On the meaning of /ûlp/ in RS 1.002 (KTU 1.40), see below, remark to pp. 777-78.

T. cites no evidence for {irby}, ‘grasshopper’, being a “Wanderwort.” The presence of both {bir} and {y} in the proto-forms of the corresponding word in Akkadian and Hebrew, as well as the plausible Semitic etymology (RBY, ‘be numerous’—only the presence of a vowel between the /r/ and the /b/ in Akkadian alloforms speaks against this derivation), lead to a first presumption that the word is Semitic.

T. cites RS 16.401:7’ 10’ (KTU 2.32) and RS 18.075:19’ (KTU 2.41:18); only in the latter text is the meaning/analysis of the form certain from context.

344T. cites the Akkadian and Hebrew forms, but omits the Aramaic. */immîrîyûn/* in Biblical Aramaic shows that Aramaic also has a short vowel in the second syllable.


346He cites RS 16.401:7’, 10’ (KTU 2.32) and RS 18.075:19’ (KTU 2.41:18); only in the latter text is the meaning/analysis of the form certain from context.
that I-y forms seldom show a trace of the /y/ in derived forms; all the more strange that that should occur in a root that in other Semitic languages appears as NTN/NDN. As regards the G-stem forms of this root, T. shows characteristic indecisiveness: in this same paragraph on p. 175 he vocalizes {•tn}, ‘I will give’, in RS 15.007:4 (KTU 2.15) as ‘/itim-/ < *’atin-’ whereas on p. 635 (§75.512) he vocalizes {•tn} as ‘/e(t)tin-/’ but indicates the regular 1 c.s. form as ‘/ətin-/’
— p. 175 (§33.215.21b). Given that the [útb] was one of the most expensive garments in the list in which the word has appeared (RS 18.024:11 [KTU 4.337]), it is unlikely that it was only a “Kleidersaum(?).” This Ugaritic datum agrees with the data from Mari, where the corresponding term designated a “habit de luxe” as compared with a “habit courant.”

— p. 175 (§33.215.22), p. 219 (§41.221.17a), pp. 248-49 (§51.3a), p. 338 (§55.12). However one may go about explaining that in the later West-Semitic languages the word /’ab-/ ‘father’, shows a short vowel (Arabic) or no vowel at all (Hebrew/Aramaic) after the /b/ in the singular absolute but a long vowel before suffixes (e.g., Hebrew /’əbîhu/ ‘his father’), accounting for the Ugaritic forms by position of accent (the noun in the absolute state would be ‘/’abu/’ while the pronominal form would be ‘/’abûhu/’—on p. 248, the construct form is also said to have a long vowel) requires further argumentation. Strangely enough, though on pp. 248-49 and 338 ḏb and ḏḥ are described in identical terms, the explanation of the different vowel length in the absolute state as based on accent is made only for ḏb (p. 175). However all this may be, neither does T. provide any evidence from within Ugaritic for the hypothetical difference of case vowel length in the absolute state as opposed to the construct and pronominal states nor does he state explicitly that it is the loss of what one might expect to have been a long case vowel in the later West-Semitic languages that is at the origin of the hypothesis. It may further be remarked that the word ḏb is quite rare in Ugaritic prose, and the absence of examples of harmony of the first vowel with the long case vowel, well attested for ḏḥ but primarily in prose (see above, remark to p. 51 [§21.341.21a], etc.), may, therefore, be owing only to this paucity of attestations of ḏb. On the other hand, T. identifies the vowel harmony in the various writings of ḏḥ as linked to the presence of the /ḥ/ (p. 176, §33.215.31), inapplicable for ḏb. The divine name {ilb}, plausibly vocalized as ‘/’ilu’ibî/ ← /’ilu’abî/ appears, however, to provide a case of vowel

347 The option indicated here by the /i/ in parentheses leaves the reader perplexed, for one has difficulty imagining a reduced vowel of the shewa type in a closed syllable, because a closed syllable is long by definition and, from the Hebrew perspective, the Massoretes recognized shewa and hatef-vowels only in open syllables.

348 Along with the ktn, the útb was valued at 3 1/3 shekels each, while the least expensive garment cost only one shekel. See Pardee, Syria 77 (2000) 41-56.


350 T.’s transcription of the case vowel of both ḏb and ḏḥ shows forms resulting from contraction (i.e., the circumflex accent); this is because he considers the roots to have probably been triconsonantal (see p. 248 and my remark here below to pp. 248-49 [§51.2-3]).
harmony involving the noun ãb³⁵¹ and that name is consistently so written with two tokens of {i}; but the origin of the divinity is unknown and the pronunciation may, therefore, represent a linguistic stratum of ‘Amorite’ different from that of Ugaritic. If the name does represent Ugaritic, it provides a bit of evidence for the noun ãb in the absolute state having a long vowel, for the harmonization of the first vowel with the second should only have occurred if the latter was long. For the time being, one must conclude that the reasons for the different orthographic conventions for ãh and ãb are uncertain, though the fact that ãdn has essentially replaced ãb in prose (see above, remark to p. 88 [§31.3]) is an adequate explanation for the paucity of attestations of ãb there. (It is not impossible is that the /b/ was doubled,³⁵² for ãh in the plural does not show vowel harmony because the plural stem was, in accordance with the Hebrew data, probably /ʼaḥḥ-/.) Finally, it must be observed that the difference in the orthographic traditions for the two words extends only to the representation of vowel harmony of the first vowel to the case vowel, for the absence of {-y} on ãb in the nominative singular with 1 c.s. pronominal suffix, claimed on p. 219 to be “offenbar,” remains unproven. Only two texts are cited there. In one (RS 2.[014]+ iv 54 [KTU 1.3]), the form in question is entirely restored—indeed, in CAT, one finds the form {ãby} restored! (For a discussion of similar claims for ãh, see above, remark to p. 51 [§21.341.21a], etc.). The ambiguity of the Ugaritic data and the difficulty of explaining the situation in later West Semitic require us to leave open the hypothesis that the masculine singular nouns ãb and ãh both had long case vowels in all cases and in all states. (On these nouns as possibly deriving from the base /qatal/ in proto-West Semitic, see remark to pp. 248-49 [§51.3a], etc.) — p. 175 (§33.215.31a), p. 261 (§51.43a). Given the double divine name ãrm-w-thmt and its Akkadian equivalents {d†UR.SAG.MEŠ u A †mu-û} (RS 20.024:18) and {d†UR.SAG.MEŠ ù dA.MEŠ} (RS 92.2004:29 [RSO XIV 22]), that is, ‘Mountains-and-Waters’,³⁵³ it is clear that thmt does not mean “Meerestiefe,” but ‘fresh-water deeps’, i.e., these waters have nothing to do with the ‘(salty) sea’, as the casual reader would understand “Meerestiefe,” but are those that underlie the earth and gush up as fresh-water springs. — p. 177 (§33.215.42), p. 179 (§33.231.21b), p. 201 (§33.411), p. 277 (§51.7), p. 278 (§52.12a), p. 301 (§53.34), p. 399 (§69.143.21b), p. 848 (§91.321c). The vocalization of the Ugaritic word bnš, ‘member of the royal service personnel’, is known from vocabulary texts to be {bu-nu-šu}, but its origin is obscure. It occurs only in prose (letters, legal texts, and administrative texts). T.’s acceptance of the old etymology as from “*bin ‘Sohn’” and “*unāš ‘Mensch’” (p. 177) inspires no confidence whatsoever. The word is unknown in Canaanite, so the presumption must be that it is Ugaritic; but in proto-Ugaritic the first element should have had a case vowel, which would have impeded the assimilation of the /n/. Moreover, there is no basis for the shift of the second vowel from /ä/ to /ü/ or /ō/ in

³⁵²Cf. Aramaic ʿabbā.
Ugaritic (T. considers both vowels possible); this also would have to be a Canaanite feature of the noun (see second following remark). In addition, the first vowel would have become /u/ only by harmony with this hypothetical /ü/, for there is no evidence that the Ugaritic word for ‘son’ was /bun-/ (on T.’s treatment of this word, see below, remark to p. 249 [§51.3b], etc.). Finally, the word does not have the broad meanings “Mensch, Person, Bediensteter,” as he glosses (p. 177 et passim), but much mores specifically designates individuals who are in royal service (perhaps rarely to individuals who are in service to other socio-political entities)—there are very few texts that cannot be so interpreted. A further negative point is that bnš is strictly a prose administrative term while nšm is well attested in poetry with the meaning ‘people’. There is no proof of which I am aware for the assertion that bnš is in some sense the singular of nšm, ‘people’ (p. 301); indeed, the fact that bnš is attested as a plural (and [bnš] in construct may, according to context, be a plural) goes directly against such a categorization and against the categorization as a “Nomen generis” (pp. 278, 399). Four further remarks on the data that are cited as comparisons in these sections: (1) The Ugaritic form of the word cited as “*unāš” is actually attested in Ugaritic in two forms, {inš ılm}, a divine name that may in form be a collective referring to the deceased, and {inst}, a word that appears in lists of royal personnel; the vocalization of the Ugaritic word is, therefore, very likely /ʼināš/-; neither form appears in T.’s index of Ugaritic words. (2) Ugaritic nšm, ‘people, humans’, may be cognate with Akkadian našû, ‘people’, and Hebrew nāšîm, ‘women’ (cf. nāšîm, ‘men’), rather than with the aphaeresized forms of /ʼināš/- cited by T. (i.e., Aramaic nāš and Arabic nās). (3) Whatever the precise etymology of Ugaritic nšm may be, the phrase bn nšm in RS 92.2014:10 (RSO XIV 52) provides no direct evidence for the origin of bnš, for it simply means ‘son of a human’, i.e., ‘member of the human race’; in that incantation text, it certainly does not have the meaning of ‘member of the royal service corps’ characteristic of bnš. And, of course, it does not show the assimilation of the /n/ of bn to that of nš that is thought to have produced bnš. (4) bnš is never divided into two lexemes by the use of a word-divider (and one wonders where the word-divider would have marked the division), so it is improper to refer to a phrase ‘X bnš’ as a possible triple construct chain (p. 848)—even if the proposed etymology were to turn out to be correct, reference to a triple construct chain would still be out of order, for there is

---

354 A possible example is {rīšym, d. ‘rb | b. bnšhm}, ‘Men of the town of Raʾšu who have guaranteed their bnš-personnel’ (RS 18.035:1-2 [KTU 4.347]). The text provides no indication of the sense in which these bnš-personnel ‘belonged to’ the men of Raʾšu, viz., were they in the personal service of these men, were they in service to the municipality, or were they royal functionaries in service in this town?


356 Here bnš is categorized as a “Nomen generis (Gattungsnomen),” but T. only cites one text in favor of that interpretation, RS 15.083:6 (KTU 4.169), where bnš is contrasted with ššwm, ‘horses’. The ‘man’ may, of course, be a member of the royal service personnel. Without further proof, one cannot accept that bnš was used as a collective with anywhere near the frequency of npš, which is cited as a comparative term. It is certainly not the usage of bnš in the singular with number nouns in the teens that will prove the categorization, for that morpho-syntax is well attested with other nouns (see p. 399, “Sonstige Fälle”).


no evidence whatever that bnš functioned in Ugaritic as anything but a three-syllable common noun. In sum, no etymological explanation of bnš is at this point anything other than purely speculative.

— p. 177 (§33.221), p. 499 (§73.611.2). The hypothesis that the Ugaritic energetic ending /-anna/ would have become /-anni/ after the dual morpheme /-ä-/ is based on no Ugaritic data, only on Arabic.

— pp. 178-80 (§33.231). This section is devoted to the “Lautwandel *ä > /ö/ ?” and as such is one of those that could have been relegated to a footnote, were footnotes used. T. first cites seven syllabic forms, six alphabetic forms, and several proper names, written both syllabically and alphabetically, that do not show the shift. One wonders why these data were not presented above in the section on the phonology of vowels (pp. 134-35 [§32.2]), where the refutation of the /ä/ → /ö/ shift could have been done as an Anmerkung. Then six data or types of data are cited in favor of the shift (traditionally known as the ‘Canaanite shift’ because it is attested in Hebrew/Phoenician but not in old Aramaic): (1) proper names with {u} in syllabic script; (2) {bu-nu-šu} (see remark above to p. 177 [§33.215.42], etc.); (3) {yšūl} for /yiš’-al-/ in RS 17.117:2 (KTU 5.11); (4) {ünk} for /änäku/ in line 3 of the same text; (5) {tüt}, ‘ewe’ or ‘ewes, in RS 15.072:3 (KTU 1.80); (6) {škn} and {šknt}, each attested once (RS 8.208:3 [KTU 4.36] and RS 15.012:2 [KTU 4.135]), for {škn}, ‘prefect’. Though all these forms have been cited by one author or another in favor of the hypothesis that Ugaritic occasionally shows the /ä/ → /ö/ shift, T. needlessly legitimizes such claims by devoting so much space to them and, in the case of bnš, by devoting a section above to an etymological explanation wherein the /ä/ → /ö/ shift is considered a valid explanatory device. Briefly: the names in (1) are probably non-Ugaritic; bnš is not plausibly derived from /binu ≥inªßi/; {yšūl}, {ünk}, and {tüt} all occur in texts that show non-standard features and these are plausibly aberrant writings rather than phonological features of standard Ugaritic;359 there is no basis to the hypothesis that {š} was always followed by /u/360 (let alone /ö/!).


---

359 As T. admits on p. 616 (§75.222): “Wahrschein. handelt es sich jedoch um ein rein orthographisches Problem” (with respect to yšūl in RS 17.117:2) and “ebenfalls {u} statt {a}” [he means, of course, {ā} for {ã}] (with respect to ūnk for ūnk in line 3 of the same text). In the case of RS 17.117, {ã} is used in other cases which appear to be phonologically identical (see Pardee, Les textes épistolaires [in preparation], ad loc.).

360 As T. has shown elsewhere (UF 27 [1995] 505-28) as well as here (pp. 40-50 [§21.33]).

361 This case provides another example of the predilection mentioned above to offer multiple hypotheses: on p. 178, the form is presented with no question mark and no cross-reference as a /qutal/ base, whereas in the section on /qitl/-base nouns (p. 254) it is presented as an example with no cross-reference back to §33.231.1b; on p. 486, it is presented as a /qitl/ base, with the possibility of a /qutl/ base, but with no cross-references to other explanations. Though the passage is translated eleven times in the grammar, never is the possibility of taking {šalm} as referring to the ‘inquirers’ rather than to the ‘inquiries’ even mentioned.
original editor thought,362 it may mean ‘investigators’ (a nomen professionalis of the form /qattāl/363). This interpretation appears to be confirmed by the correct reading of RS 17.073:2 (KTU 6.25) as {šal[...]}, rather than as {šab} (KTU/CAT).364 This label seems to have been attached to a list of the names of the ‘investigators’ operating in one of the towns of the kingdom of Ugarit.365 In any case, it is not the phrase {šil šlmy} in RS 19.029:8 (KTU 2.63) that will prove that an abstract /qitl/ form meaning ‘inquiry’ existed in Ugaritic, for the text is badly damaged and the word written {šil} may be the G-stem active participle, ‘a person who inquires’. With the preceding line, the phrase is {(7) lm [...] likt (8) šil . šlmy}, which is as plausibly interpreted as ‘Why have you not sent someone to inquire after my health?’ as ‘Why have you not sent (a letter) to inquire after my health?’. The problem of two different bases functioning to express the abstract notion of the verb is also one to elicit hesitation. If {šil} in the last text discussed may plausibly be /qitl/, or /qutl/, there is no particular reason to attempt a like explanation for {št l(m)}, i.e., to propose the existence of a /qutl/ base from this root having the same meaning as the /qitl/ (or /qutl/) base noun.

— p. 179 (§33.231.22a), p. 208 (§41.112.11b). Though T. is certainly correct in taking ūnk in the phrase written {hn ūnk bnk} in RS 17.117:3 (KTU 5.11) as a form of the 1 c.s. independent personal pronoun (normally {ānk}), the writing itself is more likely an aberrant use of the {’}-signs than a reflection of phonology (see comment above to pp. 178-79).366 Moreover, the phrase does not mean “Ich [bin] dein Sohn”; rather, since the letter clearly had two authors, the use of the personal pronoun is, as in RS 8.315:13 (KTU 2.11), a device used by one of the authors to preface a statement coming from him/her to the exclusion of the other author.367 The line is thus to be translated, ‘As for me, your son, I …’.368 This understanding of the structure of the text and the writing {yšūl} show that the {ū} of {ūnk} is no basis for seeing in that word an uncorrected error for {bnk} (the explanation put forth by del Olmo Lete and Sanmartiń369).

362Virolleaud, PRU II (1957) 188-89, text 161.
363The {ā} shows that it is not /qātil/, as the editor thought (ibid., p. 214). A vocalization is proposed for the translation in the remark below to p. 230 (§42.4).
364Here again the editor’s reading is to be preferred (Virolleaud, ibid., p. 193, text 172). The reading in KTU/CAT has been adopted, though with some hesitation, by van Soldt, UF 21 (1989) 379; without question by T. (p. 263 [§51.44c]; p. 472 [§73.411]—see remark below).
366On p. 179, T. proposes that {ūnk} may represent “[ʿonōkV]” and {yšūl} (l. 2) “[yišǒl],” but, in other cases of similar phonetic sequences in RS 17.117, the {ā} sign is used (see Pardee, Les textes épistolaires [in preparation]).
367One may note a similar use of āmtk in RS 29.093:12 (KTU 2.70) and of ‘bdk in lines 26 and 29 of the same text.
— p. 179 (§33.231.22), p. 426 (§73.121.1c), p. 449 (§73.243.1), p. 467 (§73.332.4), p. 616 (§75.222), p. 734 (§77.413), p. 778 (§82.421). Apparently as a device for explaining various aberrant phenomena in RS 17.063 (KTU 5.10) and RS 17.117 (KTU 5.11), T. sometimes refers to them as school texts, but in an inconsistent fashion: “Schultext” (p. 179), “Schultext” (p. 426), “Schultext” (pp. 426, 449, 467), “ein sogenannter Schultext” (p. 616 with reference to RS 17.117), “Übungstexten” (p. 733), and “Übungstext” (with reference to RS 17.117). As I have observed several times,370 orthographic and grammatical irregularities are not necessarily valid criteria for classifying a text as an exercise; in the cases of RS 17.063 and 17.117, both classified as school texts in KTU and not removed from that category in CAT, these texts show internally coherent features that are not known from other Ugaritic epistolary documents and the texts cannot, therefore, be products of a known school at Ugarit. They cannot be classified as school texts, then, unless one be willing to consider that the ancients were sending school texts—which by their nature as such have no inherent value—from one town to another. When one considers that these two texts in fact have all the characteristics of perfectly good letters, it becomes truly astonishing that the school-text hypothesis has received any serious consideration at all. (On T.’s analysis of several of the verb forms unattested elsewhere in Ugaritic, see remark below to p. 467 [§73.332.4].)

— p. 179 (§33.231.22b), p. 185 (§33.243.15), p. 199 (§33.323.3a), p. 258 (§51.42a), p. 284 (§52.5a), p. 297 (§53.322.4). The identification of ūṭ, ‘ewe’, as from the root TʿW and of the type /qatalat/ (assumed or stated in the first four paragraphs cited) is far more plausible than taking the [t] as a root letter (rather than the feminine morpheme—only on p. 284 is this analysis assumed). All the comparative evidence is against the latter derivation.371 The plural ūṭṭ in RS 24.247+:1 (KTU 1.103+),372 the basis provided on p. 284 for not taking the [-t] as the feminine morpheme, is either a scribal error or an example of the feminine plural morpheme being attached to the feminine singular stem (as, for example, in Hebrew dēlet, ‘door-panel’, dʾlāṭṬūṭ/‘door-panels’)—this explanation is explicitly proposed on p. 297.

— p. 181 (§33.232), p. 264 (§51.44g), p. 533 (§74.32). The theoretical basis for taking bd[t as meaning “Vertreter, Ersatzleute” is faulty. The data from Ugarit, Ebla, and Arabic lead rather to the conclusion that the term designated some kind of commercial agent, more of a ‘quartermaster’ than a ‘deputy’ or ‘substitute’.373 Regarding the difference in vocalization between the Ugaritic and Eblaite words ([bidal-] vs. [badal-]), T.’s primary hypothesis, that the Ugaritic form shows a shift of /a/ to /i/ is indubitably less likely than his second, viz., that


372For the reading [ṭāṭ], see Pardee, AFO 33 (1986) 118, 119-20, 126; idem, Les textes rituels (2000) 536-37. T. cites this plural form on pp. 179, 284, and 297; in the first two instances he rejects the reading [ṭāṭ] found in CAT without stating his reasons while in the third he does not mention the reading in CAT but cites a review of mine (JAOS 113 [1993] 615, 617) in which I criticize the first proposal of the reading [ṭāṭ] by Dietrich and Loretz (which was published in 1990, in Mantik in Ugarit).

373See Pardee, Semitica 49 (1999) 51-52, n. 78.
the word appears in the two languages in two different forms, /baddül-/ at Ebla, /bidül-/ at Ugarit.

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

— p. 182 (§33.242a), p. 183 (§33.243.11a), p. 185 (§33.243.2), p. 253 (§51.41a), p. 294 (§53.312c), p. 295 (§53.322.1a,b). When the same scribe in the same text, at two lines distance (RS 19.042:11, 13 [PRU VI 79]), writes the very same form in conflicting ways, viz., {ḥ[a-]am-ru-ú} and {ḥa-ma-ru-ú}, while other scribes write plural forms of the same word with {AM} and {MA} (p. 182), one begins wondering about the validity of drawing conclusions about vowel syncope on the basis of such writings (the first form is cited as evidence for “Posttonische Vokalsynkope in viersilbigen Formen” [§33.243.1]). {ma-ās-wa-tu} in RS 19.026:2 (PRU VI 113) and {ma-sa-wa-tu} in line 5 of the same text, parsed as identical forms (p. 253, 294-95), provide no relief. Then one encounters {ṣa-fan1-tu4}, ‘year’, where the proto-Ugaritic form cannot have been /šantu/ (see above, note to p. 146 [§33.115.44.5], etc.), but the writing is cited as evidence for “Posttonische Vokalsynkope in dreisilbigen Formen” (p. 185), followed by another case of different writings of a selfsame word {iš-tu4} and {i-ši-t[u4]}, ‘fire’ (in RS 20.201G+:8' and RS 20.189:37', respectively).374

---

374 Texts published by Nougayrol, Ugaritica V, texts 131 and 132 (the second republished with an additional fragment by Laroche, UP 11 [1979] 477-80; the reading of {iš-tu4} is credited to van Soldt, Bob 37 [1990] 732, 733). T. obviously does not know what to do with {i-ši-t[u4]}, since he notes the proto-Ugaritic form as “*iši/atu” and proposes with a question mark that /išitu/ may have developed from /išatu/ through vowel harmony (cf. p. 249 [§51.3b]); but since the vowel of the feminine morpheme usually does not
The case of {ša-šan-tu₄} shows that the result of the “syncope” in question cannot have been /Ø/, that it must not have gone beyond vowel reduction of the type that produced “vocal shewa” in Hebrew (i.e., one that was still vocalic enough in pre-Massoretic-Hebrew to have maintained the aspirated quality of a following b’gadk’hat letter, e.g., */malakay/ → */mal’ê/ in the Massoretic tradition). Another example is provided by the plural of /qatl/qitl/qutl/ nouns (see below, remark to p. 251 [§51.4l], pp. 295-96 [§53.322.1]). Conflicting writings by the same scribe make one wonder, however, if, when writing Ugaritic words, they were not capable of using {VC} and {CV} signs interchangeably, e.g., {MA} and {AM}, {AN} and {NA}, {IŠ} and {ŠI}, and CVC signs for CV + CV. If so, ‘apprentices’, was pronounced /§amarûma/ in Ugaritic, ‘fire’ was /‘ištu/ (or even /‘išatu/), and ‘year’ was /§anatu/. A study needs to be undertaken of proper names in syllabic script and conclusions drawn from orthographic variation apropos the validity of drawing narrow conclusions regarding Ugaritic phonology from such variant forms.

— p. 182 (§33.242a), p. 273 (§51.46h-k). T. twice mentions the syllabic attestations of the gentilic ‘Ugaritian’, which is {u-ga-ar-ti-PI} (RS 19.042:15 [PRU VI 79]; the same writing but without the final {PI} sign for /ya/yi/yu/wa/wi/wu/ follows in line 16, apparently a simple scribal error), and in the first paragraph he vocalizes it as “‘ugart≠yu.’” Neither here nor in his previous brief study of this word did he broach the topic of why the name of the city of Ugarit would end in -/it/, a morpheme unknown in Northwest Semitic. Moreover, neither he nor van Soldt, who first observed the implications of the syllabic writing for the pronunciation of the gentilic and of the toponym, go seriously into the question of etymology. Van Soldt does, however, qualify the writing {A.GÀR-ít} in RS 16.162:23 as “popular etymology.” Popular etymology it may well be, for it is well known that a place name is often more archaic than a given ethnic/linguistic group that inhabits it. But whatever the origin of the name may have been, it is not implausible to believe that the etymology underlying the writing in RS 16.162 reflects the truly popular etymology, i.e., that of the people of Ugarit, and hence that the pronunciation was derived from *ugāru, ‘field’. The existence of the noun in Ugaritic is attested by the double divine name Gapnu-wa-*ugāru, ‘Vine-and-Field’; the association of two agricultural entities in this name shows also that the

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

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

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


meaning' of the second was known despite the fact that the standard word for ‘field’ in Ugaritic was šadû. The form of the gentilic leads to the conclusion that the ending of the name was /-it-/ while the etymological consideration leads to the conclusion that it should have been /-ît-/.

Is there a way to harmonize the two conclusions? Yes. The data gathered by van Soldt for the syllabic orthography of the name indicate that in the vast majority of occurrences the name is registered without a case vowel; van Soldt lists only five exceptions.381 Working from the etymology and these writings with a case vowel, I have in the past assumed that the basic form was /'ugārîtu/ and that the noun was, because of this long vowel in the penultimate syllable, diptotic,382 hence, for example, /ba'lu 'ugārîta/, ‘Baal of Ugarit’.383 The gentilic may be taken to demonstrate, however, that it was the ‘absolute’ case that was standard and that the syllabic writings represent the standard pronunciation at Ugarit, viz., the last syllable was closed and the vowel of the historic morpheme /-ît-/ was hence secondarily shortened. The absolute form would, therefore, have been pronounced /'ugārit/, as van Soldt proposed in footnote 21 to his list of attestations, though his main entry is “‘Ugarit.”384 And, as T. proposed and van Soldt accepted in principle, the /ā/ would have shortened in the gentilic form because its syllable became closed by syncope of the /i/.385

The historic form of the name would, therefore, have been /'ugārîtu/, the form actually used in the late 14th and into the early 12th c. B.C. would have been /'ugārit/, whereas the gentilic would have been /'ugartiyyu/. What I add here to the proposals of van Soldt and T. is an explanation for the origin of the uncharacteristic /-it/ ending.386 As we have seen and will often see below, T. tends to overlook the principle of vowel shortening in closed syllables; this appears to be another instance thereof.

— p. 182 (§32.242b), p. 183 (§33.242.11b), p. 258 (§51.42a). One must query whether rbt is simply a secondary form of rbbt; since Hebrew has the same two forms, they would appear at least to go back to proto-West Semitic.

— p. 183 (§33.242b), p. 184 (§33.243.15), p. 189 (§33.311.3a, b), p. 204 (§33.444), p. 259 (§51.42c). Given that Hebrew has two basic forms, qéret (← /qart/) and qiryāḥ, meaning ‘city’, and that {qrt} /qart/ is well attested in Phoenician, how likely is it that Ugaritic {qrt}
and \{qryt\} reflect an identical base form? T. hypothesizes the base form to be /qariyat-/ i.e., \{qryt\} would represent a form in which the original triphthong /qariyat-/ has not undergone syncope and monophthongization because it is in “Pausalstellung” (p. 184), is a “Pausal-Var.” (p. 189) or is “in der Pausa” (p. 204). Because there is virtually no hard evidence for the existence of ‘Pausalformen,” it appears more plausible to posit that \{qryt\} would represent the base form /qaryat-/ while \{qrt\}, /qar≠tu/, would come from /qariyt-/. The writing \{qrt\} could, of course, also represent a separate form /qartu/ equivalent to the Hebrew and Phoenician forms, a direct descendant of the biconsonantal root QR.

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

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

387The reconstruction is based on the syllabic spelling \{qa-ri-t[4]\} in the polyglot vocabulary RS 20.149 iii 18’ (Nougayrol, Ugaritica V text 130).
388UF 17 (1986) 402.
389T.’s claim, p. 525, that the tD forms in the later languages are often secondary, derived from the Gt, stands in need of proving and, in any case, may not be used to deny the existence of any given tD form in Ugaritic.
390Cf. Pardee, UF 16 (1984) 252, n. 7, pointing out the division of the signs into two words in KTU (\{tšt il\}), a division abandoned in CAT.
The restoration of the verb סר in RS 16.401:10 (KTU 2.32) as [[י]ש[ית]] is not possible, for the right point of the sign preceding the {t} is preserved and is part of a horizontal wedge, not of the right wedge of {§} which in this text has a pronounced upwards slant (i.e., is inscribed as an oblique wedge rather than as a horizontal wedge). Though the restoration of the same verb in RS 34.124:42’ is certainly plausible, the absence of the preformative consonant is crucial, making it impossible to know whether the word was here {יִשתִר}, as in all other attestations.

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

Ugaritic and comparative data indicate that {r‘pt} in RS 24.252:8 should not be a participle, for that evidence indicates the D-stem for R‡P, ‘to soar’, of which the participle would be {mr‘pt}. On p. 551, T. suggests that {r‘pt} is either a G-stem participle or to be emended to {mr‘pt}, neither a particularly appealing solution. If {r‘pt} is not a participle, (dit) earlier in the same line should not be either (in the other paragraphs cited, T. analyzes {dit} as a G-stem participle). {dit} is easily analyzable as a noun, perhaps /da’tu/, ‘the bird of prey’, while {r‘pt} may also be nominal or adjectival, perhaps /raḥḥāpatu/, ‘the (feminine) soarer’.393

T. reconstructs III-y/w feminine substantives with /i/ in the penultimate syllable, e.g., /qatil/ or /qātil/ verbal adjectives or the number noun ‘eight’, in terms of vowel syncope, viz., the proto-Ugaritic /-at-/ morpheme.
would have lost the /a/ depending on the structure of the stem preceding this morpheme. He
goes so far as to write a special rule for the feminine form of the number noun ‘eight’ (p. 184:
in five-syllable words with a long vowel in the second syllable, the /a/ of /-at-/ drops). This
obviously was not the case, however, in proto-Hebrew, for the Hebrew form is š²mônâḥ, and
one wonders how T. knows, in the absence of any explicit data, exactly what the Ugaritic
form was. Indeed, Hebrew shows three clear patterns, one with /ît/, apparently derived
from proto-forms with the /-t/ form of the feminine morpheme, e.g., rē( ’)śîyt ← /riʾšîyt-/, ‘beginning’, and two others derived from proto-forms with the /-at/ form of the feminine
morpheme: (a) bôniyyâḥ, ‘she who builds’, with secondary lengthening of the /y/ in the
Massoretic tradition, and (b) yâpâḥ ← /yapiyat-/, ‘beautiful’, or s²mônâḥ ← /tâmâniyat-/, ‘eight’. What attracts attention in the last type is that these forms show no trace of
contraction, viz., /-yat-/ has not gone to /-ât-/ in proto-Hebrew, for the result would have
been /-ôt/ in Hebrew, but to /-at-/:  T. indicates both /-iyt-/ and /-iyat-/ as becoming /-ît-/ in
Ugaritic,394 and indicates others as showing no contraction (e.g. {qryt} = /qariyat-/) but his
criteria for syncope of the /a/ of the feminine morpheme are inconsistent, as we have seen
(remark to pp. 182-85 §§§33.242-243, etc.). Such reconstructions must be judged unlikely,
both from the theoretical perspective and from the comparative one.

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

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

394Nowhere in the section on triphthongs, but at various points further on in the grammar: e.g., p. 259,
§51.42f “hrt /hîrît/ ← *hiriyt ‘Schwangerschaft’, p. 348, §62.181 “tîmît /tâmânît-/, *tâmâniyat-/.”
Hebrew ‘ammiḥaḏ). T. recognizes that this phonetic change in geminate roots is not an inner-Ugaritic phenomenon, for he describes it as “schon vor-ug.”; that being the case, the proto-Ugaritic development should have been noted, if at all, under morphology (i.e., as /qall/ ← /qal Vil/), rather than under phonology. As for the rest of this paragraph, the ambiguous and conflicting data from syllabic writings (see above, remark to p. 182 [§33.242a], etc.) and the dubious nature of the two final entries (‘ib /ēbu/, ‘enemy’, would be from ‘ayibu/ or even ‘ayibu/ [cf. p. 189 (§33.311.2b), p. 252 (§51.41a)]); ‘šr, ‘ten’, is vocalized /‘ašr-/) on the basis of comparative data alone, with that form being considered secondary to /‘ašar-/) mean that there is not a single certain case of post-tonic vowel syncope in three-syllable words in Ugaritic. If the last two cases be granted (and the existence of /qatl/ forms in Hebrew and Arabic certainly makes the last one plausible), they probably represent proto-Ugaritic, not inner-Ugaritic phenomena. The only form left that would represent an inner-Ugaritic development is [ša-θan-tu4], but I remain dubious about how much can be deduced from that writing (see remarks above to p. 146 [§33.115.44.5], etc., and to p. 182 [§33.242a], etc.).

— pp. 187-200 (§33.3). It is worth remarking that all diphthongs of the types /ay/, /aw/ had monophthongized in proto-Ugaritic and in proto-Phoenician for their are no clear cases of their preservation (this is not a proto-West Semitic development, as is shown by their partial preservation in Hebrew, Arabic, and Aramaic); and that those of the types /iy/ and /uw/ had monophthongized in proto-West Semitic, as they are rarely preserved as such anywhere in any of the West-Semitic languages. On the other hand, most of T.’s cases of monophthongization of /yV/ and /wV/ in Ugaritic are very hypothetical, while there are good cases of their retention, and the behavior of triphthongs may not be described in simple terms on the basis of alphabetic writings. The evolution of triphthongs had not, therefore, reached a stable state at the time when Ugaritic was frozen in time, first by being written down on tablets, then by the disappearance of its civilization. As regards the diphthongs of the types /iy/ and /uw/, T. assumes that all /qûl/ and /qîl/ nouns which are semantically cognate to a verbal root of the hollow type result from monophthongization, viz., /qûl/ ← /quwl/ and /qîl/ ← /qiyl/.

This approach overlooks the possibility of derivations from what appear to be biconsonantal roots with a pure long vowel between the two radicals, such as is clearly the case of /qâl-/, ‘voice’ (see following remark), and /tâb-/, ‘good’ (absent from the index of Ugaritic words—see remark below to p. 200 [§33.323.4b], etc.), in spite of the fact that both are associated with hollow verbal roots in later West-Semitic languages. It appears necessary to keep an open mind on the precise derivation of at least some such forms in Ugaritic.

— p. 188 (§33.311.1b), p. 252 (§51.41a). It is unclear what T. means by “Alph. Befund” for the contraction of /aw/ to /ô/ as compared with the “Syll. Befund,” where the phonology is indicated explicitly by writings with [u]-vowel signs combined with meaning/etymology, as well as by the meaning of the entries for other languages if the cited form is from a polyglot vocabulary. One of the syllabic example cited is, moreover, indecisive: T. transcribes {PI-
mu}395 as {yu-mu} in proof of a vocalization /yômu/, though it could be normalized as {ya-mu} and match the base of the Hebrew plural /yâmîm/.396 Moreover, of the five Ugaritic examples cited,397 only two contain {û}, and one of these is uncertain: {ûšn}, ‘gift’, could be /’ûšan-/ or /’ûšân-/ rather than /’ôšân-/. Of the three others, one must be judged uncertain: {ql}, ‘voice’, may be /qâl-/ rather than /qôl-/.

On p. 252, T. cites only Hebrew /qôl/ in favor of deriving the word for ‘voice’ from a /qâtîl/ base, but Aramaic /qâl/ shows that a base form with /ā/ is just as plausible for both Hebrew and Ugaritic. Indeed, the only comparative datum in favor of T.’s vocalization of which I am aware is Arabic /qawl-/ certainly a strong indicator, but indecisive in the absence of data from Ugarit.

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

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

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

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

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

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

397 No reason is indicated for limiting the examples to five; in the case of /ay/ → /ê/ (p. 189), eight examples are cited, in that of /'îy/ → /î/ (ibid.) nine, including one proper name. Below, §33.312.12b (p. 190), a set of examples is explicitly termed a “Beispielauswahl”—the word could have been used more often.
— pp. 190-93 (§33.312). In the course of this section on diphthongs of the type /w/y+vowel/, various contractions are assumed on the basis of vocalizations based on comparative considerations alone, i.e., for which there is no evidence from Ugarit. All such vocalizations are, of course, purely hypothetical, for the proto-Ugaritic form need not have been identical to that of one or more of the other Semitic languages. Only a few of these are mentioned in the following remarks.

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

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

399 Cf. p. 193 (§33.312.32): “yh /yahî/ ← yahi (Kürzung des Auslautvokals) ← *yahyi.’’

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

401 Also without arguing the case, T. makes the same error in his treatment of the divine name YHWH (“Der Gottesname *Yahwa,” VT 51 [2001] 81-106, esp. pp. 91-92), claiming that a /yiqtil/ verbal form of a root HWY should have been /yahwî/. Such is not at all the case in proto-Hebrew: yâhî, ‘may he live’, and yâhîy, ‘may he live’, can only have come from /yVhyV/ and /yVhyV/, where the final vowel was short (e.g., /yiyây/ → /yiyâ/ → /yihâ/ → /yihî/ → /yîhî/ → *yîhî, exactly like /piryu/ → /piry/ → /pirî/ → /pirî/ → *pîrî, ‘fruit’—on the formal identity of these types of nominal and verbal developments, see also below, remark to pp. 653-71 [§75.53]). Thus, M. Weippert's reconstruction of the basic proto-Hebrew form as having a short final vowel (Reallexikon der Assyriologie V [1977] 252-53), which T. criticizes on p. 92, must be preferred. Whether the divine name YHWH was originally nominal /qatl/ or verbal /yiqtal/ or /yiqtil/ cannot, therefore, be decided by the orthography and presumed morphology of the name that is deduced therefrom; this is all the more true since a proper name can be perceived as belonging to the nominal category and be ascribed nominal inflection (e.g., Amorite {ia-wi-um}, cited by T. on p. 91, must be /yahwiyum/ or /yahwiyum/ with nominative case vowel and mimation; at Ugarit, West-Semitic names are usually, though not always, correctly inflected for case when written in syllabic cuneiform). Only the plausibility of the respective etymologies enables one to lean in one direction or the other and, as T. recognizes, there is no good etymology from a hypothetical root YHW. It appears necessary, therefore, to retain the explanation of YHWH as based on a verbal form of the root HWY. It is perhaps not totally irrelevant to observe that if
— p. 190 (§33.312.12b). This paragraph is devoted to the retention of /y/ and a following vowel at the beginning of a word irrespective of the quality or the quantity of the vowel (for example, /yôm-/ 'day', and /yad-/ hand'). T. refers to no modification of such syllables elsewhere in the West-Semitic languages; since, as far as I can remember, such syllables are normally retained in the West-Semitic languages, one wonders why a paragraph was devoted to the topic.

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

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

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

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

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

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


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

404Nougayrol, Ugaritica V (1968) text 84.

Archiv für Orientforschung 50 (2003/2004) online version

In the first section cited, ṣrw, ‘lion’, is said to be from a quadriliteral root 'RWY, in the second, to be an '/-preformative; in the second, Hebrew '/aryē/ is cited as a parallel. The precise relationship between the Ugaritic and Hebrew forms is impossible to determine without more data, for Hebrew maintained certain qatw forms (e.g., '/āhū, ‘swamp’, /ṭōhū, ‘wasteland’), and it is not clear why '/arwu/ or '/arwē/ would in proto-Hebrew have become '/ary-/ or '/aryē-/. The two Hebrew forms '/ärē/ and '/aryē/ could either be by-forms, viz., /qatl/ and /qatlal/, or the first could be a contraction of the second, viz., the proto-Hebrew form would have been '/aryē/ (that is, the genitive form retained from the inflection '/aryū'/aryā'/aryē/); '/ärē/ would be a secondarily contracted form of '/aryē/). Because the contraction of ‘regular’ III-y nouns from proto-Hebrew to Hebrew normally resulted in a final syllable with seghol, the peculiarity of the Hebrew long form '/aryē/, i.e., the spelling with šere, favors the first solution, viz., proto-Hebrew by-forms '/ary-/ and '/aryay-/.

I have, however, found no simple set of developments that would allow the derivation of the Hebrew and Ugaritic forms from an identical parent form. That III-y and III-w forms could develop side by side, however, is clear from the coexistence in Hebrew of forms such as /erwā/ and /eryā/, both meaning ‘nakedness’; so the relationship of Ugaritic {±rw} and Hebrew /'arē/ and /'aryē/ may become clearer with time.

In RS 3.325+ vi 43 (KTU 1.16) is said to be derived from “’gazw/yī,” in the second from “’gVzw/yī,” and in the third from “’gVzwī.”

Given the retention of the [y] in 'āny, ‘(group of) ships’, and comparing Hebrew /’oniyā/ (a different base but with /yy-/), it appears likely that the Ugaritic form was either /’anay-/ or /’anāy-/ , not “/’any-/ (← *’anay-)” (p. 192). The /qatl/ form is assumed on the basis of a hypothetical proto-Ugaritic stem with two short vowels of which one would have disappeared by syncope; no Ugaritic or comparative evidence of which I am aware supports it.

T. is certainly correct in reading gdt for gdm in RS 24.255:7 (KTU 1.111). I would, however, indicate the reading as {gd¯t˘} rather than as {gdt!}, for what has been taken as a vertical wedge after the horizontal is neither clean, deep, nor clearly vertical. It may well be an erasure mark; if so, perhaps of an extra word-divider.406

As T. has already recognized elsewhere,407 not only is the context of the reading {šlyh} in RS 1.064+:6 (KTU 1.27)

unclear, but the reading itself is more than questionable. Indeed, I consider the reading {l‘lyh}, ‘in the upper room’, to be far more plausible.\footnote{BSOAS 58 (1995) 232; Les textes rituels (2000) 360, 1270.} This is a prime example of T.’s vacillation as regards his data, described above in the introduction: in the first two sections cited, he bases his analysis on the reading {slyh}; in the third, he works with “(?)-lyh,” citing my article in BSOAS 58 (reference note 408).

— p. 193 (§33.312.32b). If {pǒy} is indeed the masculine form of pǒt, ‘girl’, then the Hebrew personal name pũw‘āh should in all probability no longer be cited as a cognate of the Ugaritic word, for the plene spelling in Hebrew is normally the indication of a hollow root, not of a root that is III-y (according to normal Hebrew rules, /pu‘yatu/ might have given something like /po‘o[yā]/ or /pō‘ā/ in Hebrew, but not /pũ‘ā/). Nevertheless, T. bases his vocalization of the Ugaritic form on that of the Hebrew name: “/puŷ/.” As there is no particular reason why a word for ‘boy/girl’ should have a qutl base, for this base commonly expresses abstractions, the Ugaritic/Hebrew connection must, until new data appear, be considered questionable.

— p. 193 (§33.312.32b), p. 256 (§51.41e), p. 303 (§54.111). {ri} in RS 2.[014]+ i 12 (KTU 1.3) is translated “Aussehen” and/or “Erscheinung” in all three paragraphs cited but is identified as a “n[eue] L[esung]” only on p. 303. One must object that the reading is far from new, since it was proposed by de Moor in 1971.\footnote{The Seasonal Pattern in the Ugaritic Myth of Ba‘lu according to the Version of Ilimilku (AOAT 16; Kevelaer: Butzon & Bercker; Neukirchen-Vluyn: Neukirchener Verlag, 1971) 67, 75; cf. Pardee, Ugaritic and Hebrew Poetic Parallelism: A Trial Cut (‘nt I and Proverbs 2) (VTS 39; Leiden: Brill, 1988) 2 et passim.}

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

— p. 193 (§33.312.32b), p. 486 (§73.523b). In the first section cited, it is averred that {bd} in RS 3.325+ i 5 (KTU 1.16) does not derive from the root BDY but from BDD; in the second, just the opposite. However that may be, the signs in question may just as well represent the prepositional phrase b + yd meaning ‘in the hand(s) of’, i.e., ‘into the control of’.\footnote{Cf. Pardee, UF 5 (1973) 230; idem, Context I (1997) 339; Wyatt, Religious Texts (1998) 220.}

— pp. 194-200 (§33.32). T. pretty much throws in the towel on writing rules for the behavior of triphthongs in Ugaritic, apparently because “der betreffende Befund ist schwierig zu interpretieren,” something with which I would be the last to quarrel. He contents himself for the most part with listing all the possible combinations of vowels and /y/w/; the forms corresponding to the principal rule, viz., with or without contraction, are listed first, the exceptions and/or possible exceptions thereafter. Because the examples for a given rule are always few, usually based on comparative rather than internal data, and
usually accompanied by apparently conflicting examples, one understands T.’s reticence to formulate overarching rules. This being the case, one may question T.’s decision to indicate a given form under a totally hypothetical base (e.g., {yr}, “Frühregen,” /yarû/ ← /warayu/, against Hebrew /qâtil/ [p. 198][411] on the basis of rules as poorly formulatable as are these (on pp. 195-96, four possible exceptions to the general retention of the /y/ in the sequence /âCiyV/ are indicated; why was this form not added to that list?). On the other hand, I missed even so simple a rule as: III-y/w nouns show a triptotic case system with contracted second vowel (e.g., /ßadû/ßadâ/ßadî/, ‘field’) whether the historical consonant be /y/ or /w/; the rule appears easily extractable from the various forms listed, whether attested by data from Ugarit, from comparative Semitics, or purely hypothetical. Because the rule is based on monophthongization, there is, of course, never any direct data from Ugarit on whether the proto-Ugaritic third consonant was /y/ or /w/. T. indicates one or the other (e.g., /ßadû/, ‘field’, ← /ßaduwu/ but /qanû/, ‘reed’, /qanayu/) without indicating the comparative basis for his decision. The reader should note that T. for the most part limits his choice of examples in this section to nominal forms, leaving the even more problematic verbal forms for treatment under verbal morphology (pp. 653-71 [§75.53]).

— p. 195 (§33.322.2a). T. vocalizes the month name [lyr] as /hîyar-/ but, in spite of the phonetic and calendric differences, the month name /îyyâr/ used in both Hebrew and Aramaic shows that the Ugaritic form may have had /yy/ and /â/.

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

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

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

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

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

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

413It is unclear why he does not list on pp. 663-64 the other forms with an ‘alif-sign in first position that he interprets elsewhere as G-imperatives (/išh₃n), see remark to p. 170 [§33.211.2], etc., and ʿuqr₃b), see remark to p. 202 [§33.423a], etc.).


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

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

Archiv für Orientforschung 50 (2003/2004) online version
food (in which he [= you] said): “(Here) with me, plenty (has become) absence of food,” let my son assign this: sea-faring boats. Let him […] and food […]. In other words, the author of the present letter cites a previous letter in which his ‘son’, the king of Ugarit, has pleaded that his food supplies have dwindled to the point that famine is threatening; the father-figure (of unknown identification) is apparently suggesting that the ‘son’ send ships to Egypt for food, the standard procedure in the late 13th-early 12th centuries.417

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


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


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

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


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

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

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

422The line in question is now vocalized /bêta ‘ubû‘u ‘al tubû/tubû ‘i/ in Bordreuil and Pardee, Manuel (2004) II 68 (the indication of alternatives for the vocalization of the second form are owing to the fact that the gender of the addressee is unknown).

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


425“Une formule épistolaire en ugaritique et accadien,” Semitic and Assyriological Studies Presented to Pelio Fronzaroli (Wiesbaden: Harrassowitz, 2003) 446-75. On T.’s interpretation of the formula, see below, remark to p. 244 (§45.122b), etc.
— p. 197 (§33.322.42a). Given that the vocalization “/tºk≠yi/” of the second element of the place name glltky is glossed as “inner(e[r/s]),” the /º/ must be an error for /ô/, for below T. correctly takes the proto-Ugaritic form as /tawk-/ and indicates the Ugaritic form as /tôk-/ (p. 772 [§83.312], p. 775 [§82.412]).

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

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

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

— p. 197 (§33.322.42c), p. 274 (§51.46j). The fact that the Hebrew word for ‘foreigner’ bears the nisbe ending (nokrî3) is an insufficient basis for taking Ugaritic {nkr} as the only example that would be attested of the Ugaritic nisbe ending on a singular noun not represented in the writing (“/nukrî/ < *nukrîyî”). Instead of consisting of the /qutl/ abstract base plus nisbe, as in Hebrew, the Ugaritic form probably showed one of the adjectival bases, as in Akkadian (nakru, nakaru, etc.). Furthermore, the fact that there are no certain cases of contraction of the nisbe ending, neither in the singular nor in the plural (see following remark), is best explained by the fact that the nisbe morpheme was -/yVy/-, rather than -/iy/-, as indicated by T. (pp. 273-74 [§51.46h-k]). Though one may quibble on the basis of the fact that /i/, /î/, and /y/ are homorganic in nature, the tenacity of the nisbe ending, not to mention the Massoretic vocalization with {iyy}, are indicators that the Hebrew, and probably Ugaritic and Phoenician, forms were phonetically -/iyy-/.

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

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


just as plausible, if not more so, to take the expression as meaning ‘\textit{`Atiratu} of Tyre, the
goddess of Sidon’ as to see her as the goddess of the Tyrians/Sidonians. The other two
examples of gentilic plurals claimed in this section to be written without the expected \{y\}
\{gynm\} in RS 9.453:28 [\textit{KTU} 4.44] and \{kpslnm\} in RS 17.124:1, 6 [\textit{KTU} 4.274]) have no
basis in previous Ugaritology\footnote{See, for example, van Soldt's two principal studies of the Ugaritic
toponyms (\textit{UF} 28 [1996] 653-92; \textit{UF} 30 [1998] 703-44), where neither word is considered, or del
Olmo Lete and Sanmartín, \textit{Diccionario I} (1996) 154, 222, where, respectively, \{gynm\} is taken as a personal
name,\{kpsln\} as a common noun.} or in grammatical plausibility.

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

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

— p. 198 (§33.323.2b), pp. 771-72 (§82.311), p. 817 (§87.3), p. 818 (§87.4). The
vocalizations of the various forms derived from the root BLY, ‘disappear, not exist’, do not
consistently reflect the disappearance of the /y/: the noun \textit{bl}, “Nichtsein,” is vocalized /balû/
\leftrightarrow /balayu/ (p. 198); the preposition \textit{bl} is vocalized /balî/ or /balu/ with no specific derivation
indicated (p. 771); the substantive \textit{bl} that has come to function as a negative particle is
vocalized /balî/, also with no specific derivation indicated; finally, the noun \textit{blt} that is also
classed with the negative particles is vocalized /bal(V)ti/ and not even a root derivation is
mentioned specifically (p. 818), though one may infer from the “(V),” for which there is no
evidence in any of the forms cited for comparison from other languages, that T. considers the
root here also to have been BLY. A topic that someone with T.’s expertise should treat in
more detail is that of substantives and particles apparently related to roots \textit{tertiae infirmae}
that show a short vowel or Ø-vowel in the position of the weak consonant, e.g., Biblical
Hebrew \textit{b\textsuperscript{2}li, bal, and bilt\textsuperscript{y}}, all apparently from this root BLY.\footnote{It may be remarked here that short vowels in the place of the weak consonant in roots \textit{mediae infirmae}
can often be explained by shortening of an historically long vowel in a secondarily closed syllable, e.g.,
/*\textit{bûšatu/} \rightarrow /*\textit{bûštu/} \rightarrow /\textit{buštu/} \rightarrow /\textit{bušt}/ \rightarrow /\textit{böšet/}, ‘shame’, in Hebrew.} Such variety in the
comparative material makes any vocalization of the Ugaritic particles hazardous, but either
consistency or arguments in favor of non-consistency would make T.'s presentation appear
less arbitrary.

\footnote{See, for example, van Soldt's two principal studies of the Ugaritic toponyms (\textit{UF} 28 [1996] 653-92; \textit{UF}
30 [1998] 703-44), where neither word is considered, or del Olmo Lete and Sanmartín, \textit{Diccionario I} (1996)
154, 222, where, respectively, \{gynm\} is taken as a personal name,\{kpsln\} as a common noun.}
Though the formation is not completely regular, Ford's suggestion to take tp in RS 22.225:2 (KTU 1.96) as the 3 f.s. /YQTL/ form of PHY, 'to see', certainly makes better sense of this incantation against the Evil Eye than does taking tp as a t-preformative noun or a /YQTL/ form of YPY, 'to be beautiful', the two options presented on p. 269—on pp. 199, 597, and 713, only the nominal option is considered; on p. 810, no translation is offered for tp. (On the relationship of this line to the preceding, where the verb šnwt is found, see remark below to pp. 596, 597 [%74.624]; on the form {ypn} in RS 3.322+ iii 14 [KTU 1.19], which is certainly from PHY, see above, remark to p. 60 [%21.354.1b], etc.; on the implausibility of emending tp here to tpnn, see remark below to that word in RS 22.225:5-6, p. 579 [%74.511b], etc.).

Though T. may be correct in analyzing the form yphn in three Ugaritic rituals as an indicative form with contraction ("/yiphû/ < *yiphayû"), it is also possible that the form is jussive (/yiph/ ← /yiphay/), for the form occurs only in prescriptive rituals with mlk, 'the king', as subject of the verb. In general, however, the ritual formulation prefers indicatives over jussives and his analysis is probably correct.

Structurally speaking, in spite of the poor condition of the top left portion of the tablet, {sty} in RS 2.002:6 (KTU 1.23) should be dual (T.'s alternative analysis) rather than plural.

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

T. provides no entry for his alternative analysis rather than plural.

T. provides an entry in the polyglot vocabulary RS 20.189A+B, is listed under “Alph. Befund” rather than under “Syll. Befund.” This entry provides, by the way, a prime example of the highly hypothetical nature of the reconstructed forms of hollow roots (see above, seventh general remark): “/tâbu/” is reconstructed as coming from /tayabu/ with no explanation as to why a root that rather characteristically shows /º/ in Hebrew should be II-y rather than II-w or as to why a stative adjective would...

---

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

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

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


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

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

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

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

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

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


440Ford has recently analyzed the {ū} of *qrb* as the negative emphatic particle (UF 34 [2002] 155 and 171: “woe”) but such an analysis is no more required here than in any of the other putative occurrences of the particle (see remarks to p. 196 [§33.322.3c], etc., and to p. 805 [§84.22]).
(there is no entry for it in this grammar), but of which a growing number of examples indicate the existence.\(^{441}\)

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

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

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

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

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

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


\(^{442}\)Pardee, JAOS 95 (1975) 616.

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

\(^{444}\)Lešonenu 23 (1958-59) 77.
the expectation that, if the proto-Hebrew vowel had been short, it would have dropped by the time of Biblical Hebrew. Whatever the precise development to the Hebrew forms may have been, however, the evidence of Akkadian and Arabic indicates that the rule for final short vowels in passing from proto-West Semitic to Hebrew must be that such vowels were either dropped or retained and lengthened in Biblical Hebrew; in the case of /a/, such lengthened vowels in Biblical Hebrew, where length was still phonemic, were represented by the Massoretes as qames. (The different developments of historical short vowels must, of course, be amenable to rule-ordered explanation, but the data may presently be insufficient for such an enterprise to be carried out in a fully convincing way.) Until evidence appears for /a/ → /ä/ or similar changes for the other vowels from proto-Ugaritic to Ugaritic, one must, in the light of the many archaisms undeniably present in the language (e.g., the fully functioning case system where the cases of singular nouns were expressed by short vowels), assume that the singular pronouns had final short vowels in Ugaritic (on the linking of short vowels and singular grammatical number, see further below, note 1146).

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

— p. 209 (§41.112.3), p. 212 (§41.131c), p. 228 (§41.32), p. 482 (§73.513.5b), p. 493 (§73.532), p. 615 (§75.212.5), p. 666 (§75.536), p. 784 (§83.112a), p. 827 (§89.231a), p. 863 (§93.33a). Given the implausibility of T.’s other examples of independent pronouns used dativally in Ugaritic and Hebrew (see remark below to p. 212 [§41.131c] on {lm ank} in the Kirta text), his analysis of ât in the formula ât(m) w ânk ibgyh (RS 3.361 iii 16 [KTU 1.1], RS 2.[014]+ iii 28’ [KTU 1.3]) as meaning “Dir (allein) will ich es offenbaren” fails to convince. Either his stance that the imperative of the verb ‘TY/W, ‘to proceed (to a place)’, would have been spelled {ity} because pronounced /i-itiyi/ (p. 482) is incorrect (and he cites no basis for that vocalization other than by a reference to §73.122, which we have already seen to be totally unconvincing\(^446\)) and {ât} is in fact an imperative, or else his alternative interpretation of {ât} as an infinitival form used in place of an imperative (pp. 482, 493, 615, 666) is preferable. If {ât} in RS 18.140:19’ (KTU 2.45:20), a passage that T. has not analyzed in this grammar, is a verbal form as has been proposed,\(^447\) it is more plausibly an imperative than an infinitive,\(^448\) for the use of the infinitive to replace a finite form is not particularly common in prose. The analysis of {ât} in this text as a verb, rather than as a pronoun, appears to be required from context, for the phrase ât ngt is isolated from the preceding and following phrases by the conjunction w and, for that reason, must be


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


\(^448\)The corresponding verb in Arabic is of the /yaqtil/ type, plausibly the basis for T.’s assertion that the imperative in Ugaritic should have has an /i/-vowel in the first syllable (on pp. 425-26 [§73.121], T. reconstructs the base form as qtil which has in Ugaritic become /q'til/).
translated ‘go to Nuḥašše’.

Whether the verb 'TY/W was of the /yiqtal/ type in Ugaritic, or the forms cited are infinitival, or, perhaps, the G-imperative of the /i/-type was /qatil/, must be decided by data yet to be discovered.

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

— p. 211 (§41.12), p. 213 (§41.132e), p. 514 (§74.222.3). T. avoids taking [t]-preformative forms in the ritual texts as 2 m.s. (‘you will do X’), preferring to take them as 3 m.pl., active or passive (see also below, remarks to p. 285 [§52.5c], etc., p. 325 [§54.233a], etc., pp. 505-6 [§73.643a], p. 568 [§74.422], p. 582 [§74.515.1], p. 595 [§74.623], p. 601 [§74.623.3b], p. 602 [§74.623.3d], p. 633 [§75.512], etc., p. 735 [§77.51b], and p. 770 [§82.38]). The issue becomes crucial in RS 24.248:19 (KTU 1.104), for T. cites that text as a possible example of hmt, 3 m.pl. pronoun, used in the nominative case whereas in all other cases of hwt, ‘him’, hyt, ‘her’, and hmt, ‘them’, the pronoun is clearly in an oblique case. If one takes the verb forms as 2 m.s., however, the example disappears. The context is: {w t® ¯.˘ “d®n (18) ®nm . w “d®h (19) tdn . hmt (20) w tßtn ®nm}, which may be translated ‘You will again furnish (n¬p-outfits), (18) two of each; then again (19) you will bring them near (20) and you will place (them) twice (i.e., the two of them, each as appropriate)’. T. translates “… und dann zweimal erneurt(?); am Neumondstag(?) werden sie (sc. in Z. 16 erwähnte nps-Gegenstände/Kleider) niedergelegt/entfernt und zweimal hingestellt” (p. 213). The form t® cannot have anything to do with the number ‘two’ and one wonders how hdtm could mean ‘be renewed’. t® may, therefore, be identified as an example of the imperfect of the weak root TWY discussed above (remark to p. 110 [§32.144.12b], etc.; cf. below, remark to p. 595 [§74.623]) and hdtm taken as a substantive used adverbially, literal meaning ‘anew’. Against T.’s translation, hdtm in line 18 probably has a similar meaning (the previous paragraphs of the text had dealt with the eighth, ninth, and tenth days of the festival, and fixing the next event at the ‘new moon’ is out of place at such a juncture). Otherwise, our interpretations are similar, save for T.’s taking hmt as the subject of the two

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


451T. maintains this interpretation in UF 33 (2001) 690, recognizing only that it is “sehr unsicher”; he does not address the issue of hmt as a nominative form. On T.’s treatment of line 17, see above, remark to p. 142 (§33.144.11), etc.
verbal forms. Because of the uniformity of usage of the [-t] forms elsewhere as obliques, and because the [t-]preformative forms are just as plausibly addressed to the cultic official in charge of the ceremony, it does not appear too strong to judge T.'s interpretation as straining the evidence. Whether or not the -t/ morpheme began its life as an oblique morpheme, and though one may grant that [-t] has other functions in Ugaritic (see remark below to p. 230 [§42.3], etc.), its function when attached to 3rd person pronominal forms in Ugaritic and Akkadian is clearly that of an oblique marker (Akkadian examples cited by T. on p. 211). The Phoenician forms with [-t] in nominative forms cited on p. 213 are, of course, irrelevant for determining the function of the Ugaritic forms. If the Ugaritic forms can be shown to correspond to syntactic function, and such is the case up to the present, the Phoenician shows either the remnants of such a system after the disappearance of case vowels and most case distinctions or else the remnants of another system.

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

---

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

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

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

455 See Pardee, Context III (2002) 103; Bordreuil and Pardee, Manuel (2004), text 33 in the Chois de textes; Bordreuil, Hawley, and Pardee, publication of the Ugaritic texts discovered in 1994-2002 (in preparation). This text provides a clear example of deictic *hn* (and *ht*, see below, remark to p. 229 [§42.0], etc.) preceding a noun that is in turn followed by a demonstrative adjective, a syntagm that was previously
Rather than interpreting the independent pronoun *ānk* in the formula *lm ānk ksp* in the *Kirta* story as expressing the dative (‘Wo zu soll mir Silber und gelbes Gold (dienen) … ?’ [p. 754]), it is preferable to take it as what it is in the literary structure of the text, viz., as a response to the imperative formula *qh ksp* … . Though in its first occurrence (restored in RS 2.[003]+ i 51 [KTU 1.14]) we do not know what preceded this formula because the context is damaged, in the other extant occurrences (iii 23, vi 17), *lm ānk ksp* … always follows the request to ‘take silver’. Thus Segert proposed that the formulation is to be understood as dependent on the offer to which it is responding, namely, *qh ksp w yrq hrs*, ‘Take silver and yellow gold’ (iii 22). The pronoun would thus be a functioning straightforwardly as a nominative: ‘Why (should) I (take) silver and yellow gold?’ This interpretation has recently been defended in a thorough and convincing study of verbal ellipsis (gapping) in Ugaritic poetry. Though the appropriate English translation in context might be something like ‘What need have I of silver … ?’, a more literal, and not uncolloquial, English (at least American English) translation would be ‘Why? me?, silver … ?’. One may also query the validity of the Hebrew parallels for the dative interpretation cited by T. *wa*₂₄ⁿⁱᵈ mʰᵗʰʸ míṣpᵃʳ in Gen. 34:30 may express the speaker’s identification with his men, viz., not ‘I have only a few men’, but ‘I am (i.e., my forces are) only a few men’; and, in the formula *w*²*₄ᵃᵗᵗᵃʰ sᵃˡᵒᵐ (1 Sam. 25:6), the noun may be taken as an adverbial accusative, viz., not ‘(may there be to) you well-being’, but ‘(may) you (be in) well-being’.

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

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

456S. Segert, A Basic Grammar of the Ugaritic Language with Selected Texts and Glossary (Berkeley: University of California, 1984) 121.
459Context I (1997) 249. It may be remarked, however, that there is nothing aberrant in using the order corresponding to the Semitic order in the languages I know. Certainly, in French, ‘ton nom, toi, sera Yagrušu’, corresponds to the standard spoken language.
in Ugaritic of the third-person pronoun in apposition to an object suffix to permit us to determine whether the oblique form of the pronoun (hwt) would have been used. Judging from the Hebrew example w$\text{h}^{\text{y}}$ $\text{d}^{\text{y}}$ $\text{n}^{\text{y}}$ $\text{b}^{\text{d}^{\text{k}}}^{\text{k}^{\text{h}}}^{\text{a}}$ (1 Kings 1:26), ‘and to me, I (who am) your servant(, he did not send an invitation)’, where the independent pronoun functions as the subject of an embedded nominal sentence, one may guess that the nominative pronoun would have been used in Ugaritic.

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

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

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

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

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


\(^{461}\) UF 11 (1979) 387. This analysis has been adopted by Bordreuil and Pardee, Manuel (2004) II 86, 87, 150.
1.4 \{klnyn\}), and the fifth in another text (RS 3.343+ v 20 \[KTU 1.15\] \{b’lny\}).\(^{462}\)

Though T. allows for the possibility that the {-\(y\)} may be the enclitic morpheme, that analysis is not the one he prefers. When one considers that the 14th-century Canaanite form was written syllabically {-\(n\u2032\)} (cited p. 224) and that the corresponding form may be attested syllabically at Ras Shamra,\(^{463}\) the vocalization with /\(\ddot{a}\)/, and the corollary that the {-\(y\)} is the enclitic particle in the five cases cited, must be preferred (on the problem of the 1 c.du. suffix, see above, remark to p. 196 [§33.322.3b], etc.). In RS 2.[014]+ v 33-34 (\[KTU 1.3\]), where the form is \{klnyy\} and two referents may be evoked, that form may consist either of the noun \(kl\) + 1 c.du. pronominal suffix + enclitic {-\(y\)} or of the same noun + 1 c.pl. pronominal suffix (referring to all the gods) + the enclitic particle repeated.\(^{464}\) In RS 2.[008]+ iv 45'-46', where the form is \{klnyy\} and the possibility of two referents appears ruled out, it would consist of the noun \(kl\) + enclitic {-\(y\)} + enclitic {-\(n\)}. This comparison leads to the conclusion that in the first case the suffix is 1 c.pl. and that the enclitic {-\(y\)} is repeated, while in the latter this particle is followed by {-\(n\)}. T., of course, takes both forms as representing a 1 c.pl. suffix {-\(ny\)} with enclitic {-\(n\)} or {-\(y\)}. (On the pronominal element of the 1 c.pl. /QTLa/, see remark below to p. 467 [§73.332.5].)

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

— p. 215 (§41.221.11b), p. 217 (§41.221.14). None of the three cases of a m.s. noun in the accusative case with Ø-marked 1 c.s. suffix is convincing\(^{465}\) and, since there are clear examples of such forms written with {-\(y\)} (p. 216 [§41.221.12b,c]; p. 218 [§§41.221.15b, 16]), T.’s attempt to prove that the suffix was properly /l/ in such forms (as in Akkadian and Phoenician: p. 217 [§41.221.14]) may not be accepted at face value. That being the case, the conclusion drawn on p. 217 that the “Terminativending” must have been “/-â/ < *-\(\ddot{a}\)” is valueless, for (1) such forms may be simple accusatives, and (2) as is shown by the common usage of the adverbial morpheme {-\(h\)} in Ugaritic and Hebrew, the hypothesis that it haphazardly became /-\(\ddot{a}\)/ in Ugaritic cannot be demonstrated by a few dubious cases of

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

\(^{463}\)Huehnergard, Syria 74 (1997) 219.

\(^{464}\)Pardee, Context 1 (1997) 255, n. 112 (on p. 822 [§88.24], T. accepts that the enclitic particle {-\(m\)} may be repeated and there should, for that reason, be no particular objection to that analysis of {-\(y\)}).

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

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


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

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

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

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

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


470In Kausativstamm (1990) 29. T. chose the division {r‘ yssh], and his explanation for now finding that division implausible would have been welcome.

471 In UF 31 (1999) 737, T. presented the linking of the {n} with the 1 c.s. suffix in this text as possible.
morphebymwhichanounistransformedintoanadjective(thensisbe).Theproper
interpretationisthe"powerfulone…themightyone"(T.'sthirdoption).

—p.219(§41.221.17c,d).Giventhatthepreposition‘mshowstwoforms,‘m’and
‘mn’,andthatthefirstwiththe1c.s.suffixappearsonlyas‘my’whilethesecond
appearsas‘mn’and‘mny’,itappearsnecessarytoconclude(1)that‘m’endsin
avowelthatdoesnotallowcontractionwiththe1c.s.suffix/-ya/,and(2)thatsituation
istheoppositewith‘mn’,viz.,that‘mn’+1c.s.suffixis‘immānî’while‘mny’consists
ofthesameformtowhichtheafformativeparticle/-ya/hasbeenadded(‘immānîya’).
Theseconclusionsleadtoathing:thebaseformofthesimpleprepositionwasprobably
‘imma/ (i.e.,originallytheaccusativeofthesubstantivewhencetheparticledescended)
whilethattoftheexpandedformis‘immānu/,forthe1c.s.suffixafter/-u/becomes{-Ø},
probably/-i/,whileitdoesnotcontractwith/-a/and/-i/ (seeabove,remarktop.215
§41.221.11b).T.’sprimarypresentationof‘mny’asplenewritingof‘immānî’isthus
toberejectedinvfavorofhisalternativeanalysisaccordingtowhich{-y}would
representthe
encliticparticle (onthisparticleattachedtotheprepositionsbandlee,seeabove,remarktop.
37-38§21.322.5,etc.).

—p.220(§41.221.2),p.648(§75.524).Because{sḥn}and{qrān}inRS2.[022]+i22,23
(KTU1.5)aretheonlyexamplesofthe1c.s.pronominalsuffixtakingtheformof{-n}on
3m.s.perfectverbs,T.mighthaveconsidereditworthhistimetotranscendthatthe
verbalformsinfactimperatives.473

Not
even"evtl."(p.220)maythe{-n}of{tlikn}inRS34.124:10(CAT2.72)beplausibly
considereda1c.s.pronominalsuffixwithdativeforce.Below,p.230,thisanalysisis
notpreferred(itismentionedbutonlyasathirdpossibilityonp.458,asasecondpossibility
onp.548,asapossibletranslationonpp.789and822474),buttheequallyimplausible
interpretationasa/YQTLØ/withenergicsuffixispresentedonpp.458,701(hereamong
possiblecasesof/YQTLØ/-perfectivesinprose,noneofwhichisconvincing),and733.
As
hisfirstinterpretation,T.offerseverywhereexceptonp.701thegermanpresent

tense,thesamethatBordreuilandIprefereoinoureditionofthetext.475(Caquot,inhispreliminary
edition,translatedbythepasttenseinFrench476;inmypreliminarystudybasedonthis
presentationbyCaquot,ItranslatedbythepresenttenseinEnglish.477)Therealquestion,
however,isnotthetranslationbutthereasonfortheuseofthe/YQTLu/forminthe
text,


474Onp.733,thisinterpretationisexplicitlydeniedplausibilityonthebasisoftheprepositional
complement‘m’thatisusedoftenwiththisverb.

475BordreuilandPardee,Unebibliothèque(1991)144.

476ACF75(1975)430.

477BiOr34(1977)3.
which is the obvious analysis of a 2 f.s. imperfective form in a prose text, a question that T. does not address. One may surmise that the act in question had occurred more than once; such may be also the explanation for the D-stem: 'Why do you repeatedly send a ḫuptu (and not what I've asked you to send)?'⁴⁷⁸

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

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

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

---

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

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

⁴⁸⁰ JNES 43 (1984) 244.
Hebrew), viz., /-(a)n/ and /-(a)nn/ on the basis only of forms bearing the 3rd person singular pronominal suffixes (/yqtlânn/). T. posits the existence alongside these two energics of a form that would be parallel to the Akkadian ventive ending /-nim/ and to Sabaic {-nn}. Because the /YQTLânn/ forms are often written /yqtl . nn/, it appears certain that they were perceived as consisting of a verbal form followed by a quasi-lexical suffixal morpheme. For that reason, and because the ending {-nn} is attested only with pronominal suffixes, i.e., never as a simple “energetic”, form, it appears more plausible to see the form as having arisen through re-analysis of an ending that consisted historically of the energetic ending and a pronominal ending as itself pronominal, i.e., /YQTLânu/ → /YQTLânnu/ with /-(a)nnu/ perceived as pronominal and usable after all the imperfective forms, including the energics. T.’s theory simplifies this process ({-nn} would consist of /-nvnu/ → /nvnnu/), but at the cost of creating a third energetic form. Specialists in comparative Semitics will decide which theory is the more plausible. (For more reactions to T.’s view of the energetic forms in Ugaritic, see below, remarks to pp. 497-506 §73.6.)

— p. 222 ($41.221.52c$), p. 687 ($76.331$). In 1996, T. criticized the authors of CAT for restoring the verb in RS 2.[014]+ v 1 (KTU 1.3) as [[â]mîsh . nn], viz., assuming a vocalization /'amsûh-/, and asserted that the proper restoration is [[imîsh . nn]. In the first section cited here he cites the form as “imîsh.nn’ but in the second as “[a]mîshnn.” The former is in keeping with his rule that III-guttural fientive verbs should be of the /yiqtal-/ type (p. 171 [$33.212b$]).

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

481The forms are listed in all Arabic grammars. In Hebrew, the /-(a)n/ ending is retained after long vowels as the so-called “paragogic nun” and with suffixes (/yiqtelmnû/ → /YQTLânu/); the /-(a)nn/ ending is attested only rarely with suffixes (/yiqtelnhû/ → /YQTLânnu/).


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

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

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

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

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

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

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

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

489Originally in AuOr 13 (1995) 236: “[ w . ytn*[?]nn ]”; here p. 223 (§41.221.52c: “[ ytn*[?]nn ]”); p. 637 (§75.514: “ytn-nn”), p. 705 (§76.523: “[w y]lnnn”—the last presented as a “n[eue] L[esung],” which it certainly is since the { t } was not previously indicated as visible). My collation with Robert Hawley in June of 2004 showed that there is certainly no word-divider before the second-last sign; the third-last sign is
(KTU 3.4), which may only be /QTLa/ or the infinitive (/YQTLØ/ is ruled out). On p. 224, three putative examples of the 3 f.s. pronominal suffix {-nn} attached to /QTLa/ verbs are cited. One of these must be rejected out of hand (see second following remark), while the other two are more plausible—more plausible because two non-preformative forms appear in parallel in a bicolon and the text indubitably recounts Kirta’s exploits. Nevertheless, the preceding forms of this section are all /QYTL/ forms, and the possibility must be considered that {grnn} and {šrnn} in RS 2.[003]+ iv 49, 50 (KTU 1.14) are in fact infinitives. The fact that [h] is unattested as a pronominal suffix on /QTLa/ verbal forms must be considered as an argument in favor of the hypothesis that the pronominal suffix alloforms -n and -nn could be attached to /QTLa/, at least in prose. The small number of attestations remains frustrating. As regards the three legal texts discussed above in this remark (RS 15.111:8, RS 16.191+, and RS 16.382), a not unlikely scenario is that the first occurrence of the verb in question was /QTLa/ whereas the repetition of that verb was each time the infinitive (respectively, w ytn, w pdyhm, and w ytnnn); the purpose of the different forms would have been to provide variation between the first and the second usages of the selfsame verb. In this case, all three examples would be removed from the list of /QTLa/ forms with -n or -nn suffixes. (For another possible example of -n attached to a /QTLa/ form, see remark below to p. 500 [§73.611.2f], etc.) — p. 224 (§41.221.62b), p. 430 (§73.162b), p. 503 (§73.627), p. 558 (§74.413.2), p. 559 (§74.414.1.2), p. 669 (§75.537d). It is remarkable that T. considers that grnn in RS 2.[003]+ iii 6 and iv 49 (KTU 1.14) and gr in RS 24.266:26 (KTU 1.119), everywhere meaning ‘attack’, may only be from either GRY or GRR without mentioning that Hebrew shows a hollow root with a similar meaning. This is not of purely lexicographic interest for the reason that T. mentions the orthography without {y} as a factor in remarks having to do with phonetics and morphology. Whatever the root may be, it is not implausible that the two tokens in RS 2.[003]+ are to be analyzed, respectively, as imperative and ‘narrative’ infinitive (see preceding remark). — p. 224 (§41.221.62b), p. 502 (§73.623), p. 635 (§75.512). It is simply out of the question that the form {ytnnn} in RS 16.265:9 (KTU 5.9) should represent a perfective form with pronominal suffix, as T. takes it without even indicating the alternative analysis as an imperfective. Lines 7-10 read {iršt . āršt (8) l āhī . l r‘y (9) w ytnnn (10) l āhī . l r‘h}, which may only be translated ‘A request I would make of my brother, my friend, and may he grant it to his brother, his friend, his friend forever’.490 The presence of the pronominal suffix written {-nn} after the verbal stem shows that the latter is either volitive /YQTLa/ or indicative /YQTLu/ (expressing a weakly marked volitive). The analysis as a jussive

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

490Pardee and Whiting, BSOAS 50 (1987) 11-12, where the analysis of [āršt], literally ‘I made’ or ‘have made’, as an “epistolary perfect” is presented; Pardee, Context III (2002) 115.
/YQTLØ/ appears unlikely because the presence of three \{n\}s indicates that there was a vowel between the verbal stem and the energetic/pronominal element.

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

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

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

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

— p. 229 (§42.0), pp. 232-34 (§42.7), pp. 749-50 (§81.4a). The basic form of the presentative particle \{hn\} should not be represented as “*hann(V),” but as /han/, which is usually expanded by the enclitic particle /na— at least the latter process of accretion is what produced the common form written \{hn\} (on the expanding particle \{-n\}, see below remark to pp. 823-25 [§89.1]). The short form of the particle is attested by the spelling \{hbt\}, ‘the
house’, in RS 29.093:16 (KTU 2.70), which consists either of /han/ + /bêta/\(^{491}\) or of a similar basic particle (/ha/, /ha’, or /hal/) plus the noun. I prefer to see here /han/ because that is the deictic particle best attested in Ugaritic (/ha’/) is popular in Aramaic, while Ugaritic /hal/ takes on a local nuance, at least in prose—see above, remark to p. 197 [§33.322.42a], etc.). Strangely enough, T. has no trouble admitting the existence of /han/ in proto-Hebrew and proto-Arabic (p. 229 [§42.11]),\(^{492}\) while denying it for Ugaritic. A clear case of the same particle prefixed to another (set of) particle(s) is found in RS 29.095:14 (KTU 2.71): \{(13) ht åt . - (14) dbr . hmhlkm\(^{1}\) (15) b l< b>k ål tšm\}, ‘You, for your part, don’t worry about a thing’, lit. ‘don’t put any matter at all in your heart’. As has been remarked above (ad p. 63 [§21.355.1a]), emending out the first {h} of {hmhlkm\(^{1}\})} is arbitrary. On the other hand, excluding {hn} from the list of demonstrative pronouns and identifying it as a “Präsentationspartikel” (p. 229) is correct, for not a single example of {hn} used as a demonstrative pronoun may be substantiated.\(^{493}\) I would, however, demur to accept at face value T.’s assertion that {hn} in certain passages has a “demonstrativ-identifizierende Funktion” (p. 233) that stands somewhere between that of a pure deictic and a definite article. In the cases of RS 1.002:17\(^{a}\), 25\(^{a}\), 34\(^{a}\), 43\(^{a}\) {hn ß} and {hn ≤r}/{hn .≤r} (KTU 1.40) or of RS 24.277:26’ {bt hn bnš yq” ≤z}, there is no difficulty in taking {hn} as the presentative particle.\(^{494}\) In RS 16.402:31 (KTU 2.33), hn may be an adverb, ‘here’, as in Arabic;\(^{495}\) in

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

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

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

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

\[^{495}\text{This was the solution preferred by T. in UF 26 (1994) 476, abandoned here (p. 738 [§81.11a], cf. p. 233 [§42.73]) then preferred again (p. 833 [§89.31], with a question mark but without cross-reference or indication of the preferred analysis; in JSS 46 (2001) 24, T. presents the analysis as quasi-articular without even mentioning the possible analysis as an adverb.}\]
lines 37-38, *hn ḥlm śwm* may well begin a sentence rather than ending one, as Tropper would have it, ignoring line 39.\(^{496}\) It is, in any case, certainly neither a demonstrative pronoun nor a definite article in these texts. But what, one may ask, is the function of a presentative particle other than “demonstrativ-identifizierende”? *h(n)* functions in Ugaritic either as a demonstrative pronoun/adjective (for which there is no real evidence), a presentative particle (i.e., a demonstrative adverb), or else it has developed into a true definite article. The real question is whether that particle has in Ugaritic come to be used frequently enough before nouns in any syntactic function to be termed articular. Such is certainly not the case in standard Ugaritic. The thrice-repeated formula (*hn bnš hw*) in RS 96.2039 (see above, remarks to p. 137 [§33.112.31], etc., and to pp. 211-13 [§41.31]) makes one wonder, however, if the queen who dictated that letter did not speak a language/dialect where such a usage was already well developed (twice in that text the sequence *w ht hn bnš hw* is attested, i.e., with two presentative particles before the noun and followed by *hw* functioning as a demonstrative adjective). It should be remarked in this respect that the use of the deictic particle as a definite article did not develop nearly so far, in terms of regularity of usage, in Phoenician as it did in Biblical Hebrew and Arabic (a similar development occurred in Aramaic, with its post-positive particle, but the specific articular function degenerated fairly rapidly there), and one could easily imagine Ugaritic developing along the lines of Phoenician in a relatively short time with either /han/ or /hanna/, or, for that matter, the most basic particle /ha/, at the origin of the development. What can be said with some degree of certainty, however, is that neither /han/ nor /hanna/ had in Ugaritic lost its presentative function to slip towards that of a definite article—there are simply too few cases to support such a claim.

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

---

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

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

— p. 230 (§42.4), p. 786 (§83.113i), p. 840 (§91.12e). Against the analysis of {hnhmt} in RS 15.128:8 [KTU 3.3]) as a demonstrative pronoun/adjective stands the form hnmtn in RS 94.2965:20 (b šdm hnmnt, ‘for these fields’). Given this form, it is perhaps better to take [hnhmt] as consisting formally of the presentative particle plus the 3 m.pl. independent pronoun, oblique form. The passage w mnm šalm dt tknn ‘I ’rbnm hn hmt tknn (lines 5-9) would thus be translated literally: ‘And whatever investigators (see above, remark to p. 178 [§33.231.1b], etc.) should appear, against the guarantors, even these, they should appear’ (/wa mannama ša’a’álūma dūti takūnūna ‘alē ‘urūbānīma hanna humati takūnūna/). Or, in line with T.’s analysis of šalm as non-personal: ‘And whatever claims may arise, against the guarantors, even these, they should arise’ (/wa mannama ša’a’álūma dūti takūnūna ‘alē ‘urūbānīma hanna humati takūnūna/).

If hnmtn in RS 94.2965:20 is a demonstrative pronoun/adjective in the narrow sense of the word, one will note that it is not built from the well-known stem hnd-, but consists of hn + enclitic -m + enclitic -t.

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

In RS 29.095:9 (KTU 2.71), hnk tšm’t is better interpreted as meaning ‘hear this’ than as ‘listen there’ (where else would they listen?).

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


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

500 T. takes tšm’t as a noun, which makes his interpretation of hnk plausible in the context thereby created, but that analysis is not to be preferred (see below, remark to p. 270 [§51.45w], etc.).
(29)\textsuperscript{501} shows that the masculine singular nominative form was /du/ or /dû/, probably the latter (there is no reason to see here the use of \{ú\} as a disambiguater for the first sign, as is the case with the prepositions \{le-e\} and \{bi-i\}\textsuperscript{502} and that the masculine singular was probably declined according to the three principal cases, for, if only a single form was in use, one would not expect the nominative to have been preserved;\textsuperscript{503} (2) both d and dt are attested modifying feminine singular and masculine and feminine plural nouns; (3) dt is never attested modifying a masculine singular noun \textsuperscript{504}. A simple explanation, but one that results in a rather high incidence of identical forms, would say that the original forms /dªti/ (feminine singular genitive) and /d¨ti/ (plural oblique)\textsuperscript{505} were confused with /dª/ and /d¨/ + the enclitic particle /-ti/; the other forms in the paradigm would as a result of this re-analysis have been dropped while perception of /-ti/ as the particle would have permitted that element to be dropped or retained as a matter of style. The paradigm would then be:

<table>
<thead>
<tr>
<th>Masculine singular</th>
<th>Feminine singular</th>
<th>Plural</th>
</tr>
</thead>
<tbody>
<tr>
<td>/dª/, /d¨/, /d≠/</td>
<td>/dª(ti)/</td>
<td>/dª(ti)/</td>
</tr>
</tbody>
</table>

Another possible set of developments leading to a single paradigm, one with a lower incidence of identical forms, would be this: (1) in proto-Ugaritic, as in Akkadian,\textsuperscript{506} the feminine and plural pronouns \textsuperscript{507} (cf. Akkadian /sāt/ and /sūt/) had lost their case vowels; (2) the loss of the case vowels allowed a variant form without /-t/ to develop (these variants might have arisen in speech through various assimilations of the /-t/ to the first consonant of the following word); (3) the vowel of the secondary forms would have been short (in proto-Northwest Semitic all theoretically long vowels in closed syllables become short, hence /dātu/ \rightarrow /dāt/ \rightarrow /dat/ and /dūtu/ \rightarrow /dūt/ \rightarrow /dut/); (4) the forms with /-t/ were homogenized by paradigm pressure to forms with the enclitic particle /-ti/\textsuperscript{508} (this last step appears necessary

\textsuperscript{501}Nougayrol, \textit{Ugaritica V} (1968) text 137.

\textsuperscript{502}See above, remark to p. 52 (§21.341.21c), etc.

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

\textsuperscript{504}I know of no exceptions to this view espoused by T. Loewenstamm once proposed (\textit{Tarbiz} 23 [1958-59] 80; cf. \textit{Comparative Studies} [1980] 69) that šd ūbdy ilšm’t dt … (RS 12.006:1-2 [\textit{KTU} 4.110]) was a singular formulation, but T. more plausibly sees here a triple construct chain with a plural as the head element: “Lehensfelder von ON, die…” (p. 843 [§91.312]).

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

\textsuperscript{506}This and other Akkadian data evoked below are described in \textit{GAG}, §46.

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

\textsuperscript{508}On this particle, see T., p. 836 (§89.5).

\textit{Archiv für Orientforschung} 50 (2003/2004) online version
to account for a sequence such as {dt ṯg̱rk} in RS 16.078+:22 [KTU 2.23], where /dut/ + /taḡgurrûka/ might easily have become one word, written with a single {t}). These hypotheses permit a single paradigm, according to which only the masculine singular was declined for case, but the feminine and plural forms were distinguished both from each other (by vowel quality) and from the masculine singular (by vowel quantity) and had stylistic variants with-/ti/:

<table>
<thead>
<tr>
<th>Masculine singular</th>
<th>Feminine singular</th>
<th>Plural</th>
</tr>
</thead>
<tbody>
<tr>
<td>/dʌ/, /dɑ/, /dɪ/</td>
<td>/da(t)i/</td>
<td>/du(t)i/</td>
</tr>
</tbody>
</table>

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

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

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

— p. 235 (§43.12), p. 262 (§51.43k), p. 637 (§75.514), p. 704 (§76.521.3). Rather than taking the persons qualified as ‘rk b’l ḫlb dt l ytn šmn (RS 24.292 [KTU 4.728]) as ‘offering-preparers who have not given oil’, it appears more plausible to see ‘rk as designating, as in Hebrew, a type of tax, which here would have consisted of oil. If that be the case, ‘rk would be a /qîlt/ or /qâlt/-base noun, rather than a /qâtil/-base verbal substantive and the l would be ‘emphatic’, rather than negative: /(1) āarak b’lî (2) ḫalbî dûti la yutanî (3) šamnu/ “RK-taxes for Ba’lu of Aleppo that were properly paid in oil by…” (+ list of PNN).510

---

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

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

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

— p. 239 (§44.112), pp. 240-41 (§44.241). T.’s dogged determination to present every possible explanation reaches new heights in these two paragraphs, where the interrogative particle {mn} is explained in the first as personal (‘who?’) in the second as impersonal (‘what’). The first is introduced by “vielleicht,” the second by “wahrscheinlich”; virtually the same texts are quoted and translated in both. In my estimation, the evidence favors the latter interpretation and it is possible that {mh} and {mn} exist side-by-side in Ugaritic as impersonal interrogative pronouns because the latter is an expansion of the former (/mah/ + /na/ → /manna/). T. is obviously tempted by the analysis of mn as personal by the comparative Semitics situation, where man(nu) is personal (p. 238 [§44.11]). What is important to note in this respect, is that the Ugaritic and Canaanite situation does not show just the opposite vowel for the personal and impersonal interrogative pronouns as compared with other Semitic languages, but fundamentally different forms: a basic stem /mē/ for the


512If the singular were [lh], the dual would have been written [lhm] (Vita, OLP 28 [1997] 33-41).

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

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

515T. refers to the particle in his second interpretation as “eine erweiterte Variante von mh” without proposing a specific derivation.
personals and a stem /mah(a)/ for the impersonals, i.e., with consonantal /h/. Whatever the case may be for comparison with the other Semitic languages, the two possible interpretations of Ugaritic mn(m) could have been presented without creating two largely redundant paragraphs.

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

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

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

have been: personals {mnk(m)} /mīnaka(ma)/ and, perhaps, {mnn} /mīnana/;\footnote{This form would appear only in the address of the school letter RS 16.265:2 (KTU 5.9), where {mnn} may be a proper name. I leave entirely out of consideration [mrmmnn] in RS 24.271:22 (KTU 1.123) because, despite T.’s conviction that this divine name is “offenbar” to be analyzed as Akkadian māru, “son”, plus “manman ← *man-man” [!], “whoever” (cf. p. 275 [§51.5a]), I remain dubious towards that explanation (see my commentary in Les textes rituels [2000] 703-4).} impersonals {mhk(m)} /mahaka(ma)/ and {mnn} /manna/ ← /mahnama/.\footnote{See now Bordreuil and Pardee, Manuel (2004) I 56, II 179. On the vocalization of the expanding element [-n], see remark below to pp. 823-25 (§89.1).}

— p. 243 (§45.122a), p. 820 (§88.1c), p. 903 (§97.33). As with other parts of RS 29.093 (KTU 2.70), T. is very uncertain about the interpretation of mnm ilt ‘bdk (l. 29), whether to take it as an independent clause (“Was auch immer ist, [das teile] deinem Diener [mit]” [pp. 820, 902]) or as dependent on what precedes (“Meinem Herrn gehört, was auch immer deinem Diener gehört” [p. 903]); on p. 243 and in the first translation on p. 820, these words are translated as a simple phrase (“was es auch immer gibt” and “was auch immer deinem Diener gehört,” respectively). I have proposed that mnm ilt may be taken as an unmarked adverbial dependent on the previous phrase: “… (your servant) will be sure to have a ḫipānu-garment made for my master, of whatever (is required) from your servant’s own goods.”\footnote{Context III (2002) 111. On the interpretation of the preceding lines, see below, three remarks to p. 439 (§73.223.41.8), etc.}

— p. 244 (§45.122b), p. 904 (§97.42a), p. 904 (§97.42). T.’s interpretation of the epistolary formula mnm ẓlm, lit., ‘whatever well-being’, as an indefinite pronoun in a subordinate clause is correct in my estimation, but that overall interpretation has taken a long time to come to the fore.\footnote{For a study of the formula with previous bibliography, see Pardee, Fronzaroli (2003) 446-75.} On the other hand, his interpretation of the formula as ‘following’ the corresponding Akkadian formula must be nuanced: because the Akkadian formula is known only in the West, it may have been borrowed from a West-Semitic language; since it is attested only within the Hittite sphere of influence, it may even have been borrowed ultimately from Hittite—though that is speculation for the moment.\footnote{Ibid., p. 466, n. 66.} The latter solution is also appealing because the syntax of this formula (which basically goes something like mnm ẓlm ‘m X rgm TTB ‘my, ‘whatever well-being there may be with X, may X send back word of that to me’) is not attested in precisely this form elsewhere in Ugaritic. In the case of a Hittite origin, the Ugaritic formula would probably indeed have passed through an Akkadian intermediary stage, for there is not a good deal of evidence for direct Hittite influence on the Ugaritic language.

— p. 244 (§45.122c), p. 821 (§88.21d), p. 822 (§88.22a). The ‘new reading’ (identified as such on p. 821 only) of RS 15.111:18 (KTU 3.2) as “[w ü]nt in[n] bh” will not hold up: the sign after {in} is clearly {m} (tablet collated in June of 2004). Moreover, as T. points out on p. 822, the new reading is not new but old: it rejects that of KTU/CAT in favor of the
The editor’s original reading. The KTU/CAT reading was, however, essentially correct, for on the tablet one finds \([w \, ù\, l\, n\, l\, . \, i\, n\, \, m\, n\, l\, . \, b\, h]\), ‘There is no ùnt-duty at all (attached) to it’.

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

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

— p. 245 (§45.21c-d), p. 845 (§91.314.1). In the first reference cited, T. cites three phrases in which kl, ‘all’, + pronominal suffix is supposed to be used in “absoluter Gebrauch.” The first is the formula ëhkp t il klh (RS 2.[004] v 21, 31 [KTU 1.17]; RS 2.[014]+ vi 13-14\(^{525}\) [KTU 1.3], RS 3.361 iii 1 [entirely restored] [KTU 1.1]) which he interprets as “das ganze göttliche/weite Memphis”; the second is års il klh (RS 2.[009]+ i 65 [KTU 1.6]), translated “die ganze göttliche/weite Erde”; the third in RS 24.255:6-7 (KTU 1.111) ‘srm gdt klhn, translated “(als Opfer) Vögel, (sie) alle zerstückelt/zerteilt.” The usage is compared with Arabic ’al-yawmu kulluhû, “der ganze Tag.” For the first two phrases to be so interpreted, however, one must accept that il is here serving as an expression of the superlative (p. 845), which is not necessarily the case: each time, because the formula is used with respect to the dwelling of a particular deity, il may be interpreted as functioning normally as the common noun for ‘god’.\(^{526}\) As regards RS 24.255:6-7, klhn is not simply appositional to ‘srm in a single clause, it is the second subject of a complex sentence made up of two nominal sentences: lit., ‘(As) burnt-offerings: birds, cut up into small pieces (are) they all (to

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


\(^{525}\)The name for Memphis is here written {hòkpt}.

\(^{526}\)Pardee, Context I (1997) 244 (with explanatory note 19), 255, 269 (with explanatory note 250), 346.
be').\textsuperscript{527} This category of usage of \textit{kl} may, therefore, not be attested in Ugaritic; moreover, several examples of the so-called superlative use of divine names and titles may also disappear from the list (on this question, see below, remark to pp. 844-45 [§91.314.1]).

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

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

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

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

vocalized /'ilm-, /'ulm-, or /'ulum- (or, for that matter, on yet another pattern—see remark below to p. 254 [§51.41c 'ilm]).

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

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

— pp. 248-49 (§51.3a), p. 252 (§51.41a). In these two paragraphs, T. explicitly proposes that āb and āḥ were originally /qatl/-base nouns (on his hypothesis that these two words behaved as simple /qal/-base nouns in the absolute state, see remarks above to p. 51 [§21.341.21a], etc., and p. 175 [§33.215.22], etc.). Though it is indeed plausible, as was remarked above (see note 107), that these two nouns go back to III-w roots, the identification as /qatl/-types is less clear. In my note just cited, I cited the feminine forms /'āhôt/, ‘sister’, and /ḥāmôt/, ‘mother-in-law’, as proof that the elements that produced the /ū/-vowel came before the feminine morpheme and the case vowel could not, for that reason, have been involved in the development of that vowel. Positing a base /qatal/ for these nouns would account reasonably well for the forms /'ābû/ and /'āḥû/, for /'ābawu/, with homorganic /w/ and /u/, could easily have contracted to /'ābû/ already in proto-West Semitic, with analogical alignment of the vowel in the other cases to the standard paradigm (/'ābû/, /'abû/, /'aḥû/). The /ū/ of the Hebrew form /'āhôt/ represents the Canaanite shift, since /'aḥāt/- is attested in Late-Bronze proper names; the development of the feminine form in
proto-Northwest Semitic might, therefore, have been /'aḥawat/- → /'aḥāt-/ (The Arabic form of this noun, viz., /'aḥān/, when contrasted with Ugaritic /'aḥātu/, Hebrew /'āhōt/, and Aramaic /’aḥāt/, illustrates clearly that this noun has undergone severe modification in the singular absolute in that language. It may also be taken as a strong indication that the Arabic masculine form /’aḥu/ may not be considered paradigmatic for the vocalization of the corresponding Ugaritic form — see discussions of ʿā♭ and ʿā♭ cross-referenced at the beginning of this paragraph.) Positing the /qatal/ base for these nouns might also provide a reasonable explanation for the difference in Ugaritic between ʿabû/ʿaḥû and ʿurwu, ‘resin’, for the latter could represent a true /qatil/ base from a III-w root (though proto-Hebrew shows šōrŠ, as though from a III-y root). On the other hand, ʿaḥû, ‘meadow’, seems to represent a proto-form /ʾaḥwû/ (cf. Hebr. ʾāḥāw) which has apparently fallen together phonetically with the word for ‘brother’ (the Ugaritic form of this word should also have been /ʾaḥû/). Finally, I have referred here only to West Semitic, leaving East and South Semitic out of the discussion. It appears, however, that at least for ʿaḥû, the triconsonantal root was retained in Ethiopic529, which would mean that the /w/ dropped independently in West and East Semitic and that each group of languages subsequently underwent its own development.


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

— p. 249 (§51.3b). It must be judged dubious that {dm¯t˘} in RS 16.394:50 (KTU 2.31:46) is to be equated with Akkadian dimtu, “Turm.” The state of preservation of this tablet is poor and its literary classification uncertain. That being the case, the inclusion of data derived solely from this text in a reference grammar must be judged ill advised.

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

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

530 T.’s acceptance of the emendation there to {<t>mtt} has already been criticized above, remark to p. 61 (§21.354.1c), etc.
substantives that include affirmative morphemes indicated immediately thereafter rather than discussing these forms in the sections below devoted to nouns bearing the morphemes in question (pp. 269-74 [§§51.45t-b’ and §§51.46a-k’]).

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

— p. 250 (§51.3l). Since {m•yt} in RIH 78/20:8 (CAT 1.169) designates a place where the suffering one will eat and drink, it probably does not mean “Wassertiefe,” but a place characterized by water; since it is in parallel with a word designating “the heights,” it probably designates “well-watered valleys.”

On the other hand, T.’s proposal that the {y} would be the nisbe ending is certainly plausible (i.e., the stem would have been /mā’-/), a by-form or a secondary form of /māy-/ ‘water’, indeed more plausible with my interpretation than his, and my vocalization should, therefore, have been /mā’iyyāt-/ rather than /ma’iyyāt-/. If T.’s analysis of this word is correct, it would be the only attestation in Ugaritic of what he above (p. 163 [§33.152a], p. 164 [§33.154a], p. 249 [§51.3e], p. 250 [§51.3l]) posits to be the base form of the word, viz., /mā’-/ (elsewhere only {mh} and {my} are attested).

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


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

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

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

— p. 251 (§51.4a). T. does not indicate his reason for stating that Hebrew /ê´hel/, ‘tent’, would be derived from /êahl-/ rather than from /êuhl-/. There can be no doubt, in any case, that proto-Hebrew had /êuhl-/.  

— p. 251 (§51.4a), p. 278 (§52.11). The syllabic data for /ba÷lu/, ‘lord’, are not to be found in RS 20.123+ iv B 18 (Nougayrol, Ugaritica V, text 137), etc., as is indicated in both these sections, but in RS 20.149 iii 14 (ibid., text 130) and in RS 20.123+ iii 30, 33 ([ba-a-lu-ma]). In column iv B, line 17 (!), of the text cited by T., the Ugaritic entry [ba-a-lu] designates the weather deity Ba÷lu (as is correctly indicated by T. on p. 169 [§33.182]).

— p. 251 (§51.4a). To the gloss “Holzkohle” for the noun /pa”mu/ is to be added that of “brownish-red (died wool)”, for the two principal categories of so-called ‘(royal) purple’ are called iqñû, which designates the bluer color, and phm, which designates the redder color. T. regularly translates the textile product as “roter Purpur” (e.g., p. 361 [§62.811]), but for some reason does not include the gloss here—in spite of the fact that the mention of the textile is far more common than in that of coals.

— p. 251 (§51.4a), p. 560 (§74.414.2), p. 769 (§82.36). It cannot be judged at all likely that {qdm} in RIH 78/3+30:24’ (CAT 2.81:25) has the meaning of “Vorseit,” the interpretation proposed on p. 251 with no cross reference to the other two sections cited, where the analysis as a verbal form is indicated with no cross reference back to p. 251.

— p. 252 (§51.4a). T. correctly glosses ṭl ṭl as “Kupfer, Bronze” here, but regularly uses “Kupfer” for that word when translating administrative texts (e.g., p. 355 [§62.41b]). In terms of realia, however, it would appear that copper was used primarily for alloying with tin to form bronze and that most references to the metal would have been to that alloy rather than to pure copper.536

— p. 252 (§51.4a). Whether or not {ànm} in RS 2,[009]+ i 50 (KTU 1.6) be plural, the base form is more plausibly /qâl/ than /qawl/, for the latter identification requires analyzing the attested form as plural and as derived from /’awanîma/ whereas /qawl/ and /qayl/ forms do not show dissyllabic plural stems as frequently as do strong-root forms (/’awñûma/ would have gone to /’ônica/ and been written {ûnm}).537 On the general problem, see above, seventh general remark and remark to p. 188 (§33.311.1b), etc.

— p. 252 (§51.4a), p. 275 (§51.5a). T’s. handling of hollow roots is well illustrated by the words dr and drdr analyzed in these two paragraphs. In the first, dr, “(Familien-)Kreis,” is


537 Below in this very section, T. analyzes {ú-ra-tu} in RS 19.028:6 (PRU VI 126) as the plural of /góru/, “Haut, Tierfell.” This plural would thus be based on the singular stem /gór-/ rather than on a hypothetical plural stem /gawar-/. (On this word, see also third following remark.)
identified as /dôr-/ ← /dawr-/ while, in the second, drdr, “Ewigkeit,” is identified as “/dârdâr/.” The latter is compared with Syriac dûrdûrin while no etymological basis is proposed for the derivation of the first. Hebrew dôr could, of course, come from either /dawr/ or /dâr/ and there dôr dôr always appears as two words. For the single word, Arabic has dawr- and Aramaic dâr. There is, therefore, no way of knowing exactly what the Ugaritic forms were, nor whether the simple noun derived from the same base or from different bases. It can be said that Hebrew dôr and dôr dôr and Aramaic dâr and dûrdûrin (on the morphology of drdr, see below, remark to p. 275 [§51.5a]) can all derive from /qâl/, but I know of no criterion whereby to determine whether the Ugaritic forms were built on the Arabic model (/qawl/), the Aramaic model (/qâl/), or on one of these for one word the other for the other.

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

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

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

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

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

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

the first text is this word\textsuperscript{541} while in the other two ‘back’, however etymologically correct, does not appear to be the proper translation\textsuperscript{542}. In both these cases the word is in parallel with \textit{tmtt}, ‘(body) members’, and \textit{gb} is, therefore, better translated ‘body’ than ‘back’.

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

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


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

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

\textsuperscript{545}Pardee, \textit{Les textes rituels} (2000) 311-14, esp. n. 140 (p. 314).

\textsuperscript{546}It should be remarked that Hebrew shows some strong-root construct forms where /qat/ has gone to /qatl/, but the absolute form has retained the dissyllabic stem (e.g., \textit{yārēk} and \textit{yērek}, \textit{kātēp} and \textit{kētep}).
grammar, the internal and comparative evidence shows that the loss of the second vowel in these forms took place far before Ugaritic existed as a discrete entity, and one may doubt the propriety of including a category /qatl/ ← /qatVl/ in a grammar of Ugaritic. Or, if one wished to do so, one could have a single category /qatl/ with sub-categories for proto-Ugaritic derivations (but, as I have just shown, the /qatVl/ category would be sparsely populated, inhabited by only one dubious example from a strong root and a number of examples from geminate roots).

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


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

550 On p. 555, T. cites Hebrew and Arabic evidence for √RQQ [] used to designate types of bread, but, in contrast with such usages of √DQQ for animals, such forms are usually masculine (the one exception being ruqāqat- used as a nomen unitatis). It should also be borne in mind that Ugaritic had a term gdī that seems to have designated a type of flour (see below, remark to p. 259 [§51.42c]), which may or may not be derived from the root GDL, ‘be big’. Given the comparative evidence for dqt = a type of animal and the absence of comparative data for a feminine word for ‘bread’, one may doubt that gdī and gdlt both designated cereal products.
more plausible number for a sacrificial feast involving the sacrifice of whole animals hence a fairly large number of participants. (3) T.’s argument (UF 33, p. 551) that we would expect explicit terms for ‘cow’ and ‘ewe’ is certainly unexceptionable as an observation, but it applies equally to ‘(loaf of) bread’ and is hence valueless as an argument in favor of one solution or the other. (4) Though it is clear that various foodstuffs were a part of the feast that accompanied many sacrifices, none of these items is mentioned in the ritual texts with the frequency of dqt and gdlt. This point may be demonstrated explicitly for wine (yn) because wine is prescribed as an offering for only one feast (the full-moon festival of the last month of the year) though we happen to have an administrative text that informs us of shipments of wine for a minimum of nineteen sacrificial feasts for some of which we have the prescriptive ritual wherein there is no mention of wine.\footnote{For the details, see Pardee, Les textes rituels (2000), commentary of RS 1.003/RS 18.056 (the two prescriptive rituals for the last month of the year) and RS 19.015 (the administrative text). For a correction to the calculation of the quantities mentioned in RS 19.015 (approximately 1000 liters as opposed to the number of 2000 proposed in Les textes rituels), see Pardee, Topoi 11 (2001) 673, and below, remark to p. 775 ($82.411).} In stark contrast, some two hundred each of the gdlt and the dqt were prescribed in the texts as we know them. (5) T.’s argument that offering the large numbers of bovids and ovids (roughly 500 of the former and 1000 of the latter) “hätten die Ökonomie des Stadtstaates Ugarit wohl überfordert” (UF 33, p. 551) borders on the ridiculous because we know virtually nothing about the temporal spread covered by the texts nor of the numbers of persons who benefited from the sacrifices. If we consider that most of the texts reflect the royal cult, that approximately one tenth of the offerings were consumed entirely in honor of the divinities,\footnote{Idem, Les textes rituels (2000) 911.} and that the sacrificial cult in all likelihood furnished the major source of calories from animal flesh for the palace personnel and, perhaps in some cases, for example the full-moon festival of the last month of the year, for a number of other inhabitants of the city of Ugarit, the numbers do not appear at all out of line. Unfortunately, the type of data provided by the texts from Ras Shamra allows only for generalizations as regards numbers of animals offered and no data whatsoever for the use and distribution of the animals that were not entirely consumed in fire. (6) T.’s argument (UF 33, p. 552) that the ratio of females to males among the bovids (roughly 200/300) as compared with that among the ovids (roughly 200/800) is “nicht realistisch” is unsupported by any comparative data. I have observed that the percentage of females offered as holocaust sacrifices was higher than that of males and that this was a reflection of the value of such offerings, the desire on the part of the offerer being to present the more highly valued animal when it is to be consumed entirely by the deity.\footnote{Ibid., p. 922.} If there is anything to such an argument, one might expect a higher percentage of the more valuable of the two types of animals to have been so offered. (7) Most of T.’s comparative evidence for the importance of bread offerings is from Hittite sources and, in Akkadian, from Emar. Anyone who has studied these two bodies of evidence must have remarked the important differences between them and the Ugaritic cultic texts: there are very few explicit

---

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

\footnote{Ibid., p. 922.}

Archiv für Orientforschung 50 (2003/2004) online version
indications of Hittite influence on Ugaritic cultic practice and, as for the cult of Emar, though the texts are in a Semitic language and the rites were practiced by an ethnic group of certain Semitic origin, it also shows very important differences as compared with what we know from the Ugaritic ritual texts. In contrast, there are very significant similarities in terminology and probably in practice between the Canaanite cult as known from the Hebrew Bible and the Ugaritic cult as known from the Ugaritic ritual texts—and the ideologically important but economically unimportant place of bread in the Hebrew cult is well known (i.e., practically limited to the so called ‘showbread’ in the regular cult and unleavened bread in the Passover festival). If bread held a similar place in Ugaritic cultic practice, there is nothing surprising about its absence from the texts that have come down to us. The best analogy would be the handling of blood: later West-Semitic practice shows that blood was ideologically important and one may surmise that it was not totally unimportant for the Ugaritians—but it is not mentioned even once in the ritual texts. As I have remarked in my conclusions to the study of these texts, the absence of mention of blood in these texts is no doubt owing to the literary genre: these texts prescribe offerings, not techniques for carrying out the various sacrifices mentioned. The biblical texts, on the other hand, belong broadly to the category of legislation in which offerings, offering types, techniques, and distribution of the offerings and sacrificial body parts are all important. It is highly likely that bread, along with wine (see above), was part of the sacrificial feast, but that does not mean that it should necessarily be mentioned in the ritual texts. In sum, I must conclude that, though T. has raised an interesting question, his data and his opinions regarding plausibility are not of a nature to require that the former hypothesis be abandoned on the spot. His first instincts served him better here than has his later bright idea.

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

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

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

554 Ibid., pp. 923-24.
555 For bibliography, see idem, Syria 77 (2000) 51.
objections made with regard to the meaning of the divine category *inš ilm* and the vocalization of the first word, the identification of the Ugaritic word *inš* as a collective noun for 'people, persons' is quite likely. Hence the interpretation of *inš ilm* as a genitive of identification meaning 'people (who are = have become) gods', i.e., the departed dead. As the phrase *ins ilm* is the only basis cited for the assertion that *inš* is semantically more distant from *nšm*, 'people', than one might think (p. 301), the assertion must be dismissed. Hebrew had both *nāšîm*, 'men', and *enôš*, 'people (i.e., a collective)'; why should Ugaritic not have had two similar terms?

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


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

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

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

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

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


559RS 20.149+ iii 10’ {ši-tu} (Nougayrol, *Ugaritica V* [1968], text 130).
If one accepts that the third sign is \{t\}, then the trace following that horizontal wedge can only belong to \{k\}. If someone can find an interpretation of any one of the possible readings \{blt\}, \{blm\}, or \{blq\}, such a reading/interpretation might be preferable to \{Iššlštkš[m]\}.

— p. 255 (§51.41c), p. 404 (§69.172a). It is highly unlikely that \(nš\) in RS 1.012: 5, 11 (\(KTU\) 4.14)\(^\text{561}\) means “Falke,” for that text contains a list of comestibles.\(^\text{562}\)

— p. 255 (§51.41c). I do not see why T. chooses to vocalize \{šbt\}, ‘old age’, as /šîbatu/ \(←\) /šîybatu/, citing Akkadian /šîbu/ as the reason, in preference to basing the vocalization on Hebrew \(≈êbªh\) \(←\) /≈aybatu/, for not only is Hebrew more closely related to Ugarit than is Akkadian but the latter does not express the distinction /i/ vs. /e/ with the clarity that one would wish.

— p. 255 (§51.41c). The same remark applies to the following example adduced by T.: \{int\}, ‘urine’, is vocalized as /tinatu/ following Akkadian /šîn-/ rather than Hebrew /šayn-/ \(→\) /šên-/.

— p. 255 (§51.41c), p. 300 (§53.332). In the first section cited, the Ugaritic word for ‘wheat’ is indicated as a /qitlat/ base; in the second, \{ḥm\} is identified as a noun with a masculine plural ending corresponding to a feminine singular. Neither entry is cross-referenced to the other and the index of Ugaritic words contains only “ḥm” and the appropriate reference to p. 300. One may also question the usefulness of citing \{ḥtt\} under both II-\(n\) forms and geminate forms: is this done throughout the grammar where gemination results from assimilation of /\(n\)/?

— p. 255 (§51.41c), p. 640 (§75.518). T. extracts from RS 17.117:13’ (\(KTU\) 5.11) the signs \{ttnt\}, treating \{nt\} on p. 255 (cf. p. 557 [§74.413.2] on ll. 12'-13’) as the Ugaritic word for ‘fig’ (it would be a secondary form of “\(ti'n(a)t/'\) and “\(ttt(n)t/'\)”), but taking \{ttt\} on p. 640 as a possible form of the verb YTN. Because he does not cite the context of these putative forms, it is impossible to determine to what extent these readings and analyses are based on the incorrect reading of the final sign of the line as \{t\} in \(CAT\) (“\(dbl\) ttnt yt”) However that may be, and one must consider the possibility that the \{t\} is a simple typographical error in \(CAT\) since all previous witnesses have read \{t\},\(^\text{563}\) once one identifies the word yt, ‘asa foetida’, at the end of the line, the beginning of the line is most plausibly read \{d bl ttnt týt\}, ‘that she should not give (me) any asa foetida’ (in such a statement, yt refer almost certainly to the medical product of the plant, not to the plant itself).\(^\text{564}\)

---


\(^\text{561}\)Erroneously cited as \(KTU\) 1.15 on p. 255 and in the index (p. 1033).

\(^\text{562}\)Pardee, \(AuOr\) 20 (2002) 175.


The glosses “Lieblichkeit; Reiz” for /nu‘mu/ are excessive. The root is used in Ugaritic as the general term for ‘good(ness)’. The distribution is the mirror-image of that in Hebrew where n‘m is highly marked, denoting ‘beauty’, ‘pleasantness’, etc., while T˚B is the generic word for ‘good(ness)’. In Ugaritic T˚B is the rare and specific word while N‘M has the broader semantic spread of the two.

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

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

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

— p. 256 (§51.41e). { ż}, ‘goose’, is vocalized /'uţ-/ ← /'uwz-/ on the basis of Akkadian /išu/ and the alternative vocalization /'oţ-/ ← /'awz-/ is considered, but the simplest option is ignored, viz., that this is a /q˚l/-base noun.

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

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

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

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

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

566 The only reference to this text in my files is van Soldt, UF 22 (1990) 348, 350.
— p. 257 (§51.41h), p. 638 (§75.516b). As noted above (remark to p. 57 [§21.352.1 {t} für {m}]), the last word of RS 17.139:31 (KTU 2.34:33) is to be read [{d}d{t}k], 'your dêdu-jars', not [{d}tk], 'your giving birth' (see also below, remark to p. 266 [§51.45e mrt]). The verbal substantive ldt appears, therefore, not yet to be attested in Ugaritic.

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

— pp. 257-58 (§51.42a), p. 272 (§51.46e). In the first section cited, T. identifies [{...}rgb] in RS 24.247+:19 (KTU 1.103) as a /qatal/ noun meaning 'famine'; in the second, {rgbn} in line 5 of the same text is identified as a /qatalân/ noun with the same meaning (though, for reasons unclear, he translates “Hunger” in the first case, “Hungersnot” in the second). He does not mention the strong possibility that the [-n] of the latter form is the affirmative particle, which Hoftijzer identified as appearing on each singular absolute noun in first position of the apodosis in this omen text and which, in this construction, was identified as “-n of apodosis.” If this identification be correct, there is no noun /raĝabûnu/ in Ugaritic, only /raĝabû/. On the other hand, since the text before [{...}rgb] in line 19 has disappeared, one cannot be certain that the noun is present there.

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

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

— p. 258 (§51.42a). The problem of syncope of short vowels in words of three or more syllables comes to the fore in the examples provided here of substantives presumed to come from the /qatalat/ base. T. does not state his reasons for classifying the first four as /qatal(a)t/ and the following five as /qatalat/, though one of each category is, for him, clearly /qattat/, the other /qatalat/: aht he believes to come from /гаhad/ (for the analysis of this as from a /qattalt/ base, see above, remark to p. 142 [§33.115.11], etc.), whereas {tigt/tiqt}, of which the {i} shows the form to have had either /i/ or /o/ after the ‘/' /, would, if the original base were /qatalat/, show syncope of the second vowel: /га’gat-/га’гat-/ /га’агат-/(the reconstruction of this particular form is based on Hebrew /גָּ֑גָ֑ה/). At the middle of the stream where T. changes horses is the word {nqht}, vocalized as /nahaqat-. This appears to provide the key to T.’s organization of this paragraph for, with {tigt/tiqt}, {g’t} and {z’t}, the inconsistency has been noted above in the remark to p. 189 [§33.311.3b], etc.).

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


569 The last two forms are mentioned elsewhere as derived from III-w and III-y roots. Cf. p. 200 (§33.323.4b): /ga’áit/- /ga’ayat-/, p. 199 (§33.323.3a): /za’gát/- /za’ɡawat/ (on p. 189 [§33.311.3b] the reconstruction /zi’gat/- /zi’ɡiy(a)t-/? is proposed, with no cross-referencing of one reconstruction to the other—the inconsistency has been noted above in the remark to p. 189 [§33.311.3b], etc.).
There is no solid basis on which to base these reconstructions as /qatalat/ forms, however, and, to the extent that these hypothetical vocalizations have contributed to the theories of syncope and monophthongization proposed above at phonology, they must be regarded as purely heuristic. As may also be the last example of /qatalat/ given here, viz., /tltm/, for there is no particular reason to believe that this noun, which may or may not mean 'two groups of three', was not formed on the stem of either the cardinal or the ordinal number (cf. below, remark to p. 384 [§67.22]).

— p. 258 (§51.42a). How do Syriac /dunbātā/dānubātā/ establish that Ugaritic {dnbtm} was a /qatalat/ form?

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

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

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

570 RS 2.[003]4 iii 16-18, v 8-11 (KTU 1.14). The first passage contains /tiqt/, the second /tiqṭ/.

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

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

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

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

— p. 259 (§51.42c), pp. 473-77 (§73.42). Because the Ugaritic verbal system, demonstrably in prose but plausibly in poetry as well, is of the West-Semitic type, where /qatal/qatil/qatul/ have become perfective verbal forms, the presentation of /qatil/ as a valid alternative for the vocalization of the Ugaritic passive participle because that is one of the Akkadian ‘permansive’ forms does not merit the consideration that it receives in §73.42, where /qatil/ is indicated as a valid alternative alongside /qatil/qatul/. (On the question of the form of the G-passive participle in Ugaritic, see remark below to p. 473 [§73.422], etc.)

This appears to be proven by the /qatila/ verbal forms that are transitive in meaning, e.g., /la’ika/, ‘he sent’, common in prose and well attested in poetry. It must be admitted, therefore, that in Ugaritic, as in the later Northwest-Semitic languages, one of the adjectives with a long vowel in the second syllable had assumed the role of the G-stem passive participle. What remains uncertain is to what extent the /qatila/qatula/ forms retained the function of statives in Ugaritic, as in the later West-Semitic languages (see remark below to pp. 664-65 [§75.534]).

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

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

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


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


578Cited ibid., p. 804 (texts to be published in extenso by D. Arnaud, cf. RSO XIV 22).
— p. 260 (§51.42j). As a point of etymological connection, virtually all recent scholarship on sglt (≈ Hebr. /s³gullā/) has preferred to compare Akkadian sikiltu, ‘personal possession’, over the only form cited by T., viz., Akkadian sugullu, ‘herd’. Of course, if the two Akkadian words have a common etymology, the distinction loses much of its force: because sglt is used in the West not only in its basic economic meaning but also in political-theological contexts, while sikiltu and sugullu are used almost exclusively in economic-legal contexts in Mesopotamia, sometimes with negative polarity, it is already clear that the words had separate developments in East and West. That being the case, sglt could represent the western semantic development of the word that already in Old Babylonian times meant ‘herd’ in Akkadian.

— p. 260 (§51.42j). T.’s proposal that Ugaritic bkr, ‘firstborn’, would be a secondary /qutul/ form, derived from /qutl/ (/bukr- → [bukûr-]), appears to be based solely on the fact that Hebrew shows /b³kôr/—there is no internal Ugaritic evidence. To the extent that this reconstruction was influenced by the other form classified here, viz., putative /pu³ur-/ , that analogy is to be rejected (see above, remark to p. 169 [§33.181.2], etc.).

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

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


580 See CAD S on sakâlu and sikiltu (pp. 68-69, 244-45).
seventh of days’). Nor is the case of RS 24.248:15 (KTU 1.104) terribly convincing. In that text, the interpretation of b ym ‘šr as “am Tag der ‘Zehnzahl’” is based entirely on the presence of the word ym in the formula, for the usual expression of days of the ritual month omits the word ym. There may, however, be two other cases of ym appearing in such expressions: {b ym ltmn} in line 7 of the same text and {l b ylml [. . lšb ′ l]} in RS 24.266:1 (KTU 1.119)—unfortunately, both passages are damaged and the readings are not certain. Since, however, the point of reference of the formula b + ordinal is the day of the lunar month, it does not appear implausible that the word for ‘day’ may occasionally appear in the formula. I would also query whether the interpretation of šb ′ as a noun in RS 24.256:10 (KTU 1.112) is the best. There the phrase b šb ′ ym ḥdt may be vocalized /bi šabi ′ yamĥ ḥuditi/ and interpreted literally as ‘on the seventh of the days of the new moon’. T. admits that the phrase has an “ordinale Funktion” (p. 386 [§67.43]) and I fail to see why this morpho-syntactic analysis based on the interpretation of šb ′ as a substantivized ordinal number should not be preferred. Finally, I see no need for the nominal analysis of the number words in a passage in the Kirta text, ḥms . . ṭdt yrhm (RS 2.[003] ii 30-31 [KTU 1.14]. For T., this must mean ‘for five . . six months’, and both number words are taken to be nouns from the /qatul/ base. It appears just as plausible, however, that the author was expressing the length of Kirta’s campaign by its extent in time (‘for the fifth . . the sixth month’), rather than by the total. The syntax would, of course, be that of the substantivized adjective in construct with the plural noun (‘for a fifth . . a sixth of months’). T. deems it impossible that ṭdt should be an ordinal number modifying yrhm (p. 385) without considering the possibility of genitival syntax. The vocalization would be /ya′pi lāhma dā ḫamīši // maqdā ṭaditi yaraḥima/, ‘He must bake bread for the fifth (month), fine foods for the sixth month’ (lit. ‘he must bake bread of the fifth, fine foods of the sixth of months’). — p. 261 (§51.43d), p. 349 (§62.110.2), p. 386 (§67.51). Though the reading of {r} at the end of RS 2.[004] ii 45′ (KTU 1.17) may well be correct, the restoration as {[′š][r]} is highly

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

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

583 Ibid., pp. 662, 665, 670.

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

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

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

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

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

— p. 262 (§51.43k), p. 478 (§73.431c). With regard to T.’s translation of /nd/ as “Hirte,” the same as for /rym/ (p. 263), I can only repeat what I once wrote apropos the English translation of /ndm/ by “shepherd”:

“The facts that (1) the /ndm/ are listed alongside /khnm/, ‘priests’, in the lists of personnel and (2) the high state official and scribe ‘Ilīmilku describes his master ‘Attānu-purulini as /rb ndm/, ‘chief of the /ndm/’ (CTA 6 vi 55), indicate that these persons were not the simple equivalents of /rym/, ‘shepherds’ (p. 426, cf. p. 494, n. 240, p. 514). If the association with animal husbandry is accepted on the basis of apparent attestations of the word in texts in other languages, these persons must have been major players in the livestock business, perhaps, given the association with /khnm/, suppliers of the royal sacrificial cult, which consumed large numbers of beasts (Pardee 1997a: 273, n. 283; Pardee in press: ch. 83). There is, however, no specific evidence from Ugarit permitting a precise definition of their functions there.”

582Ibid., pp. 35-39.
584The references are to Context I (1997) and Les textes rituels (2000).
— p. 262 (§51.43k). I know of no evidence proving that Ugaritic psl means “Steinmetz” (for
the specific cases of the psl ḫzm, ‘arrow makers’, and the psl qṣṭ, ‘bow makers’ see below,
remark to p. 478 [§73.431d]).
— p. 262 (§51.43k). As hypothesized nearly two decades ago by J. Wesselius,590 and
accepted by Huehnergard591 and van Soldt,592 RS 14.084:14 (KTU 4.126:31) probably
does not contain a professional title tknm, which is unattested elsewhere, but a gentilic
tknym, ‘men of the town of tkn’.593
— p. 263 (§51.44a). With regard to T.’s hypothesis according to which ‘wr, ‘blind’, would be
‘/awwir-/ in Ugaritic and that Hebrew ‘/iwwēr/ is a secondary form of the /qattîl/ base, it
may be remarked that proto-Hebrew already had /qittîl/, for, in the proper phonetic
environment, the actual Hebrew forms show the expected variant of proto-Hebrew /i/ in
the first syllable, e.g., /hērēš/ ← /hîrîrīš/, ‘dumb’.
— p. 263 (§51.44e), p. 478 (§73.431c). In the first paragraph cited, hrt, ‘plowman’, is
identified as a /qattîl/ formation, in the second, as a substantivized participle. T. is not
thinking of different words in different passages, for the same text is referenced in both
paragraphs (RS 3.325+ iii 12 [KTU 1.16]). In these same two paragraphs, hrūš, ‘artisan,
builder’, also undergoes the same divergent analyses, though here different texts are cited
(hrūš alone is identified as /qattîl/, hrūš in construct with a second term as the participial
form)—there is no reason that I can see, however, to prefer the different forms in the
different contexts.594
— p. 263 (§51.44e), p. 450 (§73.243.22). In the first paragraph cited, T. takes the first word
in RS 18.113A+B:39’ (KTU 2.42:27) as mkr, ‘merchant’, whereas in the second he accepts
the reading of the line that I proposed in 1987, viz., [lālmrmkn],595 which he translates “ich
werde/will verkaufen.” As regards the reading of the first sign, in preparing the hand-copy
of this text for the edition of the Ugaritic letters which I am preparing, I was unable to
confirm the trace of another wedge to the left of the one clearly visible, and the reading
[lālmrmkn] now appears more plausible to me than the one I formerly proposed.596 The
meaning in this case would be ‘they will sell (them—viz., the boats)’ = /tamkurūn/a.
— p. 263 (§51.44e), p. 472 (§73.411). As noted above (remark to p. 178 [§33.231.1b],
etc.), the word šāb, ‘drawer of water’, does not exist in Ugaritic. In RS 17.073:2 (KTU 6.25)
the correct reading is {šal[...]}, whereas šīb, viz., /šā’ibu/, is clearly attested in RS

593 See my re-edition of the tablet, Semitica 49 (1999) 59-64. The tiny trace of a sign after [n], which
the authors of KTU/CAT took for a [m] may as well belong to [y], which permits the reading {tknl[y][m]}.
594 On p. 263, {ḥa-ra-ṣu} in RS 20.189:7 (Laroche, UF 11 [1979] 479) is cited in support of /ḥarrāṣu/
“Handwerker.” Cf. p. 481 (§73.513.1), where {ḥ[a]-ra-š[u]} in RS 20.123+ iii 18’ (Nougayrol, Ugaritica V
[1968] text 137) is also cited for the vocalization and meaning of this word.
595 UF 19, pp. 206, 209.
596 See the pertinent textual remark in Les textes épistolaires (in preparation).
19.016:15 (KTU 4.609), as is generally recognized. (T. does not include šib under /qātil/ substantivized forms on p. 262 [§51.43k], as its use in RS 19.016 would indicate to be the correct functional analysis,597 but as a participial form on p. 472.) — p. 264 (§51.44c). In refuting the hypothesis that the Hebrew and Ugaritic nomen professionalis was /qattal/, instead of the common Semitic /qattal/, T. omits to point out that Biblical Hebrew actually has two forms, one typified by gibbôr, ‘hero’, the other by hāṭṭāl ( ), ‘sinner’. The former has assumed the form expected for Hebrew/Canaanite (disimilation of the /a/, shift of /ā/ to /ō/), whereas the latter shows neither of these changes. In the latter, however, the /ā/ is irreducible (cf. ḫāṭṭāʾōy, ‘sinners of’), which shows that the qames in this category of nouns represents historical /ā/. This leads to the conclusion that all such words were relatively late Aramaic loan-words into Hebrew, a conclusion that historians of the Hebrew language and of Israelite culture need to consider, either to refute or else to examine for its implications for Hebrew antecedents. Whatever the upshot of that may be, the weight of the evidence, I believe, favors the hypothesis according to which the Hebrew /qattal/ forms had a /qattal/ base.598
— p. 264 (§51.44i), p. 478 (§73.431c). In the first paragraph cited lmd, “Lehrling, Schüler,” is identified as a /qu/itt¨l/ formation, with the alternative of /qattal/; in the second, it is classified as a substantivized participle, though it is not specified whether the participle was active or passive. The semantics of the word would lead one in the direction of a passive form, but the /qattal/ base normally denotes active occupations, so T.’s thinking here is not quite clear. As so often in such cases, the different categorizations are not cross-referenced.
— p. 265 (§51.44m), pp. 282-84 (§52.4). Unmentioned in either the paragraph on nmrt, “heller Glanz,” or in the section on the feminine morpheme {-y} is the word nmry in RS 18.113A+B:9, which some scholars interpret as cognate with nmrt 599 rather than as designating Amenophis III.600 More plausible, however, than taking nmry as a feminine

598The retention of the qames in plural construct forms appears to me to outweigh the datum that may be taken to indicate that the proto-vowel was short, viz., the fact that some of these forms show a m.s. construct form with patah. The development of an analogical construct form (i.e. like /dābār/ → /ḏbār/ in construct) appears a more likely secondary phenomenon than the retention of qames in an open pre-pretonic syllable, something that occurs regularly only in this class of nouns and in the active participle of hollow roots (e.g., qāmēy YHWH, ‘those who rise up against the Lord’). See now Fox, Semitic Noun Patterns (2003) 258-59, who, like Tropper, prefers to derive the Hebrew nomen professionalis from the /qattal-/ base, and my discussion of the problem in my review of Fox’s work, forthcoming in JNES.
600Virolleaud, CRAI 1955, p. 75; idem, PRU V (1965) 15. In 1962, Liverani expressed his dubiety (Storia di Ugarit nell’età degli archivi politici [StS 6; Rome: Pontifical Biblical Institute, 1962] 28, n. 6) regarding Virolleaud’s Egyptian interpretation of nmry, but in his article on the history of Ugarit in the Supplément au Dictionnaire de la Bible (vol. 9 [1979] 1298) he judged the identification probable. Over the years, Virolleaud’s interpretation has been predominant; the prestige of van Soldt and Singer may swing the balance in the other direction. The problem in the identification with Amenophis III is the very existence of a letter in Ugaritic that dates to the early fourteenth century. Other than the phrase here under discussion, nothing in the letter bears resemblance to other correspondence with Egypt (one group of interpreters takes

Archiv für Orientforschung 50 (2003/2004) online version
by-form of \textit{nmrt}—because \textit{nmrt} is very likely an Akkadian loan-word into Ugaritic—would be the analysis of the \{-y\} as the enclitic morpheme attached to a masculine by-form of \textit{nmrt}.\footnote{The most appealing candidate semantically speaking is \textit{namrirrû}, a \textit{plurale tantum} meaning “supernatural, awe-inspiring luminosity” (\textit{CAD N} 1, pp. 237-38), “schreklicher Glanz” (\textit{AHw}, pp. 728-29); but the Ugaritic form cannot, of course, have been identical since it is written with only one \{-r\}.} The phrase is \textit{rgmt} \ldots \textit{nmry mlk \'lm}, ‘I (hereby) pronounce (to various deities) the splendors of (your) eternal kingship’ (perhaps something like /ragamtu \ldots namurriya mulki \‘ālami/).\footnote{This explanation appears more plausible also than Rainey’s suggestion (reference note 599), offered without translating the passage, to take the \{-y\} as the \textit{nisbe} ending: ‘I pronounce \ldots the glorious one \ldots ’ does not appear to fit the context of an epistolary blessing.} The use of enclitic \{-y\} in a construct chain is well illustrated by \textit{ily úgrt} in RS 15.008:4-5 (on which, see above, note 88).

— p. 265 (§51.45a). In addition to the /’/-preformative proper nouns listed here (\textit{áqht}, \textit{árwð(n)}, \textit{ádd}, and \textit{áhnapu}), the Ugaritic for ‘Ashqelonite’ is now attested: \textit{átqlny} in RS 94.2392*:13 (unpublished).

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

— p. 266 (§51.45e). One may doubt that \{mih[d\ldots\]} in RS 11.779:1 (\textit{KTU} 4.81) is a common noun meaning ‘port’ while \{mâh[d\]} is the same word used as a proper noun: \{mih[d\]} may be the place name in its basic form /ma’had\-/, while \{mâh[d\]} may show the variant pronunciation /ma’ahad\-/ (see above, remark to pp. 33-35 [§21.322.1]).\footnote{Van Soldt has demonstrated that \{m•h[d\]} and \{m±h[d\]} are variant forms of the same place name (\textit{UF} 28 \[1996\] 675-76).} There is, in any case, no obvious reason why \{mih[d\ldots\]} in RS 11.779:1 should not be a place name.

— p. 266 (§51.45e). The indication “[he. mæşsæb]” after the definition “eine Weinart” for Ugaritic \textit{mšb} is misplaced, for Hebrew /maššāb/ is not attested for this meaning, but only for the first meaning of Ugaritic \{mšb\}, viz., “Ständer, Gestell,” i.e., there is no Hebrew word /maššāb/ that designates a type of wine. Indeed, the etymology of \textit{mšb} as a designation for a type of wine is uncertain\footnote{Pardee, \textit{Les textes rituels} (2000) 515-18.} and classing \textit{mšb}, “eine Weinart,” here without a question mark is misrepresentative of the present situation in Ugaritic lexicography.\footnote{T. himself once recognized the fluidity of the situation, mentioning an explanation based on a \textit{I-w} root (\textit{Kausativstamm} \[1990\] 176-77).}
— p. 266 (§51.45e). The entry for m trăm, “Griff,” should be moved up from the I-N root section to the strong root section.

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

— p. 266 (§51.45e). Though one may applaud the use of two question marks, T.’s classification of [mrt] in RS 17.139:30 (TU 2.34:32) as a m-preformative noun from WR, ‘to inherit’, cannot be accepted. The text reads {(30) h n, m rt d št (31) aššu b [dltk]}, and the two lines clearly constitute a syntactic unit of which the first line cannot mean “the estate is … settled,” as Dijkstra has proposed—the interpretation accepted here by T.606 If the reading of the last word in line 31 be accepted (see above, remark to p. 257 (§51.41h)), the reference is to ‘jars’ and mrt must, as most scholars have believed, refer to some kind of agricultural product. The translation would be: ‘Behold, the mrt which you stipulated, I will have (it) sent out in your dd-jars’. Below, p. 589 (§74.62.3), T.’s translation by “Most,” though in contradiction with the entry just discussed, better represents this consensus. It is highly unlikely, however, that mrt designates ‘must’ in the narrow sense of the word.607

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

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

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

606 UF 19 (1987) 47.
608 My dictionary indicates two primary meanings for Sendung, “consignment,” and “mission.” Neither of these fits the usages of the Ugaritic word, as we shall see.
609 It is equally clear from this and other passages that mlâkt does not simply mean “message” (Watson, JNSL 25/2 [1999] 1-2, n. 2).
The reading \{m\textsuperscript{h}r\textsuperscript{t}\} is, in my estimation, beyond doubt\textsuperscript{611} and \{m\textsuperscript{h}r\textsuperscript{t}\}, which would be attested only in this passage, has, therefore, no place in a grammar of Ugaritic. Moreover, the meaning of “Ackerland” proposed by T. for \textit{m\textsuperscript{h}r\textsuperscript{t}} does not fit RS 6.028 and, for that reason, we proposed that the word means ‘plow’ in the stele inscription, as opposed to ‘plowland’ in the mythological text where it is attested.\textsuperscript{612} If these interpretations be correct, it may not be judged certain that \textit{m\textsuperscript{h}r\textsuperscript{t}}, ‘plow’, and \textit{m\textsuperscript{h}r\textsuperscript{t}}, ‘plowland’, were both /maq\textsuperscript{t}al(a)t/ forms, i.e., the semantic distinction may have been expressed by different vowel patterns.

Because T. usually indicates in parentheses the proto-root of a Ugaritic form that contains a secondary consonantal element, one is surprised that the root of Ugaritic \textit{mdb} is indicated simply as “√dwb” (cf. Hebrew ZWB).

One is also surprised to see T. proposing a single etymology for Ugaritic \textit{m\textsuperscript{s}mnt}, ‘treaty’, viz., √SMM, citing Arabic. Others have thought the word to be derived from √SMD, with assimilation of the third root consonant to the feminine morpheme, something like /ma\textsuperscript{š}madtu/ → /ma\textsuperscript{š}mattu/.

T. misses one of the key elements of the structure of RS 2.

On p. 477, T. presents the possibility that \{m\textsuperscript{r}h\textsuperscript{t}\} is a G-passive participle, but prefers the analysis as a /maq\textsuperscript{t}ª ˜l(a)t/ formation, citing Akkadian \textit{mar\textsuperscript{\texttt{\texttt{u}}}}\textsuperscript{t}tu as the basis of this reconstruction. That comparison misses the point of the Ugaritic lexeme, however, which appears to be a denominal verb from a noun such as Akkadian \textit{ter\textsuperscript{\texttt{h}}}at\textsuperscript{u}.\textsuperscript{614} As such, it will have had its own internal usages independent of Akkadian formations built from the root \textit{re\textsuperscript{\texttt{\texttt{u}}}}. Thus the sequence \textit{m\textsuperscript{t}r\textsuperscript{h}t} … \textit{m\textsuperscript{\texttt{\texttt{t}}}ltt} through \textit{m\textsuperscript{s}b\textsuperscript{t}}, with the number ‘two’ implied but not expressed, describing \textit{Kirta’s} seven brides, is solidly built on a sequence of morphologically identical forms (/mut\textsuperscript{\texttt{\texttt{a}}}\textsuperscript{h}\textsuperscript{\texttt{h}}\textsuperscript{\texttt{a}}\textsuperscript{t}tu/ … /\textsuperscript{\texttt{m}}\textsuperscript{\texttt{\texttt{u}}}\textsuperscript{\texttt{\texttt{a}}}\textsuperscript{ll\textsuperscript{\texttt{a}}}\textsuperscript{\texttt{a}}\textsuperscript{t}tu/ … /\textsuperscript{\texttt{m}}\textsuperscript{s}b\textsuperscript{bba\textsuperscript{\texttt{a}}}\textsuperscript{t}tu/).\textsuperscript{615}

\textsuperscript{610}In \textit{KTU}, one finds \{m\textsuperscript{h}r\textsuperscript{t}\}, in \textit{CAT} \{m\textsuperscript{h}r\textsuperscript{\texttt{\texttt{t}}}\textsuperscript{\texttt{t}}\}.


\textsuperscript{612}On the contrast between RS 6.028 and RS 2.


\textsuperscript{614}\textit{TRH}, ‘to marry’, is clearly not a recent borrowing from Akkadian, for the \{\texttt{h}\} shows it to be an old West-Semitic root (a recent borrowing would show \{\texttt{g}\}). On the other hand, its meaning makes it difficult to separate it etymologically from Akkadian \textit{ter\textsuperscript{h}}at\textsuperscript{u}.

\textsuperscript{615}Despite citing the history of the interpretation as a passive participle (\textit{UF} 31 [1999] 146, 156) and being aware of my structural interpretation of the passage in \textit{Context I} (reference note 613), Dietrich and Loretz adopt another structural interpretation without bothering to refute mine (ibid., pp. 133-64). Because their
misapprehension of the structure of this section, see remark below to p. 364 [§63.112], etc.).

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

— p. 270 (§51.45u). Given that the form in Massoretic Hebrew that corresponds to Ugaritic {t±nt} has a strong /≥/ (the Hebrew form is ta≥aniyyªh), it must be considered highly unlikely that {tânt} represents /tânít-/, i.e., that the /‘/ has quiesced and the {±} is used as a mater lectionis, as T. proposes here. It is equally unlikely that the by-form {t°nt} represents the /a‘/ → /ō/ shift, with the {°} here also used as a mater (on this subject, see above, remark to pp. 33-35 [§21.322.1]).

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

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

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

interpretation passes over without comment what I consider to be the key to the structure of the passage (/mutarra”atu … mu®alla®atu …/), I do not find it plausible.


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

— p. 270 (§51.45w), p. 444 (§73.233.4), p. 645 (§75.522). Though the word \{tin(t)\} (RS 2.[004] vi 40' [\textit{KTU 1.17}]) is valuable in that it shows explicitly that the Ugaritic word for 'woman' is from the root \( \text{'NT} \), it is hardly plausible that it is the plural of \{ātti\}, as T.'s translation "Frauen" (p. 270, with a question mark) indicates his analysis to be (in the other two paragraphs cited, he leaves open the possibilities of \{tin\} being a singular or a plural). As most \( t\)-preformative nouns are abstracts, the first assumption must be that the word is singular and that it means something on the order of 'womankind'. T. lists the form under /taqt\( V \)1/ but does not propose a specific vocalization. Because of its frequency in the Semitic languages, /taqt\( \text{lat/} \) is the most plausible, but all that one can say with some certainty is that the writing with \( \{i\} \) makes it plausible that the first syllable was closed.

— p. 270 (§51.45x). If one is to compare Hebrew \( t\text{'yr\( ñ \)} w\( ñ \) \) with Ugaritic \{tr\}, as does T. here, one must conclude that the base form was not /taqt\( ál/ but /tiqt\( ál/ and that the Ugaritic word was not "\( h\text{'r\( át/} \) but /\text{'r\( át/-}. The Eblaite form \{ti-ri-\( ū \) \} that T. cites is, however, closer to [\textit{DINGIR.SIR}I\( Š \)] the Sumerian equivalent of Akkadian \( \text{Sir}âšu \), with which the Ugaritic divine name \( trt \) is equated in RS 94.2004:25.620

— pp. 270-71 (§51.45z). One fails to understand why, when in his introduction to this paragraph T. proposed that \( t\)-preformative nouns from III-\( y \) roots are either /taqt\( i\)yy-\( / \) or /taqtiyy-\( / \), he proposes only the vocalization "\( t\text{ū\( ū \)iy\( ā \)h/} for \{t\( ū \)yt\} when the Hebrew tradition shows /\text{ū\( ū \)šiyy\( ā \)h/.

— p. 271 (§51.45a'). T. gives no explanation for treating "tagabbir-" under the heading /tuqatt\( ál/.

— p. 271 (§51.45e'), p. 609 (§74.645), cf. p. 671 (§75.537g), p. 868 (§93.352). Because of the absence of reference to cultic prostitution elsewhere in Ugaritic literature, a translation of \( m\text{š\'l\( tm \) \) in \textit{RS 02.002:31 (\textit{KTU 1.23}} less explicitly sexual than "(\text{zwei}) Prostituierte/Dirnen" is preferable. On the other hand, recognizing the non-divine nature of these 'women' is, in my estimation, plausible, indeed to be preferred.621

— p. 271 (§51.46a). The context of \{...rh\( bn \} in \textit{RS 16.394:67 (\textit{KTU 2.31:63}} is too damaged for us to know whether it is an adjective meaning "weit, breit," the river name \( \text{Rahb\( ānu \), or, for that matter, a verbal form. Contrary to T.'s opinion, the editors of RS

---

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


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

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

— p. 272 (§51.46d). T. classifies \(ḥtbn\), ‘account’, as /qutlān/ on the basis of Arabic /ḥusbān/- without mentioning that Hebrew /ḥešbōn/ might favor /qitlān/ (depending on one’s view of the origin of the \textit{seghol}).

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

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

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

\textsuperscript{622}Caquot and Dalix, RSO XIV (2001) 397.

\textsuperscript{623}Virolleaud, \textit{Danel} (1936) 186.

\textsuperscript{624}\textit{CTA} (1963) 80 n. 3.

\textsuperscript{625}AuOr 16 (1998) 292.

\textsuperscript{626}What looks like a very short horizontal wedge appears just to the right of the second vertical line on the photograph published in the \textit{editio princeps} (see note 623) and reproduced in \textit{CTA}; it is still visible on the tablet today. Its length appears, however, insufficient for it to be qualified as a full wedge and the appearance of a head of a wedge must be owing to a grain of sand that was part of the temper of the clay.
The word *gpn* is generally taken as denoting some part of a riding animal’s paraphernalia, not the animal itself (T. translates “Reittier”).

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

As perceived by van Soldt,629 */tmrym/ [tmrmy] in RS 14.084:3’ (*KTU 4.126:20) refers to persons from the town of [tmr], not to “Dattelpalmenzüchter, -bearbeiter,” as T. translates here.630

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

---


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


630 See my re-edition of the text in *Semitica* 49 (1999) 59-64.
is not limited to a single base form like /fu' Iay/ (see below, remarks to p. 276 [§51.5c] and to pp. 282-84 [§52.4]).

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

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

— p. 275 (§51.5b), p. 301 (§53.34), p. 301 (§53.35), p. 333 (§54.423d), p. 627 (§75.42), p. 661 (§75.532). These paragraphs contain a glaring inconsistency in the treatment of {‘r’m} and {ssnm} in RS 24.244:65, 66 (KTU 1.100). Because these forms are paralleled in the following lines by feminine forms bearing enclitic {-m} ({‘dtm} and {ybltm}), and because {‘r’r} appears in the singular in line 64, it is likely that both {‘r’m} and {ssnm} are also to be analyzed as singulars with enclitic {-m}.632 In the first three paragraphs cited, however, T. explicitly analyzes {ssnm} as a plural;633 in the fourth, he transcribes “ssn-n” and translates this and the three parallel terms in the passage as singulars; in the fifth and sixth, the verb, respectively in emended and unemended forms (see above, remark to p. 58 [§21.352.2 {y} für {ḥ}], etc.), that modifies {ssnm} is parsed as a singular (as is to be expected in this grammar since it is {y-} preformative). None of the four forms in question is cited in the section on enclitic [-m] (pp. 825-32 [§89.2]).

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

---


633In the second, {ssnm} is cited as a type example of “Plural als Grundnumerus.”
in the third ysmt is said not to show agreement with šd ‘field’ in the first text cited. The correct view, recently presented in some detail by Loretz, is that n‘my is a noun, not an adjective (at least in all attested cases in Ugaritic the substantive in question functions as a noun); because the /qataltal/ formation is basically adjectival while ysm could be either nominal or adjectival, both forms should probably be classified as adjectival and both are in these two passages, therefore, substantivized adjectives (on this topic, see remarks to p. 261 [§51.43d], etc., and p. 419 [§69.43]). The phrases n‘my ārš dbr // ysmt šd šhl mmt in the first passage are to be translated literally ‘the goodness of the land of pasturing // the beautiousness of the field on the edge of death’s realm’, while in the second passage n‘my ‘rš // ysmmt ‘rš may mean the goodness of the bed // the beautiousness of the bed’.

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

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

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


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

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

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


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

641 {nḥ kr̄m}, ‘guards of the vineyard’ (the next entry in RS 19.016), or any of the other two-word titles for functionaries, are not included in this paragraph, apparently because T. does not consider them to be true compound nouns. Such should have been the classification of šm‘rgm.
In the 'Aqhatu text (RS 02.[004] [KTU 1.17]), which T. cites as textual reference for the term {•l•b}, that word does not appear as a divine name in the narrow sense of the word, for at each occurrence it bears a pronominal suffix and means 'his/my father’s god'.642 In the ritual texts, on the other hand, {•l•b} does appear as a true divine name, both in deity lists and in the corresponding sacrificial texts,643 and designates the theogonic first principle.644

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

644 Ibid., pp. 296-99, 799.
645 On p. 392, this noun is cited as an example of unexpected usages of the singular with number nouns from ‘three’ to ‘ten’—a surprising entry, since the noun is a paradigmatic collective in Hebrew as well as in Ugaritic.
647 Ibid., p. 1199—the only uncertain case is RS 24.250:12-13 (KTU 1.106), a damaged passage.
648 The term that one would expect on the basis of Hebrew usage, bqr, is not attested in the ritual texts. The more common generic term bhmt for “cattle” in Ugaritic is also absent from the sacrificial rituals (ibid., p. 1122).
649 Ibid., pp. 1112-1114, 1128-29. There is no evidence that ālp is used in prose as a collective like sin. In proof of the classification of ālp as a “Nomen generis” (i.e., in the same paragraph with sin), T. cites one use of ālp in a poetic text (where KTU/CAT read a plural!): 1.4 vi 40 and ‘šr bhmt ālp mri from an
included here, either as a generic noun or as a collective, but ‘šr lhm, ‘ten (loaves of) bread’, in RS 6.216:3 (KTU 4.34)650 shows this word to have been a collective in Ugaritic, as it was in Hebrew. Under the heading of “Kollectiva” (p. 288), T. groups two categories of nouns: (1) collectives in the narrow sense of nouns that do not have a plural,651 and (2) nouns that semantically denote a group but that also have a plural form to designate a plurality of such groups (‘m, ‘people’, šbû, ‘army’).652 In Hebrew, at least, ‘am, ‘people, ethnic group’, can function either as a singular or as a collective when designating a single group (i.e., the singular form can take singular or plural modifiers) and it has a plural that is used to designate a plurality of ethnic groups (e.g., ‘ammēy hāʾāres, ‘the peoples of the earth’ [not ‘the people of the earth’]). Though the data are fewer in Ugaritic, there is no reason to doubt that these two formal categories existed there, and it would have been useful to have kept them distinct. It would also have been useful to provide some explicit argumentations in favor of the identifications of the various nouns cited as generic or collective.

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

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

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


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

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

653For the general interpretation of this text, see comment above to p. 61 (§21.354.1c), etc.; for more detailed interpretations, Pardee, Context III (2002) 93-94; idem, Les textes épistolaires (in preparation).
— p. 280 (§52.222). The reading {mnht} in RIH 78/02:9 (CAT 4.771) cannot be considered new, as T. claims here, when the editors suggested it.654

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

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

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


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

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

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

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

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

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


658 Cf. also the place name Jericho, in Hebrew y'riḥōw, perhaps ← /yariḥā/.


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

661 Already above, p. 739 (§81.11e), p. 799 (§83.231b), p. 853 (§92.232a), he had translated “die ‘Hand’/‘Hände’” without providing the basis of the translation.
— p. 285 (§52.5c). T. takes the variants {tbr} and {ybr} in RS 2,[014]+ iii 33′ (KTU 1.3) and RS 3.322+ ii 46 (KTU 1.19) as proof that the noun ksl, which he translates “Lende,” was variable in gender. Because, however, the personage whose organs are being described is feminine in the first case but masculine in the second, the possibility should be considered that, in the second verse of the sequence in RS 3.322+, the subject of the verb is not the body part but the person and the body part is in an adverbial case (literally, ‘behind, he breaks in the sinew, he rattles in the points of his sinews’).662

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

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

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


664 One might consider an analysis of tizr that T. does not: it would be a G-passive and pnm an accusative of respect, ‘Ṭa ’uṭka is to be veiled as to the [viz., her own] face’. If, however, one admits the existence of 2 m.s. forms addressed to the officiant in these text, such acrobatics are not required.
Because the plural of ṭq, a textile product of uncertain identification, is attested as ṭqt, T. assumes that it is feminine in gender. But ṭsʾm ṭq tn kbd, ‘ninety-two ṭq-textiles’ (RS 18,[539]:1 [KTU 4.595]), shows that the gender is masculine, for the number noun tn/tt, ‘two’, shows gender agreement and tn is the masculine form of that noun. Compare the identical morpho-syntax of ṭsʾm mrḥ āḥd kbd, ‘ninety-one spears’ (RS 15.083:9-10 [KTU 4.169]), with the masculine form of the number adjective ‘one’. ṭq/ṭqt provides, therefore, an additional example of masculine nouns with a plural in -t (on this phenomenon, see T., pp. 297-300 [§53.33] and my remark below to that section).

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

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

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

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

665T., p. 391 (§69.127).
667Pardee, Les textes rituels (2000) 681 (the form may be singular and refer to the royal entrance located on the west side of the city).
‘gate’, in Ugaritic, but abandons that attempt on p. 707, where a contextually less satisfying interpretation is presented without a cross-reference to the other analysis, viz., “Sie (sc. ‘Anatu) verschloß die Tore des Palastes der ‘Anatu …”

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

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

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

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

---

670 Ibid., pp. 31, 149, 177-78, 470.
671 Ibid., pp. 31-32.
672 Ibid., pp. 732, 734.
673 Del Olmo Lete and Sanmartín, Diccionario II (2000) 257.
674 For a full epigraphic and philological discussion, see Pardee, Les textes rituels (2000) 417-18, 425-27 (with references to earlier literature, to which may be added Pardee, JAOS 113 [1993] 615; idem, AuOr 16 [1998] 89 — the latter shows that T. was aware of my epigraphic and philological reservations but chose to ignore them; indeed this article, of which T. was well aware because I sent him a copy before publication and because it appeared before the cut-off date for the present work, is not even listed in the bibliography).
The agreement among the Massoretic vocalization of Biblical Hebrew and the Jewish and Christian Aramaic traditions on the vocalization of /qatl/qitl/qutl/ plurals with aspiration of third-radical /bgdkpt/ letters shows that these forms had dissyllabic stems not only in proto-Hebrew but in proto-Aramaic as well. That being the case, it is necessary to group these West-Semitic languages together with Ugaritic over against Arabic, where a very different system of plural stems is attested.

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

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

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

The masculine gender of qrn cannot be proven for Ugaritic by the texts cited: ādr qrnt in RS 2,[004] vi 22' (KTU 1.17) may mean not “die mächtigsten

---

676Virolleaud, PRU II (1957) 21.
677Hawley and Pardee, forthcoming in Semitica 52.
678Virolleaud, PRU II (1957) 22.
Hörner," but 'I will vow horns',\textsuperscript{679} while the restoration of \{qr\nh\} in line 14 of the same text is simply incorrect (a trace of the sign preceding \{nh\} is visible and it cannot possible correspond to a \{r\}) and any possible link between the noun \textit{qrn} and a masculine verbal form is thus eliminated.

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

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

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

— p. 300 (§53.331.4). If \textit{hpnt}, ‘a type of garment’, is correctly interpreted as feminine in gender, this is more plausible because masculine and feminine forms of the word existed (viz., \textit{hpn} and \textit{hpnt}) than because the word was grammatically masculine in the singular and dual, feminine in the plural. The former phenomenon is well attested in Biblical Hebrew, the latter not—and if the Hebrew sources were more abundant there would probably be even fewer of the latter. In any case, the phrase \{[…]\textit{hpnt} . dqt\} (RS 34.180,10:4 \textit{KTU} 4.765) appears in isolation in a badly broken text\textsuperscript{682} and cannot be cited as proof of the plural in [-t]—it may in fact be the feminine singular.\textsuperscript{683}

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

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

\textsuperscript{680} Ibid., p. 262 n. 178.

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


\textsuperscript{683} Bordreuil and Pardee, ibid., p. 167, translated “HPNT mince.”

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

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

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

Because Hebrew *mēʾīm* designates a part of the body, usually translated along the lines of 'entrails', one may doubt that Ugaritic *mmʾm* means "(viel) Blutgerinsel." The parallelism with *dmm*, 'shed blood', must not be inferred to suggest
strict parallelism of meaning, but similarity, viz., the entrails that run out when a body is torn to pieces.
— p. 303 (§54.111). There is no doubt that $\sqrt{RP}\geq$ normally functions to designate an “Ahnenbezeichnung” in Ugaritic; nevertheless, the precise meaning of $rpi$ in the title of 'Aqhatu’s father that runs mt $rpi$, ‘man of RP’’, must be classified as uncertain.
— p. 303 (§54.111). Asserting without a full-scale argument that the Ugaritic triptotic case system {/-u/-i/-a/} goes back to an earlier system that would have been {/-um/-im/-am/} leaves unexplained why the West-Semitic languages show /-m/-n/ in the plural while Akkadian shows /-m/ in the singular.
— p. 304 (§54.112.2). Because both /rāḥbāna/ and /rāḥbānì/ are attested in syllabic writing for the genitive case of the river name Rāḥbānu (modern Nahr el-Kebir), T. concludes that nouns ending in /N:n-/ were inflected both diptotically and triptotically. Is it not possible, since all such data come from syllabic texts, that the triptotic inflection reflects the language in which the texts were written, the diptotic inflection the local language? If the diptotism of nouns of these types is a West-Semitic phenomenon, one would expect it to influence the use of these words in Akkadian but not necessarily that it should totally replace the proper Akkadian inflection.
— pp. 306-7 (§51.12). As we will see in the following remarks, not a single case of the plural oblique form being used in syntactically nominative slot is convincing. Indeed, this section must be regarded as one in which T. takes one of his more ‘maximalist’ positions (see above, first general remark). Though anything may be possible, the absence of case confusion in the singular and the dual make such a confusion in the plural prima facie unlikely. When one further considers that what is taken as the primary marker of the confusion in the plural, viz., {-/y} in its putative use as a mater lectionis, also functions as an enclitic particle, the inherent implausibility of the grammatical category becomes, in my eyes at least, obvious.
— pp. 306-7 (§51.121.2a), p. 835 (§89.35). On the analysis of the {-/y} of ʻily ūgrt in RS 15.008:4-5 as the enclitic particle (an analysis that T. on p. 306 considers only “theoretisch” possible), rather than as a mater lectionis proving the degradation of the case system, see above, note 88, and remark to p. 265 (§51.44m), etc.
— pp. 306-7 (§51.121.2a). On the analysis of mrḥy in RS 24.247+:7 and 47' (KTU 1.103+) and in RS 24.302:10' (KTU 1.140) as a feminine singular noun, rather than as a masculine plural noun, in the wrong case and with a mater lectionis used to designate the wrong case, see above, remark to p. 52 (§21.341.21b), etc. It truly boggles the mind that so rare a combination of phenomena can seriously be proposed to have been repeated three times in two texts.
— p. 307 (§54.121.2a). Though the interpretation of RS 24.247+:33'-34' (KTU 1.103+) must be deemed uncertain because of its broken state, even if one admit with T. that {ymy} is more plausibly nominative than oblique, the {-/y} remains more plausibly the enclitic particle686 than a mater lectionis for the incorrect oblique case vowel.

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

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

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

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

---


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


690 Pardee, Les textes para-mythologiques (1988) 22, 23, 72. On parsing the form as indicative imperfective, rather than as a jussive as I did here, see remark below to p. 662 (§75.532).
Ugaritic forms, a very rare phenomenon in the singular in West Semitic, when accusative plus the enclitic particle are all one needs, i.e., /-ama/. In any case, such Hebrew forms as yômām, ‘by day’, could have arisen from /yômama/ or from /yômam/, but probably not from /yômamma/, for /a/ in a sharpened syllable at the end of a word in proto-Hebrew most commonly shows up as patah, not as qameš, in the Massoretic tradition. Remarkably, though T. includes dual and plural forms in his list, he does not discuss their vocalization. Apparently this is because mimation is part of the marking of the dual and plural absolute forms, and none of them shows an additional {-m}—hence such adverbial accusatives (i.e., obliques) were not expanded with another enclitic {-m}. I would deduce from this fact that the vocalization of the two was probably similar, if not identical and hence that the adverbial enclitic was /-ma/ (perhaps with a dissimilatory form /-mi/) just like the dual/plural element. It is well known that enclitic {-m} is very rarely attached to masculine dual/plural absolute forms.

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

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

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

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

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

---

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

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


694 Virolleaud, PRU II (1957) 39.


696 Ibid., pp. 863, 869-70.
accusative of direct object, *bky*, if expressed in the accusative, would appear to the listener to be agreeing with that noun rather than saying something about the subject. What T. has done in this section is to present as morpho-syntactically identical (1) adjectival phrases that provide an element of description of the subject of the sentence and (2) nouns with an adverbial function—the reader deserves an explanation for why these two categories have been considered to have been marked identically. Finally, as regards *bky* in this phrase, on pp. 482 and 666 T. presents as an option the analysis as an infinitive, which analysis seems to be proven correct by the phrase *ttu gh bky*, ‘she raises her voice in weeping’, in RS 3.325+ ii 36 (*KTU* 1.16).\(^{697}\) In this passage at least, the infinitive may indeed be in the accusative, functioning as an adverbal noun (‘she gives forth her voice as regards weeping’).

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

— p. 313 (§54.133.2e), p. 844 (§91.314.1). T.’s solution for the different distributions of *adr* in RS 2,[004] vi 20-23 (*KTU* 1.17), viz., that the form would always be a substantival adjective (“die mächtigsten Hörnen” [l. 22], “die größte der Eschen” [l. 20]) but in line 23 not be in the construct with the noun it modifies (instead, the noun would be in the adverbial accusative: *adr b ġl i qnm*, “das größte an Schilfrohren (qnm) im göttlichen(?) Röhricht”), is not altogether convincing. When the solution of taking *adr* as the l c.s. /YQTL/ form of NDR, ‘to vow’ (see above, remark to p. 298 [§53.331.2]), does not involve this morpho-syntactic difficulty, and when it accounts for other features of the list better than does the analysis of *adr* as adjectival (the principal problem is that *adr* shows no variation in form though one might expect *qrnt* in line 22 to be feminine [see above, remark to p. 298, §53.331.2] while *qnm* in line 23 is certainly plural\(^{700}\)), it should be given more serious consideration.

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

---

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


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

\(^{700}\)This fact requires T. here to translate the adjective as singular, while in the two other instances cited he translates as dual/plural.
should be an adverbial accusative (this analysis is proposed on p. 313, where T. translates “an Silber”; on p. 402, he translates “an/in Silber”) rather than an accusative of direct object (on pp. 408 and 879, T. translates without “an” or “in”). T. explicitly parses is‘ as from NS‘ (p. 171 [§33.212b] p. 449 [§73.243.21], p. 598 [§74.624], p. 627 [§75.42]) and as a transitive verb (p. 598), and there is no obvious reason why ksp should not be taken as the direct object of that verb and ḥmšm as an appositional explicative to ksp. The vocalization would be the same, /kaspa ḥamšima/, in either analysis.

In the first section cited, T. manages to use over half a page (twenty-six lines) to argue that {lkš}, which is to be emended to {l kši}, in RS 34.126:20 (KTU 1.161) is a vocative formula (‘O throne’), rather than a prepositional one (‘from the throne’), without mentioning the fact that the editors of the text have always taken the formula as prepositional and, in the official editio princeps, have provided explicit arguments against the analysis as a vocative.701 The most important of these arguments are: (1) the vocative of this very word appears in line 13, without the ʾ, and (2) this passage is indubitably imitating three passages from the Baal Cycle (RS 2.022+ vi 12, 25 [KTU 1.5], RS 2.009+ i 7 [KTU 1.6]), where someone is said to descend ‘after Ba‘lu; in the first of these, the god ‘Ilu ‘descends from the throne’ (YRD + l ksi), the very same verb-preposition combination as in RS 34.126. In §54.222, T. provides another argument against his analysis of the particle when he remarks that RS 34.126:20 provides the only example of the vocative particle occurring at the end of a poetic line-segment that depends syntactically on the following line.

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

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

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

703 Une bibliothèque (1991) 158.
hypothesis that this /li/ is identical with the emphatic particle /la/, rather than with the preposition, in spite of the fact that only Arabic shows a parallel usage, viz., a particle /la/ or /li/ inserted between the vocative particle /yª/ and the noun, which is in the genitive, and the origin of which T. accepts to have been in the preposition. Unfortunately, if the example of RS 34.126:20 be rejected, as it must be (see preceding remark), there is no other example yet attested of this particle /li/ attached to a singular noun ending in /ª/ to provide information regarding the case with which the particle was used. The Arabic use of /la/li/ followed by the genitive may not be ignored, however, in identifying the Ugaritic particle and its syntactic relationship to the following word.

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

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

— p. 321 (§54.315.1), p. 323 (§54.316), p. 366 (§63.123), p. 831 (§89.27a). T. must be congratulated for going against the trend of considering the {-h} of {l ‘nth} in RS 1.005:13

---


705 T. considers pnm a possible case of the terminative case without {-h} on the analogy of Hebrew pānimā¶, ‘on the inside’ (p. 323 [§54.312.2f]) without remarking that the case and numbers markers occur before the -mā¶. In UF 33 (2001) 628, T. avers that his putative case ending is attached “im maskulinen Plural sehr wahrscheinlich an das Nomen mit Endung -ām(a)’ with no attempt to explain how such as thing is possible. It goes against the grain of Semitic morpheme structure for a case ending to follow a case/number marker that consisted of the case/number marker itself and enclitic /-ma/.
(KTU 1.43) as only pronominal; the possibility must be considered that the {-h} is the adverbial morpheme. On the other hand, his assertion that the second sign may be {t} rather than {h} must be rejected.

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

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

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


708 Ibid., pp. 216, 1266.


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

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

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

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

712Ibid., pp. 416-21, with previous bibliography.
713Les textes rituels (2000) 567, 570. My hesitant proposal on p. 372 and in the glossary (p. 1198) to see in {tpnn} an L-stem form is not supported by any data gathered here by T.: all such forms are from hollow or geminate roots.
714The situation is complicated by the fact that the word was originally read as {ḥṣ‘}. The first to have suggested the reading with {ḥ} was, as nearly as I can determine, Ginsberg (Kitve Ugarit [Jerusalem: Bialik Foundation, 1936] 112), but without a supporting note. Since that date, all the translators and commentators whom I have consulted (see bibliography in Les textes rituels [2000] 16) have taken the word as meaning ‘half’.
715Ibid., pp. 19, 64; in English, Ritual and Cult (2002) 68.
716T. remarks that this reading is new in CAT without indicating that it was also present in the transcriptions of Ugaritic texts by Bordreuil and myself that we made available to him for the purpose of including the data therefrom in this grammar.
emendation.717 (4) T. offers no better etymology for the word than the obvious comparison with Hebrew HŠY.
— p. 325 (§54.323c). Perhaps {ḥḏṯ} in RS 24.248:18 (KTU 1.104) does not mean “bei Neumond” but ‘anew, again’.718 For a more detailed remark on this passage, see above on p. 211 (§41.12), etc.
— p. 325 (§54.323c), p. 332 (54.423c). {ḥḏṯm} in RS 19.015:13 (KTU 1.91) probably does not mean “zu Neumond” and is not linked to the next line because in lines 3-20 of this text each line is devoted to a separate festival unless there be an epigraphic indication of the contrary (l. 14). Rather, the {m} marks the plural and refers to a series of new-moon festivals.719 The basic form of the entries in lines 3-20, where the sacrificial festivals are cited by a name or catch-phrase, also speaks against seeing {ḥḏṯm} as the equivalent of {b ḫḏt} (p. 332) in the standard ritual texts: only one festival-name is introduced by b in this text, and there the meaning is local (l. 15); the only names that incorporate a temporal element are two that begin k + ‘RB, ‘when such-and-such a deity enters’ (ll. 10, 11), and they refer to liturgies that are not defined in terms of a day of the month. Because none of the other temporal expressions that consist of b + day of the month occurs in this text, that is probably not the function of ḫḏṯm. Finally, as is clear from a statistical analysis of the ritual texts, the offerings at the new moon, though undoubtedly ideologically important, were not particularly numerous,720 and grouping these festivals for administrative purposes, as here, may be considered a reasonable procedure.
— pp. 326-35 (§54.4). T. devotes these ten pages to a presentation of examples of a locative case for which the internal proofs are limited to two: {šbû špš}, ‘at the rising of the sun’ (RS 1.003:47, 53, paralleled elsewhere by {šbā špš}721) and {špûy}, ‘as a source of devouring’ (RS 2.[009]+ vi 11 [KTU 1.6]). At the beginning of the proofs (p. 326 [§54.412]) are also listed the four cases of the infinitive used ‘absolutely’ from III- ‘ verbs and which show {ū}, hence an ending in a /u/-vowel. There is no obvious reason why the latter should be ‘locative’,722 and even less that the narrative use of the infinitive, of which he lists three examples with /‘/ that show {ū}, should be ‘locative’ (below, p. 335 in an “Anmerkung” to §54.423g, T. suggests that the locative may be the regular case of the narrative infinitive). Moreover, neither of the first two examples is “locative” in the strict sense of the term (the first is temporal, the second the apparent equivalent of a purpose clause [ytn b‘l āḥym špûy, ‘Ba‘lu has given my own brothers that I should devour (them)’]). The question must,

718 Ibid., pp. 567, 570-71.
719 Ibid., pp. 491, 408-9.
722 T. asserts below (p. 329 [§54.422]) that in the comparable Akkadian construction the parāsu(ma) forms are locative, but provides no proof to that effect, not even to the extent of citing an Akkadian grammar. Even admitting the correctness of the identification of the Akkadian forms, however, that analysis may be correct for Ugaritic only if the locative case (still) existed there.
therefore, remain open as to whether Ugaritic had an adverbial case in -/ũ/ or whether the infinitive used ‘absolutely’ was in the nominative case and the first two cases cited represent nothing but this ‘absolute’ use of the infinitive. T. lays out his methodology explicitly on p. 328 (§54.421): because the locative case is difficult to distinguish from the adverbial accusative, the most likely Ugaritic examples will be those that are similar to locative formulae in Akkadian, where the writing system allows it to be isolated. The methodology would be acceptable if the existence of the case itself were more solidly established as a productive grammatical category: the basic problem is that all of T.’s examples except those already discussed in this remark could be adverbial accusatives. T. recognizes the questionable nature of the methodology (“methodologisch fragwürdig”) but prefers setting up a long list of questionable Ugaritic examples over simply saying that the case may exist but that there is no way of determining whether that is in fact the case nor how widespread its use may have been. It must be said that, because of the linguistic separation between Akkadian and Ugaritic and because of the dubiety regarding the very existence of the case in Ugaritic (beyond, perhaps, a few frozen forms), the Ugaritic examples cited have nothing but heuristic value and should not have been cited in a grammar of the Ugaritic language. In his conclusion to this section (p. 335 [§54.424]), T. asserts that, because the locative is not exclusively used with prepositional forms (on the difficulties of such forms that T. claims to exist, see below), the case must be “zumindest bedingt produktiv.” I would say that the two cases of nouns with {ũ} are at the inner limit of being sufficient to prove the existence of the case itself, let alone its productive nature. As an illustration of the problematic nature of the methodology, one may think of what could be done with the dual in Biblical Hebrew if the language were written entirely defectively, had no tradition of vocalization, and were as poorly attested in texts as poorly preserved as is the case of Ugaritic: working as does T., one could easily come to the conclusion that the dual was a productive grammatical number in Hebrew whereas, in spite of provable dual forms being far more numerous than are even debatable examples of the Ugaritic locative case, the dual number is indubitably only a grammatical relic in Hebrew.

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

— p. 326 (§54.412). RS 16.394 is too badly damaged to allow for a certain analysis of {yũ} in {[…]}w₁ yũ . ānũk[[…]} (l. 40′ = KTU 2.31:36) as an infinitive: it could be a participle.


723One may doubt that b p′nh (// b ḥrg’h) in RS 2.[012] i 40′ (KTU 1.12) means simply ‘on foot’, as T. claims (p. 333).
The point of the expression is that the king, who would normally, when on outings, have ridden or been carried, must participate in this procession on foot. Nor does it appear likely to me that the form is in the locative case and that the -m is enclitic (both preferred explanations): the king has to walk on both feet, not on only one, so the form is more plausibly simply the dual, and the case may be the oblique (= accusative), rather than the poorly substantiated locative, supposedly an instrumental locative (p. 333). Finally, we have another case of indecision here, for on p. 771 T. translates “zu Fuß” with no indication of the interpretation preferred above and below.

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

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

— p. 328 ($54.417$), p. 332 ($54.423c$), p. 832 ($89.27b$). T.’s analysis of {b’lm} in RS 24.643:3 and 11 (KTU 1.148) represents one of the more flagrant and regrettable errors of the book. Because these sacrificial sequences correspond directly to the deity lists in which the multiple hypostases of Ba’lu are written {b’lm}, because that sequence corresponds formally to {dim 2} through {dim 7} in the Akkadian versions of this deity list, that sequence of signs in RS 24.643:3-4 and 11-12 can only mean ‘(for) another Ba’lu’, not “am nächsten Tag.” ‘lm alone means ‘on the next day’ (see below, remark to p. 331 [§54.423a]), and it is unthinkable that the otherwise unattested formula b ‘lm would occur multiple times, one after the other, in this text (T.’s analysis requires that the text read ‘in on the next day, in on the next day, in on the next day’, etc., with no intervening liturgical act of any kind). To the extent that T.’s analysis is based on KTU/CAT’s unacceptable reconstruction of line 3 with only two tokens of {b’lm}, by which a discrepancy is created between this text and the corresponding deity list, it must be observed that the epigraphic situation was clarified in detail as early as 1992 and had been described more briefly four

---

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

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

726 Pardee, Syria 69 (1992) 160; cf. Les textes rituels (2000) 792; BASOR 320 (2000) 67. In this case as in so many others, T.’s data are not presented consistently: on p. 332, he says that {b’lm} occurs five times in lines 3 and 4, six times in lines 11 and 12, whereas on p. 832 he says that the sign sequence occurs “jeweils sechsmal” (which is the correct description of the situation). Nowhere does he say that lines 3 and 4 are largely reconstructed.
years earlier. 727 Many things are uncertain in Ugaritic, but choosing to ignore one of the comparatively few interpretations of a passage that are established by specific textual correlations and replacing that well-established understanding of the text with one that has no parallels and makes no sense can only discredit the grammarian. Furthermore, when this is done in the interest of establishing a dubious grammatical category (the locative case) and an even more dubious syntactic category (the use of the locative case with a preceding preposition), one’s faith in the grammarian’s objectivity is shaken.

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

— p. 328 (§54.417), p. 333 (§54.423d). The simple fact that the Ugaritic expression is 了一批 (‘without’) whereas the Akkadian expression cited as support for taking 了一批 as in the locative case has 了一批 should give the grammarian pause. As an example of the formula consisting of preposition + locative case it must be judged worthless.

— p. 330 (§54.423a), p. 739 (§81.11e), p. 799 (§83.231b), p. 853 (§92.232a), p. 885 (§95.11). T.’s treatments of 了一批 in the phrase 了一批 了一批 in RS 4.475:11-12 (KTU 2.10) is more ambivalent than is required by the text itself. In the first section cited he claims that the adverb 了一批, which appears only in this passage, may have had the locative case attached to it, but the very existence of the case is in doubt, even more its unnecessary attachment to a lexical adverb. In the second, he identifies 了一批 as an adverb, but says that it may be a conjunction, provides a cross-reference to the section on the conjunction 了一批, but, when one goes to that section, one finds no reference to this passage. T.’s basic interpretation of lines 11-13 ( 了一批 了一批 了一批 'z mid) is: "Une die ‘Hand/Hände’ der Götter ist/sind hier(?) sehr stark, (so stark) wie Menschen / wie der Tod" (pp. 739, 799); his ambivalence regarding the status of 了一批 is expressed by replacing 了一批 by ellipsis points on pp. 853, 885. Only once, on p. 885, does he attempt to explain why the predicate adjective ‘z does not agree with the noun it is supposed to be modifying, viz., 了一批, a feminine noun. There he proposes two explanations: (1) “Genusinkongruenz”; (2) 了一批 would be “Pl.cs.” and ‘z a/QTLa/ verbal form rather than an adjective. The first would involve a simple scribal error, while the second is out of the question as stated: the plural of 了一批 is in all probability 了一批,728 as in Hebrew, and, however that may be, it is not the plural that is used to express two paired body parts when


728 A word 了一批 of which the meaning is unclear appears in two economic texts (RS 18.024:9 [KTU 4.158] and RS 94.2600:8 [preliminary presentation in Bordreuil and Pardee, Manuel (2004), text 48 in the Choix de textes]), in the first case as the designation of a commodity going for ten shekels per unit (see Pardee, Syria 77 [2000] 27), in the second apparently preceding the commodity in question and hence perhaps meaning ‘portions’. The latter meaning appears to be attested for 了一批 in poetry (RS 2.[022]+ i 21 [KTU 1.5], RS 24.293:11 [KTU 1.133]); T. analyzes this form as a plural (p. 284 [§52.5c]). The problem with this interpretation of the word in RS 94.2600 is that one would expect a standardized measure in an economic text, and there is as yet no evidence that 了一批 would have functions as the designation of a standard measure.

Archiv für Orientforschung 50 (2003/2004) online version
referring to many possessors but the dual. So stated, the proposal is morphologically plausible (/wa yadā 'ilīma pā ka mōtima 'azzā ma'da/ 'the two hands of the gods are here, like death they have proven to be very strong'). However, as a consultation of the comparative literature shows, the word for 'hand' in the idiom 'hand (of a deity)' = 'disease' is normally singular in both Akkadian and Hebrew. All the more remarkable, then, that T. does not even mention a syntactic analysis of the passage that goes back at least to 1934 and which solves the apparent problem of absence of gender agreement by taking mtm, understood as singular + enclitic -m, as the subject of the nominal phrase. This entails taking k as the conjunction rather than the preposition: /wa yadu 'ilīma pā kī mōtuma 'azzu ma'da/ 'Moreover, the hand of the god is here, for death is very strong'.

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

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

730Albright, BASOR 54, p. 26; other references are cited on p. 68 of my article cited in the previous note.
731{ilm} is vocalized and translated as singular + enclitic -m, just like mtm, because the reference in the Akkadian and Hebrew texts is normally to a single deity. Whether that deity is identified as Mot in the following phrase or yd îlm is generic in nature (like qat ili in Akkadian) and mtm is a common noun cannot be determined. For this interpretation of the sentence in context, see Pardee, Context III (2002) 107-8; Bordreuil and Pardee, Manuel (2004) II 81.
which T. translates “am Tag, am folgenden Tag,” is of dubious value because it is never repeated; moreover, the context of this occurrence is broken and there is simply no way of knowing the meaning or the syntactic function of {ym}. Finally, it is a bit ungenerous to say that the meaning “dann, ferner” is “traditionell” when the meaning accepted by T. was first proposed by de Moor in 1970—though there is no doubt that the majority of scholars have preferred the meaning ‘next’.

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

— pp. 331-32 (§54.423c). Though it is unclear why T. decided to put his comment on the semantics of šbu as meaning “Sonnenaufgang” here rather than on p. 261 (§51.43c), where the word is presented as a /qat≠l/ form, his reasons for adopting the meaning of ‘sunrise’, not ‘sunset’, match my own; the essence of the argument goes back to 1983.

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

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

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

---

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


the translation proposed on p. 500 ("ich reinige/banne (dich)"), I do not see how it could have the second, i.e., with an implied 2 m.s. object. Throughout this incantation, its beneficiary and he only is addressed in the second person and he is not, one would think, subject to banishment. ‘I purify (you with) sacred wood’ is, therefore possible in context, but ‘I banish (you with) sacred wood’ is not. T. does not even mention the interpretation that was proposed in the preliminary edition of the text, viz., ‘to move back and forth (transitively), to shake’.\footnote{Pardee, Les textes rituels (2000) 829-31; cf. now, Ritual and Cult (2002) 159; this was the interpretation indicated in the proofs of the edition of the texts from 1986-1992 that were made available to T. (”J’agiterai du bois sacré”) and which has now appeared in Bordreuil and Pardee, RSO XIV (2001) text 52. Ford has recently proposed that lines 1-5 (üzb … ‘qrb) would be the words of the evil sorcerer (UF 34 [2002] 119-52), but that structural analysis is based in no small part on an analysis of üzb that I cannot accept (ù would be an emphatic particle: see remarks to p. 196 [§33.322.3c], etc., and to p. 805 [§84.22]). Furthermore, Ford has no proposal for the meaning of zb, which constitutes a second major difficulty with this re-analysis of the passage. On the possible analysis of zb as a stative verbal adjective, rather than as a /QTLa/ perfective, see below, note 1220.} That interpretation relies on the recognition of a verb MRR, ‘to pass (by)’ (it is highly unlikely that there was a geminate root in Ugaritic that meant “vertrieben” in the G-stem—see below, remarks to p. 600 [§74.626.2], etc., p. 605 [§74.632], etc., and to p. 673 [§75.62a], etc.). The “dich” is, in any case, misplaced because the second-person forms in this text refer to the person in whose favor the incantation is proclaimed, not the banished entity.\footnote{In his study of RS 92.2014 in UF 34 (2002) 119-52, Ford proposes that àmrmr would mean ‘to make bitter’, in context, ‘to produce venom’. I have no particular objection to this analysis of àmrmr, for it would be based on the root MRR ‘to be bitter’ that is well attested in Ugaritic (see remarks below to p. 601 [§74.626.3b], etc., and p. 673 [§75.62a], etc.). This structural analysis is, however, part and parcel of his overall interpretation of the passage of which the principal difficulties are described in note 741.} T. never explains to what root the meaning “reinigen” would be attached and I know of none (the only one that appears to come even close is MR, ‘to bless’, on which see the remarks just cited and, more particularly, to p. 540 [§74.35], etc.).

1. \( “300 rote Madchenkleider im (Wert von) 300 (Schekeln)” \) (p. 334)
2. \( “und die Ausstattung des zweiten/eines weiteren Hauses” \) (p. 366, translating only the first four words), “ein weiteres/zweites Haus” (p. 419, translating only \( bt \) \( tn \)).

The semantic field of npš is very broad, from a ‘garment’ to the collection of items necessary for a function, including garments, vessels and tools; it thus corresponds reasonably closely to English ‘outfit’ which has a similar range of meaning (cf. British English ‘kit’ which can denote a set of clothing, as in ‘evening kit’, whereas in American English it tends to denote a collection of tools). This would be the only text where npš is used to express the ‘outfitting’ of a house. The function of tlł màṭ at the end of line 16 is certainly to number something in this line (i.e., this line is syntactically independent of the preceding and the following lines), but that something could be npš, \( tn \) or \( bt \) \( tn \). In the last two cases npš could be a summary term for the list of following
items and it would be used either absolutely ('list of goods') or in construct ('the goods of the house/daughter'). In the first of these possibilities, bt could denote a type of textile\(^{744}\) ('And the inventory: bt-textile dyed scarlet, 300 units...').\(^{745}\) The problem here is that nothing in the text indicates to whom this list of items belongs. According to the second of these possibilities, there is no indication of to whom the house or the daughter belongs whose goods are being inventoried, unless it be the name that occurs before the horizontal line that separates lines 16-23 from what precedes (bn grgš), but this does not appear plausible. Moreover, the mention of 'wine' in the last two lines renders dubious any hypothesis which sees npš as a summary term for the items named in lines 16-23; it appears preferable, therefore, to see line 16 as one entry among others. Since there is no obvious identification of bt taken as 'daughter' (if the phrase is generic, as T.'s first translation supposes, why would it not be npš ätt, 'women's garments?') and since the meaning of the translation 'the npš of the house' would not be at all clear, it appears more plausible to take npš bt tn as a unit and to translate the line as 'three hundred npš-garments of bt-textile (dyed) scarlet'.

---


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

---

\(^{746}\) Nougayrol, Ugaritica V (1968) text 18.


\(^{748}\) Pardee, Syria 77 (2000) 36-37.
distinct from the number noun /ṭittu/, ‘six’, which would mean “Sechszahl”; it is translated in context “an der Sechszahl der Tage (d.h. am sechsten Tag) der Neumondphase (des Monats) ḫiyaru” (p. 385). He rejects the interpretation of the passage by Pardee and Swerdlow as “during the six days of the (rituals of) the new moon of ḫiyūrā” on the basis of the “parallele Konstruktion” in RS 24.256:10 (KTU 1.112). That text, however, reads [{b} ʾṣḥb’1 ym ḥdt], without the following month name (which was almost certainly indicated in line 1 of this ritual text), and the phrase in context must mean ‘on the seventh day of the new-moon (festival)’. It is the latter formulation that is anomalous in the ritual texts, for the simple phrase {b ʾṣb’}, ‘on the seventh (day)’, is expected. This text, then, plausibly contains a particular construction, which may be vocalized /bi ʾšab’i yami ḫudṭi/ and translated ‘on the seventh of the days of the new-moon festival’. This type of construction is well attested in the mythological texts, e.g., {b ʾṣb’ ymm}, ‘on the seventh of days’, in RS 2.[004] i 15’ (KTU 1.17), which is preceded by a series of ordinal numbers (that they are ordinals is proven graphically by {rb’} in line 8 and {ṭdt} in line 11). {ṭṭ} in RS 12.061:1 cannot, on the other hand, be an ordinal number, nor can it be the /qatʿl/-noun of which he posits the existence elsewhere (see preceding note), and the simplest interpretation is, therefore, simply, ‘during the six days of the new-moon (festival), i.e., the six days extending after the festival celebrated on the day of the new moon’. There is, therefore, no reason to invent a new noun which would have the value of an ordinal number.

— p. 348 (§62.181). How does T. know that the feminine form of the number noun ‘eight’ was /ṭamānīt/-, rather than /ṭamānat/- as in proto-Hebrew (/šaṭmōnāḥ/)? On the general problem, see above, remark to p. 184 (§33.243.13-14), etc.

— p. 349 (§62.192a), p. 364 (§63.112), p. 368 (§63.19). It is difficult to admit {w b tšʾ.} as a “N[eue] L[esung]” (p. 368) of RS 24.248:11 (KTU 1.104) when previous editors have read the {ʿ}754; the break in the tablet runs through the putative {ʿ}, however, and the reading is epigraphically uncertain.755 The claim to a new reading apparently refers to the word-divider, which would confirm T.’s interpretation of b tšʾ as meaning ‘on the ninth

750 For the rare structure b ym + ordinal number, see above remark to p. 261 (§51.43d), etc.
751 Pardee, Les textes rituels (2000) 634, 638-39. T. posits the existence of a noun {ṣb’} distinct from the standard /ṣab’-/ to explain this passage (p. 261, §51.43d), p. 386 (§67.43), but such an explanation is no more plausible here than in the mythological texts (see continuation of this remark).
752 T. explains ʾṣb’ in this sequence as a /qatʿl/-noun (see ibid., as well as p. 365 [§63.113]), accepting, however, that the other number words in the sequence, including rb’ and ṭdt, are ordinal numbers. But the morpho-syntax just proposed works equally well for the ordinal number ‘seventh’ and it avoids the problem of a noun meaning ‘seven’ functioning as an ordinal number—this is a real problem, for in most cases the sequence appears in the literary topoi that consists of detailing the seven days or months over which a situation extends. Only in a passage in the Kirta text, ḫmš … ṭdt yṛḥm (RS 2.[003]+ ii 30-31 [KTU 1.14] is ṭdt taken to be a /qatʿl/-noun (see above, remark to p. 261 [§51.43d], etc.).
753 In addition to the brief note by Pardee and Swerdlow cited above, see now the full commentary by Pardee, Les textes rituels (2000) 416-27.
754 Herdner, Ugaritica VII (1979) 39; KTU/CAT, ad loc.
(day), but, though I agree with this interpretation of $b\ t$ $s\ '$, 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 (§62.20.2.5), p. 353 (§62.25). My collation of the tablet confirms T.’s epigraphic756 and grammatical observations that the restoration of $l$ between the ‘$5$’ element and the ‘$10$’ element in the number noun ‘$13$’ in RIH 83/07+:8 (CAT 4.777)757 was incorrect: the break is indeed too narrow to consider restoring $l$ and the reading {ḥm $[\ldots]$ $s\ rh$} 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 ‘$8r$, ‘$10$’: (1) there is no internal proof that the form is not dual (‘$20$’ should in theory be the dual, not the plural, of ‘$10$’);758 (2) if the form is indeed plural, the stem may be dissyllabic, not monosyllabic (e.g., ‘$i's\ r\ $uma/, rather than ‘$i's\ r\ $uma/). 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 /$i$d-$t/- rather than on /$i$da-$t/- . It is of interest that in the other West-Semitic languages, the number ‘$20$’ shows the unexpected plural ending (e.g., Hebrew /‘e$s\ r\ $im/- whereas the other decades, if a /qa$t\ /qi$tl/ base, show the unexpected singular/dual stem (/$i$s‘$y$m/-, /ti$s‘$y$m/-). T. claims that the /$-\d$ 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 (§62.4.2b). Though the meaning of {ḥm $m\ .\ t\ t\ .\ rkb\ .\ rtn\ $}$ in RS 24.643:20 (KTU 1.148) is indubitably difficult,759 T.’s interpretation of rkb as ‘wagon load’ is implausible, for thirty-eight wagon loads of whatever rtn is (perhaps some sort of textile760) is far out of proportion with the other offerings in this list.761

— p. 356 (§62.43.1a), 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


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

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


761The 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 rqḥ 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 nḥ as “Schweinefett,” but does not give his reasons.762 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.763 The phrase šmn nḥ was previously attested in RS 11.795:3-4 ($KTU$ 4.91),764 there followed by šmn rqḥ, ‘perfumed oil’. In RS 92.2057, the designation of the preceding commodity has disappeared from the tablet while the following is ztm, ‘olives’.

— p. 364 (§63.111). T. cites Akkadian, Arabic, and Ethiopic as a basis for reconstructing the ordinals numbers as formed on a /qāṭil/ 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 /qāṭil/ form, or, for that matter, yet another. There can be no doubt, as T. demonstrates, that the Ugaritic base was disyllabic (this is shown by {ṭd}, ‘sixth’, in contrast with {ṭ}, ‘six’ = /ṭṭu/ ← /ṭṭu/); 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-/),765 for this morpheme had retained the consonantal element {y} in Ugaritic; if one believes that the /qāṭil/ 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. /qāṭil/, 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 /qāṭil/ base and the nisbe morpheme. Another datum not exploited by T. that

762 In 1995, T. identified Ugaritic nḥ with Akkadian nāḫu. “(Schweine-)Schmalz” (ZA 85, p. 64). If nḥ 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 uz, ‘goose-fat’ (RS 16.399:22 [$KTU$ 4.247]) and šmn ʾālpm, ‘fat of bovids’ (RS 94.2405:1—the reason for the plural ʾālpm 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).


764 The reading was Herdner’s in CTA 141; in the editio princeps, Syria 21 (1940) 274, Virolleaud read {m(?)}nḥ, 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.

765 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 {®mn} and which one might have expected to have appeared as {®mny} if the base form were /®amâñîyu/.

— 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]+ 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 in, “eine zweite wurde ihm zu(m Anlaß) einer Totenklage,” is, however, T.’s alone and goes against the reading of all preceding editors of the text, some of whom have worked on the tablet itself, as {fâšr ūmml}. 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 {tn} as an ordinal number or an iterative adverb stands up to examination. The reading {®nth} in RS 1.005:13 (KTU 1.43) must be rejected (see above, remark to p. 321 [§54.315.1], etc.), while {tn} 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 “yrh yrh tn yṣr” in spite of the fact that most of

766 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 /ṭāmin/ or /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, {®mn} in RS 24.250+:18 (KTU 1.106), as /ṭāminî/ was in error: assuming the Northwest- Semitic base as I was, the form should have been /ṭāmûnî/ (←/ṭāmâniyî/ [genitive case]). For another possible case of ®mn functioning as an ordinal number (RS 24.248:7), see above, remark to p. 261 (§51.43d), etc.


768 Dietrich and Loretz, after reading {fâšr ūmml} in both KTU (p. 38) and CAT (p. 36), have more recently (UF 31 [1999] 149) muddied the waters by reading {t rûm} 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 {â} (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 {â} (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., {â}: La légende de Keret roi des Sidoniens publiée d’après une tablette 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 {rîm} (p. 150: Akkadian “ru’ ‘unu D etwa ‘ab, wegschlagen’”), ones 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 {yrh . yr{l-[-(-)]}l-[- . hllh]}, 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 {yṣ̄}.
— p. 365 ($§63.123$), p. 409 ($§69.222$). The ordinal adjective ṭn, ‘second’, is not present in
RS 24.266:18 (KTU 1.119) because the reading may not be admitted.$^{769}$ Moreover, the
word ṭn is, in my estimation, more plausibly interpreted as the cardinal number ‘two’ in RS
1.005:14 (KTU 1.43).$^{770}$ T. objects to this interpretation because the ‘two shekels’ is usually
expressed by the dual of ṭql without the number noun ‘two’. Because ṭn is used with other
nouns and because ṭql 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 Gaṭaru’ would be in this passage.$^{771}$
— p. 366 ($§63.123$). By the standard rules of West-Semitic morpho-syntax, <s>d ṭn nhlıh
(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 ṯinê nhlıḥu/), or (2) ‘a field, the second one of his heir’
(/šadû ṭanû nhlıḥu/). 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 ḡlpm in RS 24.255:14 (KTU 1.111:15) as having that
meaning. There was certainly another sign before {ṭn}, however, and the interpretation of
these three signs cannot, therefore, be considered certain.$^{772}$ T. is certainly correct, on the
other hand, in rejecting the interpretation of ṭn here as the ordinal number ‘second’ and as
expressing the ‘second day’ of a sacrificial sequence because the latter concept is expressed
by ‘lm in line 13.
— p. 366 ($§63.13$). Because the word for ‘donkey’ as the object of a sacrifice is ‘r 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’, ḡmr, 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.$^{773}$

$^{771}$Textes rituels, pp. 240-41, 248-49.
$^{772}$Ibid., pp. 619, 621. T. asserts that “Vor ṭn 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.
— 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.\(^{774}\)

— pp. 368-69 (§63.211). Because above, pp. 364-65, T. has not listed an ordinal number ‘first,’\(^{775}\) 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 l b . ‘ṣrt\} (RS 15.035:1-2 [KTU 4.416]), 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 . ūmšt\} 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).”\(^{776}\) Granting that T.’s interpretation of tn is correct,\(^{777}\) 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 ūmnt 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 ūm, ‘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 ūm 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 ūmnt . iy[l-ūm\} to \{b ūmn tiy[l-nl\m\}, ‘on the eighth (day), figs . . .’, but I was tempted to do so.\(^{778}\)

— 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.

\(^{774}\)For other epigraphic and philological reasons, see Pardee, ibid., p. 198 with n. 219.

\(^{775}\)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 ūm pr‘ 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]).

\(^{776}\)La terminologia dei tessili nei testi di Ugarit (Collezioni di Studi Fenici 20; Roma: Consiglio Nazionale delle Ricerche, 1985) 76.

\(^{777}\)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).

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 text779 and that they designated the repetition of some act associated with the offering expressed by the verb T‘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.780

— p. 373 (§64.23), p. 420 (§69.54b). It is difficult to see how {[b .] sb‘[t . w .] nsp . 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 1995.781 The reading is only “new” with respect to CAT, the authors of which failed to pick up Márquez Rowe’s correct restoration of {sb‘[t]} in place of {sb‘[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 27.782

— 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) ytbn yspr l ħmš as containing a /YQLTØ/-form (here with energetic ending!) followed by a /YQLTLu/-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 {ytb}, viz., there is no trace whatever of a {n} on the tablet, and it is equally possible to restore something along the lines of ytb [•l] yspr l ħmš, ‘Ilu sits down, he counts …’.

— p. 376 (§65.12). T.’s translation of yspr l ħmš 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 ħmš may judged uncertain for that reason; (3) the structure of the text as laid out on this tablet speaks against this passage.

779As 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).

780For 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).

781Márquez Rowe, UF 24, p. 260, n. 7; Tropper, AuOr 13, p. 138.

782For a discussion of these matters, see Pardee, Syria 77 (2000) 43, 55.

783Bordreuil and Pardee, Manuel (2004), text 5 in the Choix de textes.
consisting of a ritual command.\textsuperscript{784} Therefore, the interpretation ‘He (‘Ilu?) sits, he counts to five (months)’ makes more sense in context.\textsuperscript{785}

---

p. 377 (§65.131b), p. 514 (§74.222.3), p. 650 (§75.527a). In all these sections, T. either translates or else explicitly parses \textit{tšt\textit{m}} in RS 24.248:20 (\textit{KTU} 1.104) as G-passive of \textit{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.).\textsuperscript{786}

---

p. 377 (§§65.132-133). Among other arguments\textsuperscript{787} against taking \textit{mlū ksm \textit{tlm}} in RS 1.003:18-19 (\textit{KTU} 1.41)//RS 18.056:20-21 (\textit{KTU} 1.87)\textsuperscript{788} as meaning “die Becher(?) [werden] dreimal(?) [gefüllt]” are the facts that (1) the ‘precative perfect’ is unknown the ritual texts as well as in Ugaritic prose in general\textsuperscript{790} and (2) the corresponding number is \textit{ḥmṣ ‘ṣrh}, ‘fifteen’, in the similar passage in RS 1.001:9-10 (\textit{KTU} 1.39), where the form in question is \textit{mlūn}. The number there is obviously half of ‘thirty’, an indication that \textit{tlm} 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 \textit{mlun}”).

---

pp. 377-78 (§§65.142-143). In his discussion of the etymology of the Ugaritic iterative morpheme -\textit{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 \textit{s\textsuperscript{2}lt’d} that the iterative morpheme was historically a noun meaning “Zeitpunkt, Moment.” The comparison proposed with Hebrew \textit{‘az}, Aramaic \textit{‘edayin}, and the Ugaritic adverb \textit{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 \textit{-\textit{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

\textsuperscript{784}I have discussed these matters in \textit{Context} I (1997) 282 n. 60; \textit{BASOR} 320 (2000) 59.

\textsuperscript{785}Below, p. 383 (§67.11), T. indicates this interpretation; neither section contains a cross-reference to the other.


\textsuperscript{787}Cf. Pardee, ibid., pp. 174-75 (cf. pp. 61-63).

\textsuperscript{788}T. refers explicitly to RS 1.003:19 and to RS 18.056 as reading \textit{ksm \textit{tlm} [mlū]}, but in the latter passage the word \textit{mlū} is extant at the head of the phrase whereas in the former that is the only plausible restoration (ibid., pp. 146, 174-75).

\textsuperscript{789}On p. 401 (§69.152.2) T. observes that \textit{ksm} in this passage “dürfte kaum ‘30 Becher’ meinen” because the plural of \textit{ks}, ‘cup’, is elsewhere attested as \textit{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 \textit{ksm} is the singular of \textit{ksmm} in RS 1.003 as well as in RS 1.001.

\textsuperscript{790}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 “stattivische 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\(^{791}\) of RIH 78/14:12’ (CAT 1.163:5) [hm] tlt id ynhp yrh b yrh âhrm, 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 yrh as meaning ‘month’ may, however, be said to be plausible.\(^{792}\)
— 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 “lik’t” and his equally confident analysis of the resultant form as a 1 c. sing. perfective, ‘I have sent’,\(^{793}\) do not take into account the damaged state of the tablet. Because only the upper left corner of the [\text{\tt 1}] is extant,\(^{794}\) 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.\(^{795}\)
— 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 pàmt \text{\tt n} 1 ‘srm in RS [Varia 20]:20-21 is that it does not reflect a typographical error in CAT, where the \{t\} of pàmt was omitted.\(^{796}\) This word was correctly read in the \textit{editio princeps}.\(^{797}\)
— p. 381 (§66). T. gives two interpretations of the indication of the price in RS 15.062:5 (KTU 4.158) that reads tlt t’ srm 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.\(^{798}\)
— p. 384 (67.22). Information regarding the words tlttm and \text{\tt hmšt}, 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, \{âtt âdrt\} in RS 11.857:09 (KTU 4.102) is translated “eine alte Frau”; in the second, \{tlt âtt âdrt\} in line 16 of the same text becomes “drei vornehme Damen.” One would expect âtt in an administrative


\(^{792}\)The details of the argument may be found ibid., pp. 869-70.

\(^{793}\)Following Dietrich and Loretz, \textit{UF} 5 (1973) 77, n. 32, and their subsequent presentation of the text in their collections of Ugaritic texts.

\(^{794}\)Virolleaud’s hand-copy represented very well what remains on the tablet (\textit{PRU V} [1965], p. 14).


\(^{796}\)See the corrigendum in Dietrich and Loretz, \textit{Word-List} (1996) 225.


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.799 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.800 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 \{pºn.dr(2)m\} constitute a single word. If that is the case, the entry is \tn pºndrm and the final -m is the dual morpheme of a single word.801 This leaves only \tt tnt, ‘two (pieces of) scarlet (tissue),’802 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.803 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).804

— 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”).

---


800See 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.

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

802In explaining the meaning of \tn as “\tn-Gewänder” in §69.124.1, T. refers to Akkadian \sºnatena (Alalah) and \sºnitu. Since Ugaritic is a West-Semitic language, it might not have been out of place to refer to Hebrew \sºnavo as well.

803Below, p. 392, the same comparison is made with the other iterative number adverbs (\lºlid, 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]).

804In 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’ (ptḥ) 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 {m ≤ḥr} (instead of the more common form with ‘šṛh’) 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 [. tḥ]’m} or {w [. šḥ]’m}805 for RS 16.395:8 (KTU 4.243) though he had also earlier claimed that the {b} of the latter reading was partially visible.806 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 ṯrgmn ṭlt 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 {ṭlt} in line 1 as part of the title of the text; (2) every other entry in the text includes the word {ṭlt}, which would not be the case of the first entry in lines 2-3 if ṭlt 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’ mṭ š’ṛt [kbd] iqnû (RS 15.115:19-20 [KTU 4.182]) tells us anything about the regular syntactic relationship between the number noun ‘šṛt 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 ṭlt 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’ (ṭlt ṯḥ’ ym), the second in a related phrase that is attested only once, ‘d šb’ṯ ṣnt (RS 3.322+ 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).807 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

805 As previously proposed in AuOr 13 (1995) 237.
806 AuOr 16 (1998) 293.
807 This explanation was adopted in Les textes para-mythologiques (1988) 116-17 to explain the phrase w n’mṭ ṣnt il, ‘(for) the goodly years of El’ (lit. ‘the good ones of the years of El’), which functions as a superlative.

Archiv für Orientforschung 50 (2003/2004) online version
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 (‘taštīa rābī’a yômā) 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 “išr š ‘my\ mmm ištik,” which is rendered “Wünsche von mir, was immer du willst!” (on p. 425 išr is parsed as a G-stem imperative but the context is not translated). On p. 720 he also identifies {išr} as a ‘new reading’. This treatment requires several comments: (1) it is debatable whether {išr} is properly termed a new reading, since Gordon presented the reading as a restoration in 1968 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, is difficult to accept, for the {i} would have to have been significantly less wide than the {h} just above it in line 14’; (3) virtually no one, including T., has attempted to interpret lines 16’-17’ as sequential to line 15’ with the reading {išr} in line 16’—with good reason, for line 15’, which begins with the deictic particle hnm and cannot, therefore, be the end of a more complete sentence, makes no sense on its own; (4) none of the Akkadian parallels cited for lines 17’-19’ includes a formula similar to išr ‘my before the indefinite pronoun. The only alternative that I have

808 UT, p. 19*, 367 (§19.379)
810 See photo and copy in my Les documents épistolaires (in preparation).
811 The word may be read as {iḥnhn} or as {iḥmn} (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 enûm (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.
812 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.
discovered is to read {¯y˘rß} and to take lines 15’-16’, another related unit. The entire passage may be read and translated {¯h˘nm . ‘bdk . hwt (16’) lyhrș . ‘my (17’) mnm . irštik (18’) d [.] Īṣr . w . ānk (19’) āṣtn .. l . iḥy} “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”.814

— 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 {ārš} must be an imperative and, because the imperative of ‘RŠ is several times written {irš}, the result is some mental gymnastics to account for the {ā}: the preferred interpretation (p. 426) is that the orthography shows anaptyxis (/wa≥rVß/ → /wa’arVß/ — 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 {ā}-signs (these are limited, however, to two cases of {ū} for expected {ā}) 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.815

— p. 425 (§73.121.1a). T. interprets [isp] in the second of the serpent-banishment texts (RS 24.251 [KTU 1.107])816 as showing “Anaptyxe /i/ (bzw. Murmelvokal /@/),” which he represents as “/’ispī/ (Vokalsynkope) ← *’(V)supī.” He bases this complicated development, for which there is no clear parallel, on the fact that the verb ‘SP 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.)

814 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).


816 For a new set of reconstructions of the passage of this text where the forms of the verb ‘SP 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 /r⁴gum/, 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.817 (Does Arabic /≥uqtul/ 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 /'/, 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 {±}. I refer to ±rk in RS 2.002:34 (KTU 1.23), where the combination of form and context indicate the presence of a volitive form.818 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 ‘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 {ḥdl} 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 (“/b⁵nû/ < *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 /uwû/ 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 ibai—

817 For the few possible cases of isolated nouns, see Testen, JNES 44 (1985) 143-46.
/*yabniy/ and the imperative binî.\(^{819}\) (2) T. has trouble keeping his reconstructions straight. [ābn], ‘I build’, is reconstructed as both /*ābni/ ← BNY (p. 451 [§74.243.21b]) and /*ābnû ← BNW, i.e., /*ābnwu (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 (D≥W and L≥W 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: “d’Vyi” vs. “l’uyî”). Here and on p. 663 (§75.533), the latter form is vocalized /lî/i whereas on p. 617 the vocalization is /lû/i. On p. 443 (§73.233.1), T. explicitly indicates “/tābnû/i” as deriving from /tānuw/—whatever the merits of the reconstruction /tānuw/ may be, /tānî/ cannot derive therefrom and would have to be a secondary form by paradigm pressure. Perhaps for that reason, T. indicates only /tānû/ for the same form in the same text on p. 659 (§75.532). On the other hand, just below “/tābnû/i” on p. 443 is indicated “/āhdî/i” but here as derived from either /*āhdiy/ or /*āhduw/ (ditto p. 451 [§73.243.22c]); on p. 660 only /*āhdi/ ← /*āhdiy/ 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 /ya≤luw/, though the verb shows consonantal {y} in the /QTLa/, the participle, and the infinitive. The basis for the choice of /ya≤luw/ 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]: “/lî/i < *l’uyî < *l’uwî”\(^{820}\)). 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.

---

\(^{819}\)See W. G. Lambert, NABU 2001, p. 39, on OB ibnî.

\(^{820}\)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/W₁ “siegen” (p. 617). He analyzes tlū in RS 24.244:68 also as /yaqțulu/ but from the root L/W₂ “schwach sein” (p. 617). Whatever the ultimate solution may be to the appearance in Ugaritic of (a) root(s) containing the elements L and W 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 tlīnn, if the emendation from [tlūn] 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’—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/ < *kiriyyī” hardly makes sense. It can only be understood as some kind of shorthand for /kiriyyī/ ← /kiriyyī/ ← /kriyyī/, 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. 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≤} 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 /-ä/ 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 /a/). 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

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

822 The hesitation continues below: on p. 429 (§73.142), the expanded m.s. imperative {mhy} is vocalized without the option of syncope (“/mʰiyyād/”).
in his vocalization of all forms he indicates /ä/. I pointed out above that in T.’s discussion of {šā} as perhaps showing quiesced /ʻ/, he vocalizes the lengthened imperative option as /šaʻä/ (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 qames 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 {[…]}šā in line 6 represents the m.s. impv. of NŠ, ‘to lift’, for the surrounding text appears to be couched in the 3rd person.

— p. 429 (§73.142), p. 475 (§74.242), p. 517 (§74.223.2). In the first reference cited, {m̄y} 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 /QLØ/. 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 {ā} as the final sign and III-y forms that may be /YQTLa/. He offers as another proof the existence of the I 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

823 Claiming that 3rd and 2nd person forms with final {ā} may be plene writing for quiesced /ʻ/ (p. 456) appears to me to be a rather obvious case of special pleading. For the case of yqrā in RS 2.008+ vīi 47 (KTU 1.4), see remark below to p. 456 (§73.263); for that of tḥā in RIH 78/20:5 (CAT 1.169), see remark to p. 456 (§73.264). Nowhere in this grammar does T. treat {[t]mlāh}, ‘she FILL it’ (RS 3.322+ 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.\textsuperscript{824} The best proof of their independence is that the volitive negative particle 'al 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).\textsuperscript{825} 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.\textsuperscript{826} 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 volitiveness, 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.\textsuperscript{827}

— 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?

\textsuperscript{824}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@tºb/) 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., ‘get 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?

\textsuperscript{825}Perhaps the best possibility is in RS 2.[022]+ iii 11 (KTU 1.5), where one finds ±l ±st , “I surely will not place,’ followed in the next line, after a break, by ±hpkk , ‘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 ±ßt as jussive, rather than as /YQTLa/, is, therefore, perfectly plausible.


\textsuperscript{827}Pardee, JNES 52 (1993) 314-17.
— p. 430 (§73.163). T. devotes well over half a page to the attempt to prove that an imperative verbal form may bear a pronominal suffix of the same person, gender, and number which has an “indirekt-reflexive” function corresponding to the so-called “ethical dative” in the later Northwest-Semitic languages (e.g., /lek-l²kä/ in Gen. 12:1). The parade examples are provided by the three-verb sequence ḫšk ‘šk ‘bšk, ‘hurry, press, hasten,’ attested several times in the Baal Cycle. But, as T. recognizes, these may be verbal nouns functioning as imperatives, with the expression of the grammatical person provided by the pronominal suffix attached to the verbal noun (lit. ‘your hurrying …’).828 To nail down his preferred interpretation, T. cites the form trkk, “Knie du nieder” (RS 2.[012] i 26 [KTU 1.12]), which would be a jussive form with the suffix performing the same function. As T. has already recognized above, however (p. 63 [§21.355.1b]), this form is a prime candidate for explanation by ditography and more examples are needed to prove the existence of the morpho-syntactic usage.

— p. 435 (§73.223.34.6), p. 621 (§75.232), p. 687 (§76.331). On T.’s interpretation of zbl ‘ršm yšù in the context of the aspectual verbal system, see remarks below to pp. 684-701 (§§76.3-4).

— p. 436 (§73.223.34.9), p. 593 (§74.622.3), p. 605 (§74.632), p. 651 (§75.527g), p. 652 (§75.527h). T. argues on p. 436 that because the preceding verb forms bear objective pronominal suffixes while yšq̱ (RS 2.002:10 [KTU 1.23]) does not, the latter must be a passive form (this is also the analysis preferred on pp. 605 and 652; on pp. 593 and 651, the preferred analysis is as 3 m.s. active). The analysis as passive ignores the fact that the explicit expression of pronominal direct objects is optional in all the old Northwest-Semitic languages. The verb may, therefore, be 3 m.s. Š-stem /YQTL/ of QL, meaning ‘he causes (it) to fall’.829

— p. 436 (§73.223.34.11), p. 889 (§95.235). T.’s claim that the bothersome y’rb in RS 2.002:62 (KTU 1.23) should be read t’rb830 is not borne out by examination of the tablet: clearly visible are two heads of vertical wedges where T. would read {t}. It appears necessary to admit either that the verb is 3 m.s. because it precedes its subject and is separated from the compound subject by the poetic structure or else that the 3 m.pl. form could take a y-preformative in this text. Because this would be the only demonstrable case of the latter phenomenon in this text, the former explanation is preferable.831

828 This analysis is indicated pp. 487 (§73.523c), 492 (§73.532), 649 (§75.526), and 677 (§75.66).
829 Pardee, Context I (1997) 277; Bordreuil and Pardee, Manuel (2004), text 5 in the Choix de textes. According to this interpretation, the form may be either /YQTLa/ (yašqili/) or /YQTLØ/ (/yašqil/ [not “yušqil,” as T. vocalizes on p. 650]); there are many /YQTLØ/-perfectives in the myth recounted in lines 30-76. It is also possible that, in this sub-section of the long ritual introduction to the myth, the forms in lines 9-10 are jussives (so, for example, Lewis apud Parker, Ugaritic Narrative Poetry [1997] 208). Finally, T.’s reading of the form as {yšq̱} is indubitably correct (the claim by Dietrich and Loretz, UF 32 [2000] 187, that the tablet in fact bears {yšq̱} may not be accepted—see the photo and copy in the Manuel: though the middle wedge is narrower than the other two, it is indubitably present).
830 A claim he had already made in AuOr 16 (1998) 107.
831 wa yi’rb b phm /wa ya’rabu bi pîhumª/ Into their mouth does enter
‘šr . šnm /’ussûru šâmîma/ bird of heaven

Archiv für Orientforschung 50 (2003/2004) online version
T. accepts without question the reading \( \{y\leq n\} \) that is proposed in KTU/CAT as the first word in RS 2.[014]+ iv 5 (KTU 1.3) in spite of Herdner's refusal to propose a reading for the badly damaged first sign\(^832\) and in spite of the marking of the \( \{y\} \) as uncertain in both KTU and CAT. As Smith reported in 1997,\(^833\) 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 \( \{\text{¯m˘ʌn}\} \) and that the poetic line contains a \textit{figura etymologica}: \( \{\text{¯m˘ʌn} \cdot \text{¯lmm} \cdot y\née n\} \), ‘With a response did the two lads respond …’.\(^834\) The parallel with RS 2.[022]+ i 11-12 (KTU 1.5) that T. sees here is incomplete, for in the latter text one finds only \( \{y\né n\} \), not \( \{y\né n \ldots y\née n\} \), viz., the text reads \( \{w y\né n \cdot gpn \cdot w \text{°gr}\} \), not \( \{y\né n \cdot \text{¯lmm} \cdot y\née n\} \), the reading of RS 2.[014]+ iv 5 indicated in KTU/CAT. In RS 2.[022]+ there is certainly, therefore, a /\text{YQTLØ}/ form and it may be considered an open question whether it is singular (i.e., \( gpn\text{-}w\text{-}\text{°gr} \) are expressed as a single entity, vocalization /\text{ya’ni}/) or dual (i.e., \( gpn\text{-}w\text{-}\text{°gr} \) are expressed as a duality, vocalization /\text{ya’na}/ ↔ /\text{ya’niyā}/); the latter analysis is, however, to be preferred on the basis of the preceding phrase \( tb\é w l ytb \· lilm \) (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 /\text{YQTLu}/ form (vocalization /\text{ya’niyānā}/) appears to be required by the dual form of the common noun \( \text{¯lmm} \) which is the subject of the verb.

T.’s own ambivalence regarding his own rules for monophthongization is evident in these three sections: in all three, \( y\né n \) in the phrase \( y\né n gpn \text{ w } \text{¿gr} \) (RS 2.[022]+ 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 \( \{y\né n\} \) may represent monophthongization of the triphthong (“\( \text{ya’niyā’} \rightarrow \text{ya’na’} \)).

Though \( t\text{°dtn} \) in RS 24.258:23 (KTU 1.114) is certainly 3rd person fem. dual,\(^835\) it is possible that \( t\text{ttb} \) in line 27’

\[
\begin{align*}
w \text{dg } b \text{ym} & . & /\text{wa dagu bi yammi}/ & \text{and fish in the sea.}
\end{align*}
\]

\(^832\) CTA (1963) 17 with n. 6.

\(^833\) Apud Parker, ed., \textit{Ugaritic Narrative Poetry}, p. 168 n. 74.


\(^835\) As is \( t\é dbn \), repeated in lines 12 and 13 of this text where it has as subject the same goddesses as in line 23’ (Pardee, \textit{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.

\textit{Archiv für Orientforschung} 50 (2003/2004) online version
and \textit{trpà} in line 28' are not ('\textit{Anatu} and '\textit{Aṭartu} may be presented as working separately).\footnote{Pardee, ibid., p. 21-23, 67. By the logic of an alternative interpretation proposed there, I could have preferred the interpretation of \textit{trpà} as a dual: ‘\textit{Aṭartu} does [X], ‘\textit{Anatu} brings (something) back, the two of them heal'. What led me to interpret \textit{trpà} as singular was that \textit{ḥsdn} bears the 'indicative' {-n} while \textit{trpà} does not. If the only function of the /\textit{YQTLa}/ form in Ugaritic is volitive, the analysis of \textit{trpà} as 3 f.s. /\textit{YQTLa}/ is more difficult, for the verb is in a marked subordinate clause. I have difficulty accepting that view of /\textit{YQTLa}/, however: see above, remark to p. 429 (§73.143), etc., below, remark to p. 456 (§73.264), and cf. Pardee, \textit{Ritual and Cult} (2001) 160 (interpretation of \textit{ḥsdn} in RIH 78/20:5[\textit{CAT} 1.169]) and 169 (interpretation of \textit{trpà} and \textit{ḥttb} in RS 24.258 [\textit{KTU} 1.114]).}

— 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 "\textit{n[neue]} L[esung]" (see above, ninth preliminary remark) is encountered here: the reading "\textit{y[q]ll*}n \textit{alpm},' two bovids shall fall', i.e., without the word \textit{tn}, 'two', inserted between the verb and the noun (RS 1.009:11 [\textit{KTU} 1.46]), is so described in spite of the fact that in 1996\footnote{\textit{AIO} 42-43 (1995-96) 271.} T. declared himself in agreement with my reading of the verb as \{\textit{y[l]q[ln]}\} and without \textit{tn}.\footnote{BSOAS 58 (1995) 230; in \textit{AuOr} 16 (1998) 88, I comment on T.’s acceptance of the reading \{\textit{y[l]q[ln]}\}.} As I point out in my more detailed bibliographical treatment in \textit{Les textes rituels},\footnote{P. 268, n. 14.} the proposal that the word \textit{tn}, 'two', was not on the tablet goes back at least to 1970,\footnote{De Moor, \textit{UF} 2, p. 324.} but is still present in the text of \textit{CAT}. As regards the reading of the verb, T. again indicates \{\textit{y[q]ll*}n\} on p. 441, but \{\textit{y[q]ln}\} on p. 460, and \{\textit{yql[ln]}\}, the reading in \textit{CAT}, on p. 646. Finally, T.’s alternative parsing of \{\textit{y[l]q[ln]}\} as a G-passive, indicated only on p. 460, instead of the analysis as active, can hardly be given serious consideration, for \textit{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 \textit{Ritual and Cult} modified several details of the restorations of RS 24.251:32-37 (\textit{KTU} 1.107) that were proposed in \textit{Les textes para-mythologiques},\footnote{Pp. 248-51.} such is not the case of the restoration \{\textit{[t•]sp}\} in line 40', a 3 f.du. form that is reconstructed as such to stand in contrast to the masculine form \{\textit{yisp}\} that occurs elsewhere in this passage (the subject of the verb is ‘\textit{nt w tttr}, the goddesses \textit{Anatu} and \textit{Aṭartu}). On p. 439, T. argues forcefully for the restoration \{\textit{[t•]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 \textit{ymgy} in RS 29.093:25 (\textit{KTU} 2.70) and \textit{yšb’l} in line 27 of the same text as 3 m. dual forms\footnote{On p. 660 (§75.532), T. analyzes \textit{ymgy} as a 3 m.s. and only alternatively as a 3 m.du.} founders on the form \textit{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 \{\textit{b’ly}\} to \{\textit{b’in[y]}\} (pp. 439, 589, 790), as has been proposed in \textit{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
text is \{w . k ym\#y \(26\) ‘bdk . l \#lm \(27\) ‘mk . p l . y\#b‘l \(28\) hpn . l b‘ly\}), which cannot be said, as regards the use of the pronoun ‘my’, to make any sense.\(^{843}\) 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 \(hip\#nu\)-garment made for my master … ” (i.e., the male of the two writers is here speaking).\(^{844}\)

\(^{843}\)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).


\(^{845}\)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 \{t\#l\} in line 23 of this text as a jussive or a preterit is criticized.

---

\[^{843}\] 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).


\[^{845}\] 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 \{t\#l\} 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:

\[ w \text{ ndd } g\dot{a}t \text{ l } /w\text{a } \text{nádā d gazara } l\text{ ë } /g\text{a}zari/ \text{ Each stands at one extremity,}^{846} \]
\[ y\text{'db } ?ymn j ñm\dot{a} /y\text{a}’dubu ’ō yamēña ’ō šam’al/ \text{ each consumes on right, on left;} \]
\[ b \text{ phm } w\dot{l}b tšb’n /b\text{i pîhumā } w\text{a lā } tšb’ānā/ \text{ (but of what is) in their mouths never are they satisfied.} \]

— 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]+ v 42 (KTU 1.4) as a passive, usually vocalized as a G-passive (“tul’akānī” [pp. 460, 511, 569, 619]), once as a D-passive (“tula’ akānī” [p. 511]), but also once as a G-active (“til’akānī” [p. 448]). The last is probably to be understood as a simple error, since T. translates even there as a passive and recognizes that L’K 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 tn’n ñdm (RS 3.343+ 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 ñdm, derived from the city name ñdm, is dual and the verbal form is 3 f.du. or else ñdmm expresses [irregularly] a gentilic, ‘the people of ñdm’ [normally such a gentilic would be {ñdmym}]), on p. 460 the tn’n 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]+ iv 47-48 [KTU 1.14] ymēy l ñdm rbt w ñdm ṭrr, ‘he/they arrive at Great ’Udmu, at Well-Watered ’Udmu’), the analysis as a dual appears the more likely.\(^847\) 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 nllhm) 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 /ayā/ 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: /ništū/ (1 c.pl. indicative < /ništayu/—on this vocalization of the indicative ending, see remark below to p. 656 [§75.531e]), /ništā/ (1 c. pl. jussive/perfective < /ništay/), /ništā/ (1 c. pl. ‘cohortative’ < /ništay/), /ništāna/ (1 c. du. indicative < ništayāna), and /ništā/ (1 c.du. jussive/perfective < /ništay/).

\(^{846}\)Lit. ‘(Each) stands at an extremity with respect to (the other) extremity’.

— 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 "t‘r[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 /taqtulâ/-, which would permit short and long forms (/taqtulâ/ vs. /taqtulâna/) 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’ {tq††}), has its parallel in 2 f.pl. forms with [-n] 848. The writing with [i] of one of these forms indicates /YQTLna/ as the probable pronunciation (t‘†•n in L. 22’, 23’, and plausibly restored in L. 19’ = /ti‘hta‘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 {tnšân} (see remark above to pp. 52 [§21.341.21b], etc). As regards the interpretation of the 2d 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 (\{tqtit\} 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 {tqtit}, it is not necessary to see it functioning as a perfective. In this text, similarly to the use of the /YQTLu/ form t‘htá 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

848Unfortunately, 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 {tqtit}. T. suggests either emending to \{tqtit<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'.

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. 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 /YQTLO/ 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.

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),
but that verb is the only word extant in this line and the reconstruction of mṛḥy mlk as its subject is
only a guess—and a poor one at that, given that the preformative consonant is probably
[y-]. 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.

— p. 445 (§73.233.43). Hebrew /tiqtōlnā/ cannot be considered as evidence for the
affirmative element of the f.pl. /YQT/L/ forms being /-nā/ because the Canaanite shift was
operative in proto-Hebrew and proto-Hebrew /taqtulnā/ would have become /tiqtōlnā/. (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.

(§75.237a), p. 733 (§77.412.3d). T. consistently parses tḥṭān in RS 2.[008]+ viii 20 (KTU
1.4) as /YQTLO/ + 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: "tuḥṭa 'ā-anna") thus refusing to come to terms with the fact that the marking of the dual
by /ā/ would disappear (the form should become /tuḥṭanna/ with the Energetic I morpheme—of

850 On the place of the /YQTLa/ form in the Ugaritic verbal system, see above, remark to pp. 429
(§73.143), etc.; on tḥā in particular, see remark below to p. 456 (§73.264).
542.
852 On the form of mṛḥy, 'lance', see above remarks to p. 52 (§21.341.21b), etc., and pp. 306-7 (§51.121.2a).
course, to retain the vowel length, one need only invoke Energic II, which would give /tuḥta ’ā-na/). 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/ + energetic, as one of the independent energetic 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 1st 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 energetic 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 ±l, the negative adverb that appears earlier in this sentence and on which the verb tḥtān must depend,854 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 ±l (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 ’ā-na/), depending loosely on the preceding ±l, where that particle modified directly what was almost certainly a jussive (±l y’tbdkm, /’al ya’budkumāl, ‘that he not set the two of you’) — though it could be /YQTLa/ if one admit the construction consisting of that form negativized by ±l. 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 energetic endings could be attached to /YQTLØ/ forms, for normally ±l 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}855 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 /takribnā/; in the fourth section cited, the imperative is indicated as /t’rub-/ Thus the waterfront is covered.


854 T. correctly negativizes this clause in his translations on pp. 499, 512, and 733, but neglects to do so on p. 446.
856 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 åd is taken to be an infinitive functioning as an imperative);857 or “Von(?) PN sollst du überhaupt kein (Geld) einfordern” (where åd is taken as an emphasizing infinitive); on p. 649, the alternative analysis of åd 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 tåd and åd may be derived from different verbs and constitute a sort of pun.858 tåd, as most scholars have thought, is from ‘(W)D, ‘to burden (someone with something)’,859 but åd from 'DY, attested in Arabic with the meaning ‘to pay’ in the D-stem. The translation would be: ‘Do not burden Nürân; pay for them yourself, (a total of ) sixty (shekels of) silver’, with the appropriate vocalization /nûrãnâ ’al ta’ud ’addi ’atta lêhumu/.860

—— 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 tîl} rather than as the commonly accepted {gr bt îl}, ‘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 îl} (the basis being that this would be the only attested /YQTL/ form of a root II-£ in Ugaritic that would show /i/ as the stem vowel), in the third he cites only {til} as an example of the root W’L. On pp. 448 and 633, he vocalizes the form as /ta’îl/, on p. 617 as /tâ’îl/—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 tîl is in fact present.

—— p. 448 (§73.243.1), p. 618 (§75.227). In the first paragraph cited, lâk 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-£ 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

857 This interpretation goes back to del Olmo Lete and Sanmartín. AuOr 6 (1988) 261.


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

860 T.’s vocalization of the jussive form as /ta’ûd/ (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 /ta’ûd/ 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 /ta’ûd/ ← /ta’uwâ/ while, on p. 643, he gives the last two /ta’ûd/ ← /ta’ûd/. It is likely, however, that the form /ta’ûd/ 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 /ta’ûd/ 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 . lâl.k . ‘ml-[…]’ ‘So, O king, send m[e (a message) …].’

— p. 448 ($§73.243.1$), p. 616 ($§75.222$), p. 810 ($§85.7b$). tšál 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 tšál (24) bt . ‘bdk}, where k appears to be the adverb ‘thus’ rather than the conjunction ‘for/when’ and the subject of the verb appears to be bt ‘bdk, ‘the house(hold) of your two servants’. Since bt is a masculine noun, tšál 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 kâ tîš’alû bêtu ‘abdêka/). 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.

— 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/).

— p. 449 ($§73.243.21$), p. 660 ($§75.532$). On p. 449, T. lists ihd(î) in RS 15.007:7, 10 (KTU 2.15) as possible examples of /yiqatal/ 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 ḫDY, ‘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 ḫDY, ‘to seize’ (cf. Hebrew ye’êhôz), an interpretation that goes back to the editio princeps.

— 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 /yiqatal/ 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,

861 On p. 810, T. suggests hesitatingly that k is the emphatic particle and that Š’L 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.


863 E. E. Knudsen, “The Mari Akkadian Shift ia ← ê and the Treatment of n “Formation in Biblical Hebrew,” JNES 41 (1982) 35-43, esp. pp. 40-41, proposes that Hebrew /yiqtet/ is from /yaqiya/, 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.

864 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, अन्षु, is from the root नश, ‘to kiss’, rather than from नश (← नश), ‘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 /∗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:

- p. 450 (§73.243.22a), p. 590 (§74.622.3). T. is loath to accept that {अश्व} in RS 2.[003]+ iii 45 (KTU 1.14) be the correct reading (he would emend to {तोश्व} and analyze the form as चुमी- श्व- stem). He has preferred, however, to ignore two important factors:
  1. that the root श्व 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 श्व, fits the context remarkably.

- 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 /≥abilir/), “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 /≥atin-/, 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 घ, ‘to hunt’, as “‘अश्वद’", (with the remark that the root may be द or द (which means for him that the /YQTL/ would be “याद”- and the same form of घ, ‘to cry out’, as “‘अश्वद’”- (with no remark as to a possible alternative origin); on p. 645, each is presented with alternative forms, “याद”- and “याद”-.” 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 /yašvd/, Arabic both /yašîd/- and /yašad/-, in the second the imperfective is

---

attested only once in Hebrew, then with a 'strong' formation (yīṣwāḥūw) while Arabic shows /yāṣīḥ-/.

In neither case can the Ugaritic form be /yiqāl-/ because the 1 c.s. form is attested for each and the writing with { å } shows the vowel of the preformative to be /a/ (the Barth-Ginsberg law says that /yāṣād-/ would have become /yiṣād-/ 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 /yāṣūd-/ (because that is the Hebrew form) and ŠH should be /yāṣīḥ-/ (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 /yiqtal/ 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 uncertain866 to lends any plausibility whatever to T.’s view that the signs { å } may be found there which would constitute an anomalous 1 c.s. form of YD‘, ‘to know, which is elsewhere written, as expected, { id‘ }.

— 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 solider.

— p. 456 (§73.263). In my translation of yqrā in RS 2.[008]+ vii 47 (KTU 1.4), I expressed the /YQTLa/ as an iterative.867 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 Mōtu 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 1.1.2) first as “er fand,” acknowledging however by his alternate translation “auf daß er finde” that the form may well express volitivity.868

— 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


867“Mōtu is always proclaiming” (Context I [1997] 263).

/yabkiy/ (i.e., a jussive without monophthongization of the /iy/ diphthong).\textsuperscript{869} 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.\textsuperscript{871} Finally, on p. 659, he proposes yet a fourth analysis, that of a G-passive.

— p. 456 ($\S$73.264), p. 620 ($\S$75.232). One may doubt two aspects of T.'s treatment of /ṭḥā/ in RIH 78/20:5 (CAT 1.169): (1) that the /[a]/ represents quiesced /‘/), i.e., /tithā/, 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.\textsuperscript{872}

— p. 463 ($\S$73.313), p. 610 ($\S$75.211), p. 614 ($\S$75.212.3), p. 716 ($\S$76.534), p. 718 ($\S$76.55), p. 819-20 ($\S$88.1). T. asserts that the particle of existence /ī/ 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, /ī/ itself may have to be parsed as 3 m.s. /QTLa/ ("Tatsächling ist die Abgrenzung von /ī/ als nominaler Part[ikel] und /ī/ 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.\textsuperscript{873} No inkling of this uncertainty is provided by T. In particular, the presentation on p. 614 of these

\textsuperscript{869}Bordreuil and Pardee in Une bibliothèque au sud de la ville (1991) 154; Pardee, Les textes rituels (2000) 819

\textsuperscript{870}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.

\textsuperscript{871}This is the vocalization now proposed in Bordreuil and Pardee, Manuel (2004) II 64.

\textsuperscript{872}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 ($\S$73.233.41-42), etc. In Ford's recent re-interpretation of this text, he accepts the analysis of /ṭḥā/ as /YQTLa/ (UF 34 [2002] 155, 172-74). I am not convinced by his arguments for taking /ḤT/ 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.

\textsuperscript{873}The present reviewer has never accepted the interpretations of the forms written /īt/ 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-verbs, all the rest of which show {à} 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 i® 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 /’i®a/), not from the root /’T/, which would have given /’a®a/ or /’ª®a/; √/TY would also have given /’a®a/ or /’ª®a/.

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 iṭ 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 iṭ modifying a feminine form and not itself showing gender agreement: in the double formula hm iṭ šmṭ hm iṭ ‘zm, ‘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 iṭ 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 iṭ 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 {ṣa-ma-TA} (RS 15.086:16 [PRU III, p. 51]) and {ṣu-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 {qr±} in RS 34.126:4-7, 11-12 (KTU 1.161) as G-passive, following the editors’ analysis, is undoubtedly correct, his reconstruction of the subject of the verb in lines 4 and 5 as {rp[±]} (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 {rpû}. The reconstruction of this noun with {à} is not explained, but it would appear to be linked to T.’s analysis of the plural oblique form rpîm 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, {qr±} 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.

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

— p. 465 (§73.331.3). T. reconstructs the 2d person singular pronominal elements in the /QTLa/ as /-tā/ (m.) and /-ā/ (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,875 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 2d person singular forms, T. cites ʾgīmīt (RS 2.008)iv iv 34ʰ[KTU 1.4] as indicating the absence of a vowel between the verbal stem and the pronominal element (ʾgīmi, ʾyou are thirsty’, must be /gāmiʾtī/, not /gūmiʾātī/). 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 /qatalātū/. One form shows only one {t} for the expected two {smt} for /samat + tu/ in RS 2.014ʰiii 44ʰ[KTU 1.3], the second shows assimilation of non-emphaticness (ʾmḥʾst) for /maḥš + tu/ in the same text, col. iii, lines 38, 41, 43, 45), while the third shows assimilation of the third radical /n/ (ʾytt) 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}: {ytnn}, ʾI gave’, in RS 94.2406:4,876 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.877 Similar writing is attested for the š-stem of this root: [šntn] in RS 94.2479:21 meaning ‘I have had delivered’878 (the same writing is attested in a broken context in RS 94.2580:18). Similar data exist for hollow roots, where the endings /-ātū/-ātā/ 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

875Gelb, Sequential Reconstruction (1969) 64, 66.
876I have cited this passage in JAOS 121 (2001) 136; see now Bordreuil and Pardee, Manuel (2004), text 31 in the Choix de textes.
877The 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.
878Text 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 (ḥqım̱ tā = 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 qʳù 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., rpìm, 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 {rpùm}, rather than {rpìm}. Hence the editors’ decision to parse qʳù as active rather than passive\(^{879}\) (cf. above, remark to p. 307 [§54.121.2b]).

— 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 qʳù 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., rpìm, 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 {rpùm}, rather than {rpìm}. Hence the editors’ decision to parse qʳù as active rather than passive\(^{879}\) (cf. above, remark to p. 307 [§54.121.2b]).

— p. 467 (§73.332.3), p. 560 (§74.414.2), p. 670 (§75.537d). In the first section cited, T. states that ġlttn 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,\(^{881}\) surely this example, where the meaning must be transitive,\(^{882}\) 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)\(^{883}\) 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 štntn in RS 17.063:4 but adds that the form may be 1 c.s. or 2 f.s. On p. 636, he analyzes yṭbn in RS 17.117:5 as 1 c.s. but with the indication “Deutung unsicher.” On p. 664, he considers that ḫtn 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 yṭbn in RS 17.117:4 as “halte ich(?) mich(?)” What I find astounding from a grammarian of T.’s expertise is that

---

\(^{879}\)See references in note 874.


\(^{881}\)Del Olmo Lete and Sanmartín, Diccionario I (1996) 158.


\(^{883}\)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, 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 /-nã/. He does not cite here, any more than above when he was dealing with the accusative/genitive pronominal suffix, Huehnergard’s proposal885 that this latter form may be attested syllabically as /-nã/ (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 MËY, ‘to arrive’, was /qatala/ or /qatila/ in the suffix conjugation; everywhere else he indicates /qatala/ (p. 196 [§33.322.3b], p. 655 [§75.531d—he here states that the 2 m.s. form may be /maśîta/ “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 ‘Šahuwa-Šalimu are born’, but ‘(They have borne) two offspring (namely) Šahuwa-Šalimu’.886

—— 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 on article on the Ugaritic roots NDY, YDY, HDY, NDD and D(W)D887 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 1997888 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,889 but the Ugaritic question is open, the Arabic root cited does little to prove his case (nadda means ‘to flee’, not

886 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 /yaldēya šahrə wa šalima/, i.e., with accusative case vowels on the names, rather than “yaldēya šahrə 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.”
887 UF 20, pp. 339-50.
‘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 IIIrd 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’; nadjda means ‘to urinate’—an anthropological linguist might make something of that!), and he ignores Hebrew m<sup>r</sup>zuzāh, ‘door-post’, exactly the form and meaning that one would expect from an original hollow root DD<sup>r</sup>.890

— 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 ßmm}, ‘the heavens are powerless’891 (RS 2.[014]+ v 18 [KTU 1.3], RS 2.[008]+ viii 22 [KTU 1.4], RS 2.[009]+ 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 ßmm not to have been singular in Ugaritic, and a polyglot vocabulary shows the form to have been plural (/ßamûma/, not /ßamâma/), 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,892 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 ‘â,” which he says is derived from “la ‛āwV” (p. 618) or from “la ’âyV” (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,893 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’ayu/. 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., /ßadû/ßadâ/ßadî/.) In his presentation of qatat/ 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

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

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


893On the basis of {yrū ... tt’} in RS 2.[009]+ vi 30 (KTU 1.6), the anomalous writing of the first word in {yrûn ... tt’ . nn} in RS 2.[022]+ vi 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 {yrū . 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 /âyV/ 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 là is not the infinitive used narratively: (1) là is 3 m.s. /QTLâ/, used absolutely, and šmım is in fact an adverbial accusative (‘there is weakness in the heavens [which are] under the control of ’Ìlu's son Mot’); (2) là is nominal and the entire phrase là šmım 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 ’Ìlu's son Mot’). In the first case là 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 /qatâl/- infinitive or another de-verbal noun.

— p. 469 (§73.333.3), p. 647 (§75.524). The vocalization of {bštım}, ‘tarry, go slowly’ (RS 3.361 iii 18 [KTU 1.1] RS 2[014]+ iv 33 [KTU 1.3]), as “/bâštumâ/” 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 /bªštºmª/.

— 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 /-nû/. 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 /-nû/ or /nû/, 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 ‘particiles’ (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 perfectly, 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., /kāḇêd/, which is either perfective or participial, but /kāḇdā/ perfective over against /kāḇêdīm/ 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.  

894 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, /qatul/, /qatil/, and /qatul/ serve as stative verbal adjectives while the explicit expression of passivity is reserved for /qatul/ and /qatil/ (see above, remark to p. 259 [§51.42c], etc.). The best argument for the existence in Ugaritic of the passive verbal adjective /qatul-/ remains that provided by RS 15.098:11 (KTU 2.17:4), where {lôk} can, in spite of the difficulty of the following line, be nothing but a G-stem passive participle: {w ḫt . lôk m ml[…]} (12) p̄ḡsdb . șmlṣn] most probably means ‘And now, P̄GSDB ŠMLSN is hereby sent with (this) [my] mes[senger party].’ 895 All the forms cited in favor of Ugaritic having a G-stem passive participle /qatil-/ (p. 474 [§73.424]) may, even if correctly read and interpreted, be analyzed as /qatil-/ adjectives. In Hebrew, where the /qatul/ passive participle is productive, many /qatil/ 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 /qatil/ im Ug. als gewöhnlicher MphT [Morphemtyp] des G-Ptz.pass. diente.’ Immediately after this call for reason, however, T. falls into the all-too-familiar trap


895 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 {l•k} in RIH 83/07+:2 et passim (KTU 4.777) in favor of the /qatǐl/ passive participle (or /qatîl/ 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. {blq} and {‘db} cited pp. 474-75, are useless for the purpose of determining whether the G-passive participle in Ugaritic was /qatǔl/ or /qatîl/. In this respect, it appears equally uncertain to assign to Phoenician896 the G-passive participle /qatîl/ 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 /qatǔl-/, for that is the only form indicated in reconstructions of the G-passive participle of weak roots, e.g., III-’y [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-’y 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 uzr in RS 2.[004] i 2’ et passim (KTU 1.17) as a G-passive participle modifying the subject of the verb897 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 {û} 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.898 Since there is explicit evidence for the G-passive participle being /qatüb/ 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}

896See also T., UF 31 (1999) 738.

897Pardee, Context I (1997) 343 n. 2.

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 ḫlq in RS 19.018 iii 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 spy b ḥrṣ 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]+ i 13 (KTU 1.14) as a [m]-prefractive noun or as a [m]-prefractive G-stem passive participle are, respectively, implausible and highly unlikely. As T. shows in this section, the case for [m]-prefractive G-stem passive participles is very weak. As regards the identification as a common noun, in support of which the Akkadian marḥitu is cited, the root TR‡ in Ugaritic had its own development independent of Akkadian rû. 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 bnt (‘builder of offspring’, said of ’Īlu) and qnyt ilm (‘productress of gods’, said of ’Āširatu). 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 /qāṭil-/ 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 /qāṭil-/ (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 ḥdm/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.899 That the primary meaning of PSL in Ugaritic was not ‘to carve stone’ is proven by the occupation title psl qšt, ‘bow-

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.900

— p. 479 (§73.432.1a). T. correctly recognizes here the existence of the personal name snr. I note that, where snr exists, a by-form snn is plausible and T.’s proposal to emend the latter form to [s'n̂rn] in RS 11.858:8 (KTU 4.103)901 is for that reason inherently dubious. This theoretical stance is confirmed by a second attestation of snn 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‘tób, šēbet, b’nōt, and qūm in Hebrew); non-paradigmatic forms (e.g., ’ahšāḇb 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 /qātal/ 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‘tób ’et-hassēper, ‘to write the document’).

— p. 482 (§73.513.5a), p. 487 (§73.523b), 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.902 An equally certain example from this same root is to be found in RIH 78/14 (CAT 1.163), lines 2’ [b ‘ḥl[y]ɪhɪ], 4’ [b ‘lyh], and 6’ ([b ‘]llyh], ‘in its rising (said of the moon)’.903 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]+ i 38 [KTU 1.14]), such is the preferred vocalization of {‘ly} in RIH 78/14:2, 4.904

— 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.


900 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).
901 Tropper, AuOr 16 (1998) 293.
[yd = fem."] But on p. 614, he explicitly parses the form as 3 m.s. /QTLa/. 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 ('/arakî/) 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 'Ilu has just met on the shore of the sea to continue through line 35 after which 'Ilu's alternative is explicitly resolved.905 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: šmî 'hî nîhtû (RS 4.475:7-8 [KTU 2.10]). He fails to mention that, because of this morpho-syntactic anomaly, it has been proposed that ʰî is in fact a common noun meaning 'blow', in the plural and in construct with the following verbal phrase.906 The sentence may be translated somewhat literally: ‘I have heard of the blows with which they have been smitten’,907 less literally, ‘… that they have suffered defeat.’908 (On the verb nîhtû, 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., ‘ahabªh 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 /qatªl/ form has lost infinitival value, to be replaced by /qutul/ in the strong root; in Aramaic all the derived-stem infinitives show /ä/ 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 /qatªl/, 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 /qatªl-/, /qitl-, or yet another. For example, in the case of {hg} in RS 2.[003]+ ii 91 (KTU 1.14 ii 38), the form is indicated as /higî/ ← “/higw/yi/” (p. 486, §73.523bα), though Arabic /ha∆w-/, Tigre /higyä/.

907Cf. ibid., p. 66.
and Medieval Hebrew /hãgãyãh/ 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 {ṣr} 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/).

— 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 /qVtVl/ (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≤ n‘mn ĝlm il, lit. 'in the tear-shedding of the goodly lad of ≥Ilu' (RS 2.[003]+ ii 8-9 [*KTU* 1.14]). Because one of these is indubitably /qatāl/ (b šal /bi ša‘ālil/, 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-≥ 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., /qVtVl/ in Hebrew or /mīqṭal/ 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 qām 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‘L, /ša‘ālu/ existed alongside /ša‘lu/šī lu/šu lu/ and that the former was used in the b + infinitive syntagmeme, 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 qatālī/. (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‘ākuma ‘il’aku, ‘I will surely send’, RS 16.379:19-20 [*KTU* 2.30]) and in the preposition + infinitive syntagmeme just discussed, but another verbal noun when the structure is accusatival, e.g. hlk ±”th b≤l y≤n, ‘Ba‘lu saw the going of his sister’ (RS 2.[014]+ iv 39 [*KTU* 2.31]).


910One may doubt on this basis that the sequence [[… . t‘rb . b ši} 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 ($§73.523a$).

911That this usage was not universal is shown by the formula tr b lkt in RS 3.362+ ii 28’, 29’ (*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/ULK (see remark below to p. 625 [$§75.332$]).
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 /qatâlu/. 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 (§§73.523ba-ß), pp. 666-67 (§§75.536a-b). Because T. himself recognizes the existence of /qatâl/ infinitives alongside /qitl/ verbal nouns, one is at a loss to understand why he classes the verbal nouns from III-y roots in §73.523bß as /qatl/qitl/qutl/ forms when these are written with [-y], in contrast with a group gathered in §73.523ba 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 /qatâl/ and other types—see further remark below to this section.) Because most of the examples in §73.523bß consist of the syntagmeme consisting of preposition + infinitive, for which the example of {b ß±l} proves that the infinitive may be of the /qatâl/ 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 /qatâl/ form. More important than speculating whether the forms cited in §73.523bß are or are not /qatl/qitl/qutl/ (a classification that appears to border on the purely arbitrary) would be showing why those cited in §73.523ba may not be historical /qatâl/ 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 {nß•} in the phrase b nß• ≤nh, lit. “in his/her lifting of his/her eyes’, is “wohl /niß≥i/.” He goes on to claim that the Ugaritic form “entspricht dem he. Inf.cs. n@≈o≥ .” 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@≈º≥/ a suffixal form such as /no≈≥≠/). 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., /naスーチ≥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]+ 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 /qatâl/ 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.912 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

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 trhsnt 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 tlūăn (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,913 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 iq nú, 40 shekels’ worth of ktn).914

— 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


914Bordreuil, ibid., took iq nú as referring to lapis-lazuli, ktn to the stone designated in Akkadian as katinnu. Since ktn commonly designates a garment in economic texts and iq nú 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.\textsuperscript{915} 

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

1. If one may judge from Arabic, the two Ugaritic /YQTL/ forms termed ‘energic’ 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 energic morpheme, not that morpheme followed by a pronominal suffix) written with a single {-n} as Energic I (/-anna/) or Energic II (his Energic 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 Energic 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 energic 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., /tQTL-nya/ 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,\textsuperscript{916} 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 {tšûn}, for example, a 3 m. pl. form, does indeed contain the Energic I morpheme (the one with geminated /n/), then it can only be vocalized /tišša’unna/

\textsuperscript{915}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 Bordesreib 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.

\textsuperscript{916}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.
← /*tiśsaʿūnna/, which makes it formally identical with singular indicative forms plus
Energetic 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 energetic element ended in a vowel (T., p. 501).

(6) T.’s posited third energetic form (his Energetic 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 (/yiqt@lenn¨/ [← YQTLannahu] and
/yiqt@lenh¨/ [← /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 energetic morpheme per se, but to a
combination of energetic and suffixal morphemes.917 T. recognizes the necessity of dealing
with this combination of morphemes, but prefers to posit the existence of a third energetic over
envisaging possibilities of recombination of well-attested West-Semitic morphemes.917 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 energetic morpheme and a pronominal suffix (i.e., /-n + hV/ → /-nnV/, itself reanalyzed
as a pronominal suffix and attached to the Energetic I morpheme, i.e., /-nn + 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 energetic form is that it is unattested
independently, viz., only {-n} is attested as the energetic ending without associated
pronominal suffix in Ugaritic. In order to argue against 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 energetic morpheme unattested
elsewhere as such in Northwest Semitic.918

(7) Two verbal forms are clearly attested with three tokens of {n} affixed to the verb
RS 1.026+:11 {ṭšknnnn}, ‘(someone) shall establish it/him/her’, and RS 15.174:17 {ṭtnn ḫ1
nn}, ‘(someone) shall give it/him/her’. One interpretation of these forms is as enerics + a
suffix {-nn},919 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
eneric would have been attached to another.

918This stance should not be taken as denying any historical relationship between the Akkadian ventive and
the West-Semitic enerics, 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 energetic morphemes
in Hebrew and Arabic.
(8) Most fundamentally, T. takes the energic 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.\footnote{On T.'s view of /YQTLa/ forms, see above, remark to p. 429 (§73.143), etc.} This is a pure reconstruction in the sense that there are no explicit Ugaritic data to support it (of /YQTL/ forms from III-\(^{-}\) roots that do not themselves end in a vowel, no form spelled \{yHTun\}, i.e., /yiqta\-'un(na)/, is attested to date—granted, there are very few forms attested\footnote{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: /iq\(\acute{r}\)an\}/ /iq\(\acute{r}\)'an(n)a/. To this form is perhaps to be added \{y\(\acute{s}\)an\} in RIH 77/2a:3 as a 3\(^{\text{rd}}\) person /YQTLa/ form, though the /\(\acute{\text{a}}\)/ here may represent the dual morpheme (see remark below to p. 500 [§73.611.2h], etc.).}; nor are there any spelled \{y\(\acute{\text{q}}\)Tin\}, i.e., /yiqta\-'(a)/\footnote{On p. 500 (§73.611.2h), T. cites KTU 2.54:2 for the form \{y\(\acute{s}\)in\}, but the reading is uncertain and the interpretation even more so (see remark to p. 500 [§73.611.2h], etc.).}. Moreover, it goes against the Arabic evidence, where /YQTLanna/ and /YQTLan/ function as distinct moods (cf. p. 730 [§77.411]\footnote{Here T. describes the situation in Arabic as permitting the energic 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.\footnote{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).}}). On the other hand, the data from fourteenth-century Canaanite appear clearly to favor T.'s hypothesis (cf. pp. 501, 730).\footnote{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.} 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 afformative [-n(n)], i.e., as perfectives, imperfectives, and all the volitive forms. Because there is yet no certain example of a spelling \{yHTun\}, 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].\footnote{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.} 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 (/yiq\(\acute{\text{t}}\)\(\text{̄}\)lenn\(\text{̄}\) = ‘he will kill him’, /wayyiqt\(\acute{\text{t}}\)\(\text{̄}\)lenn\(\text{̄}\) = ‘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 (/\(\text{n(V)}\)/ + /\(\text{nV}\)/); 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...}
to /YQTL/ forms ending in a long vowel to express the indicative over against the jussive/perfective (i.e., /YQTLũna/ vs. /YQTLũ/). 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 (/tQTLũ+nna/ → /tQTLũnna/, 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 (/tQTLũna/ = /tQTLũ + na/).926 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,927 /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.928 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 Energic 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 hlmm 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.

---

926T. rarely alludes to this possible source of homophony (e.g., p. 663 [§75.532] on tštyn).
927The problem in Hebrew is neither with forms of the type /yiqt@lenn¨/ (← /YQTL + an + hu/), nor with the rare forms of the type /yiqt@lenh¨/ (← /YQTL + anna + hu/), but with those of the type /YQTLẽhũ/, which should, all other things being equal, derive from a proto-Hebrew base /YQTL + i + hu/ (cf. Aramaic /yiqtšinnẽ/).
928T. 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 energikuslosen Verbalformen mit Objektsuffixen gebraucht werden” (p. 730 [§77.411]).
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 /yiqtℓêhu/ and /wayyiqtℓêhu/, 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/929, 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/.

In summary, the simplest solution to the problem of the enerics 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 from were distinguished from indicative plural forms (jussive /YQTLû/, indicative /YQTLûna/), 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 imperfectives 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 {tluûn} in (RS 02.[003]+ i 33 [KTU 1.14]) that the pronominal suffix form, at least, could be attached to the /YQTLu/ form (see

929Cf. {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 /yêbêl/, Akkadian /ibil/.

— pp. 499-500 (§73.611.2). T. suggests that the final vowel of the Energetic I /-(a)nni/ may be dissimilated to /i/ when following the dual ending, i.e., he vocalizes /mïyn/ “/tamîyânni/.” But he does not do so when the form ends with what he reconstructs as /â/, e.g., /yphn/ is vocalized “/yiphâ-nnal/” Is that logical? He cannot, of course, consider that the same vowel might dissimilate following /a/, as I would reconstruct forms of this type,931 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 tm†rn in RS 2.[009]* iii, lines 6 and 12 (KTU 1.6), as /YQTLO/ “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 trîpn, translated “flatterten,” in RS 3.340 iv 20’, 31’ (KTU 1.18) and RS 3.322+ i 32 (KTU 1.19) as /YQTLO/ + 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+ i 15, ii 37 (KTU 1.16), as GYL and he vocalizes the form /nagîluna/ or /nagîlanna/; on p. 644, he identifies the root of the same forms as GWL and vocalizes the stem as /nagûl-/.

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

931See above, remark to p. 190 (§33.311.5), etc.
Hebrew GL, ‘rejoice’, there the stem vowel is /i/. In the apparent Arabic cognate, GWL, the stem vowel is /u/, 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 /yagi1-/.

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+ 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/.932 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.933 (On the D-stem vs. the L-stem of hollow and geminate roots, see below, remark to pp. 575-76 [$\S$74.50]).

— p. 500 ($\S$73.611.2d), p. 644 ($\S$75.522). The arbitrary nature of T.’s classification of /YQTLn/ forms is well illustrated by his analyses of tmtn in RS 3.325+ i 22, ii 43 (KTU 1.16): on p. 500, the form is given as 3 m.pl. jussive + Energic I and vocalized “tamūtū-na” (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 (“tamūtūina”). On p. 644, only the latter is indicated.

— p. 500 ($\S$73.611.2f), p. 506 ($\S$73.634c), p. 625 ($\S$75.331a), p. 731 ($\S$77.412.1b), p. 733 ($\S$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 energetic morpheme, and on p. 733 he translates without an objective suffix (“Schlage f ü r wahr … ”). In denying the presence of an object suffix on pp. 500 and 731, he points out that the form in RS 3.322+ 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 energetic 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 energetic morpheme which may historically contain this same

932 On this form, see also p. 55 ($\S$21.342.1b), p. 442 ($\S$73.223.5), and p. 641 ($\S$75.521).

933 This is not to deny the possibility of a /yaqvVl/- form existing in Ugaritic—cf. Hebrew /yiśwāhûw/ (Isa. 42:11) the verb that corresponds to Ugaritic sbh/ysh—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 [yšân[…]] 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 I l l, ‘and at night’, and the mention of the deity Rašap 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/)934 or else the dual (it is possible that another divine named preceded [w ršp . 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 [yšîn]; in the second, he considers dividing [yšî 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. /yašî’/ could not have “(a)nnâ” (T.’s representation of his Energic I) attached to it, for /yašî’anna/ would be written [yšàn] and /yašî’na/ is an impossible form. /yašî’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 /yašî’/ 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 . yšî[…]} ‘/al yašî’/ ‘may he not go forth’. It is unclear why he does not consider any of the /QTLa/ possibilities, viz., that the sign following [yšî] may have been [t] rather than [n] (the editors of the text correctly represented the presence of only the left edge of a horizontal wedge935) and that the form may be /QTLa/ in one of the second persons or 1 c.s., viz., /yaša’t-/, which would, of course, be written [yšit(-); or the sign may indeed have been [n] and the form 1 c. pl. (/yaša’nû/ (which would, of course, have been written [yšîn])). 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˘nn˘} 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

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)T 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 “zerreißten, 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)T, ‘to be ashamed’, to have appeared with an accusatival 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’936 (though, formally, one cannot rule out that the forms in lines 28’ and 29’, which do not bear a suffix, were from B(W)T while yb®nn in line 31’ was from BTT937).

— p. 504 (§73.632). Two of T.’s claims regarding the Energic II (his Energic 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’,938 not to mention the standard plural form /yiqt@l¨h¨/, 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 {t[[®]®bn} 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 /YQTL¨na/ type, for which the expected form would be {t®®bnh} or {t®®bnn}) may be considered unnecessary. If the form is 2 m.s.,939 there is no problem as it stands. The proposals for emendation may be considered particularly pernicious: that the form should be

936This is a classic interpretation of the passage: see Caquot and Sznycer, Textes outharitiques I (1974) 139.
937For a literary argument against this interpretation of the forms, see Pardee, Context 1 (1997) 249 n. 63.
938*Kuntillet Ajrud ybrk wyšmrk, ‘May he bless you and keep you well” (Z. Meshel, *Kuntillet ‘Ajrud: A Religious Centre from the Time of the Judean 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 althebraischen 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).
read {t[t]bn[1]n[1]} because the single [n] present consists of four wedges\(^{940}\) is belied by the hundreds of signs with extra wedges in Ugaritic;\(^{941}\) the emendation to {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 {[yʃ]˘r˘kn} in RIH 77/18:7’ (CAT 1.175) as Energetic II (i.e., his Energetic III, that is, /-an/). It appears more plausible, in this prose text where there are no other certain energetic afformatives, to see in the {-n} the energetic 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 “yhrk-n” in spite of the fact that the fragility of the reading was known to him (below, p. 605 [§74.632], he transliterates “ytʃh˘r˘kn”). The caution shown on p. 605 is merited, for the {h} is no longer visible and the {r} is badly damaged.\(^{942}\) 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 (|[yʃ:mm]|) nor what the exact form was of the verb ending in {[--]lr˘kn}. On p. 506, where the ambiguity of the {-n} of {[yʃ]lr˘kn} is stressed, T. interprets {yʃ:mm} 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 {yʃ:mm} is treated, however (p. 605 [§74.632], p. 673 [§75.61e], p. 679 [§75.676]), the form is everywhere parsed as S-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 ‘-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

---

\(^{940}\)I did not copy the sign as consisting of four wedges.

\(^{941}\)See my article “RIH 77/27, RIH 77/12, RIH 78/26 et le principe de l’écriture cunéiforme alphabétique” forthcoming in Syria 79.

\(^{942}\)The editors transcribed {[yʃ]lr˘kn}: 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 $l yiḥd šṭqšlm b ūnšt$ in RS 15.125:1-2 ($KTU$ 2.19) has disappeared and because the passive idiom $Ήट b$, ‘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, 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 $yṣq$ 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.943

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 $ṭsqn$. On p. 512, T. analyzes all the appearances of $yṣq$ 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 $√DK(K)$ and $√YSQ$ as G-passives is unlikely for contextual reasons, it is all the more unlikely for $√MSS$ in these same texts, for one would not expect a G-stem form of a geminate root to be written $\{ymṣ\}$ or $\{ymš\}$ 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 $√MSS$, 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.945


944 Ibid., p. 51.

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 {ïyisl1} in RS 24.247+:14 (CAT 1.103+) 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}.946 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,947 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]+ 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 accouting that Ba‘lu is to be the recipient of good news (√BŠR), 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’.948

— 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 (§75.357a). In the first section cited, td in RS 2.[008]+ 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/

947Ibid., p. 552.
(pp. 639 and 717) once /YQTLØ/ (p. 516). The latter appears preferable, for the utterance is indicative, not volitive.\(^{949}\)

— 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);\(^{950}\) 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 (ūšḫry, 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 ḫrmnm, because it was old, (new) clothing was issued to them in the royal palace’ (mlbš ḫrmnm k ytn w b bt mlk mlbš ytn lh lm).

— 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.\(^{951}\) T. remarks that “\(/kly \) Gp

\(^{949}\)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).

\(^{950}\)On p. 786, the passage is only translated and the precise parsing behind the translation of ytn by “alt (geworden) ist’ cannot be determined.

ist offenbar semantisch deckungsgleich mit √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.952 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 (“PKKi”) or as /YQTLu/ (“PKŁ”); on p. 668, the form in both texts is parsed as “PKK,” 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/953); 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 . [r[…]} is to be translated “Wine which is to be consumed under the supervision of […].”954 A similar translation of RS 24.248:3 is plausible,955 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.956

— 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.957

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

952 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]).
953 In Les textes rituels (2000) 491, I vocalized “yiklā”; for the reasons behind the revision to /yiklû/yikkalû/, see below, remark to p. 656 (§75.531e).
957 Ibid., p. 348.
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 {[•]pt”}, “I indeed shall open”.959

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/tštñ}.960

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’.961

— 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 ← WDY₁ 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+), Biblical Hebrew, in particular Ezek. 17:21, is of at least equal importance as a point of comparison with Akkadian sapāhu (T. cites only the latter).962

— p. 514 (§74.222.3), p. 888 (§95.234). T. does not give his reasons for considering that, according to his interpretation of { yšpk₁ . kmm . ārš l kšp . dbbm} (RS 92.2014:12-14 [RSO XIV 52]963), viz., “Es soll(en) wie Wasser(?) zur Erde hin ausgegossen werden die Beschwörer und Zauberer,” yšpk would not show number concord with the plural subject. It is apparently in part because elsewhere in the text, the negative particle is l, not


960T., p. 458 (§73.272).

961See 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 tštñ, apparently a contracted “long” plural form (T. p. 663) of the verb ŠTY, appears three times in this text.


963Preliminary edition in Les textes rituels (2000) 829-33; see also now Bordreuil and Pardee, Manuel (2004), text 18 in the Choix de textes.
ál, 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 {t°dn} (l. 8).964 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 (/<yašpukû>/), 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 t°dn 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 (/lá/), rather than by the expected ál (/’al/). So rare a usage must have been extremely emphatic and hence appropriate for an incantation.965 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.966

— 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 lâk 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.967 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 /qˁil/, 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

964The same is immediately visible in my preliminary edition in Les textes rituels (2000) 830: “ti’danû” and “yašpukû.”

965T. cannot object too strongly to this analysis, since he has a section specifically devoted to the category “l vor volitivischer PKK" (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 lô + jussive in the Hebrew Bible.

966See Bordreuil and Pardee, Manuel (2004) II 69. In UF 33 (2001) 696, T. maintains his interpretation of the form 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.

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.\(^{968}\) T.’s citation of the form as proof that the Ugaritic G-passive /QTLA/ was of the /qutila/ 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 /qutila/ 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 /suqtala/. (T. prefers /quttala/ but /suqtala/ 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 /qutila/ stative pattern—after all, Arabic in general shows more adaptations to various secondary systematizations than do Hebrew or Aramaic. On the other hand, if š in RS 2.[003]+ 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+ 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 qb•tm in RS 34.126:3 (KTU 1.161) is in error, for the tablet is well preserved at that point and the reading certain.\(^{969}\)

— p. 517 ($74.223.2$), p. 533 ($74.32$). Surely the interpretation of ngb in RS 2.[003]+ ii 32, 33 (KTU 1.14), as a G-stem passive participle is just as plausible, if not more so, than T.’s

\(^{968}\)Bauer and Leander, Grammatik des Biblisch-Aramäischen (Tübingen: Niemeyer, 1927) 104, hold that the Aramaic development was /qutal/ → /qutila/ (under the influence of the /qatila/ stative pattern) → /qutila/ (under the influence of the G-stem passive participle, which would have been /qatila/). T. holds (p. 518 [$74.223.2$]) that the /qutila/ 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 /qutila/ → /qtila/ form is surely old and appears to have been the basis for the finite form—whether it be by mutation or by analogy to /qutila/—which was spelled /qtîl/ in old texts where the mater lectionis in all probability represents a phonomically long vowel.

\(^{969}\)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 yaṣi’ // 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 “precative 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]+ I 31’, 32’ (KTU 1.4) should be analyzed solely as from the G-passive /QTLa/. The verse reads kt i l nbt b ksp // šmrgt b dm ḫrš, “… a (throne-)stand for ’Ilu with silver decorations // interspersed with (decorations of) ruddy gold.” 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 ‘precative 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 Ṭarriyelli, who has just been mentioned) or as an imperative addressed to the recipient of the letter would better fit the grammar of Ugaritic prose, 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, Ṭarriyelli the queen or the queen-mother. The phrase yšāl ṭry Ś ṭrgmt l mlk šmy /yiš’ al ṭarriyelli pa ragamat lē malki šumaya/, ‘may he ask of Ṭarriyelli 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.

---

972 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 √HLK; the word “pluralische” appears again p. 521, with respect to √HSB) 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 plurality would have been in order. The expression of durability does not appear to have been at the forefront in the West-Semitic aspectual system, 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 /yiqtaṭil-/ type, as opposed to Arabic /yafta‘il/, 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 /šiqtal/ rather than /šaqtal/—that is, if it was not /šaqtila/ (see below, remark to pp. 596-99 [§74.624]).

— p. 519 (§74.232.1), p. 527 (§74.232.22). The contraction of {‘myḏtmr} to {‘mtmr} 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

973 Curiously, the verb ḤHS, used three times in parallel with ḤSB with both in the Gt stem (RS 2.[014]+ ii 5-6, 23-24, 29-30 [KTU 1.3]), is described only as “iterative bzw. durative (intransitive) Nuance.”

974 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.


976 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’ (/‘ammīyiḍtamir/ → /‘ammiḍtamir/ [the vowel that remains after the contraction is short because in a closed syllable] → /‘ammiṭtamir/ [/d/ → /t/ by assimilation to the non-voicedness of /t/] → /‘ammiṭtamru/ [with case vowel appropriate for a nominalized verbal form and syncope of the stem vowel])

977 T. reconstructs the original form as /‘ammVyiḍtamVru/. The same must be said of the place name ≥Ilißtam≤u, 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 {'myḍtmr'}):

978 surely the most plausible reconstruction is /‘ilīyištami’, ‘my god has heard’ or ‘let my god hear’; here T. reconstructs the nominal element as a nominative in the absolute state (/‘iluyīštami’/ or /‘iluyīštamV‘i/).

979 Unfortunately, the form with {i} is attested only once, as {tšištîl} 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.

980 — 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 {yṣṭîl} and {yṣṭâl} 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. Unfortunately, the form with {i} is attested only once, as {tšištîl} 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.

979 UF 17 (1986) 402.

980 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.

— p. 519 (§74.231.1). Regarding syncope of the stem vowel discussed in the preceding remark, T. says only “Bei Antritt (lang-)vokalischer Endungen ist mit einer Synkopierung … zu rechnen (vgl. etwa den PN 1a-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 tqtnsn (RS 1.012:58 [KTU 1.23]), which T. vocalizes “/hiqtanVš-/,” the vowel between the first {n} and the {s} seems not to have elided, for, if it had, the /n/ might be expected to have assimilated to the /s/. The form ystrn (RS 2.[008]+ vii 48 [KTU 1.4]), if indeed from vSRR 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 /yäsob/ ← /yasubbu/); by analogy, the Gt form may have been /yistarr-/.

980 — p. 520 (§74.232.21). T. reconstructs the Gt of √HLK as /yîtalVk-/ 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-ʔ; (2) the tD in Hebrew is formed on the strong root with the /h/ unassimilated to the preceding /t/ (/yiṭṭalēk/). With these forms in mind and considering that the G-/YQTL/ in
Ugaritic is {yfk}, not {yhfk}, surely the more plausible reconstruction of {ytfk} is to treat it either as based on a biconsonantal stem like the simple G-stem, viz., as simply /yitalk-/ (cf. {flk} /aiik-/ ‘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 /yiytalk-. 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 tpp, ‘she makes herself X’, in RS 2.[014]+ 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 /qatalal-/ with infixed-/t/ of YPY, ‘to be beautiful’, /tiytapaypi/, ‘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 ytß• 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). 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 ytß• 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 2d person pronoun attached to the preposition in the phrase ‘dn yßtal ‘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 …”.'
In July of 2003, I was able to recollate the tablet to assess T.’s suggestion to read as {ü} what I previously took as {û} 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 {[y̱ẖr][û]}, viz. that the following verbal form, yatn, 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 {‘} of the first syllable of the Gt-imperative ({iqttl}) might not have been realized in speech is not to be retained, for it is based on the analogy of the Arabic alif al-wasla 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., yiisp, ‘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

strengthened intransitivity when the G-stem was already intransitive (e.g., n‘r, ‘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.\(^985\) I originally expressed these categories in terms of “voice”\(^986\) 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.\(^987\) 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;\(^988\) 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.

\(^985\)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]).

\(^986\)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.


— 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+: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 {±tn} 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 “nātana” 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 {ḥzr} 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 n‘rb 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 nṣkh 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. 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 nimšā’ 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. 534 (§74.32), p. 527 (§75.42), p. 669 (§75.537d), p. 685 (§92.234b). On what I consider to be a false analysis of ndbh, n‘y, 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 ‘alif-signs representing the vowel that precedes the consonant and, for the particular example of iṣpī, by other orthographies when a vowel follows the {’} (i.e., {ispī} and {ispā}), it is highly

---

unlikely that the second {i} of ispi tells us anything about the quality of the stem vowel of the N-/YQTL/.
— p. 536 (§74.333). If bnš in the phrase bnš tprn RS 9.453:28 (KTU 4.44) means “Personal” as T. translates, then the verb yâḥd 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 bnšn, 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 ynsiltyy (RS 19.013:22-23 [KTU 1.90:21-22]) as ablative and t’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’-sacrifice”).
— 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 -âna/ instead of -âna/ or -âni/ (T. would indicate -âni/—see above, remark to p. 210 [§§41.112.8-9], etc.): ymsân, yngn, 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 tntkn ūdm’t (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: ysnphy in RIH 78/14:12’ (CAT 1.163) is so classified on the basis of the German “sich zeigen, erscheinen” (Hebrew nirâh is compared). Because, however, the form does not mean literally ‘the moon sees itself’ 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 nirâh 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 tplg in line 69 of RS

991 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\textsuperscript{992} is a /YQTLu/ form (p. 660, with a question mark)?\textsuperscript{993} 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, Horînu, or the magical devices that the latter has put to use, that actually effect(s) the division of the poison into harmless rivulets.\textsuperscript{994}

— 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 (/yipparsîh/), 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\textsuperscript{995} 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., /yipparsîhu ... yaqîlu/ and /yipparsîh ... yaqîl/. As to the semantic value of the N-stem (this parsing, based essentially on Akkadian napalsuḫu,\textsuperscript{996} is probable though uncertain—on p. 681, the alternative parsing as G is presented, viz., /yuparsîh/) 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',\textsuperscript{997} 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-Ưgårû, 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 (šā, 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 'allīf'-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


\textsuperscript{993}On this latter form, see below, remarks to p. 656 (§75.531e) and to pp. 700-1 (§76.427a).


\textsuperscript{995}Pardee, Context I (1997) 249.


\textsuperscript{997}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. “naqtāf’)? 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 qames 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),998 is stated in the first section cited here to be “wahrscheinlicher” read as [nmrt] and that assessment is said to be “nach Kollation.”999 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 ±lk nmrt “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 nfkp in RS 4.475:14 (KTU 2.10) to be the noun in the following sequence hm nfkp m‘nk, “falls dein Gegenangriff (w.: deine Antwort) zurückgeworfen/abgewehrt wird”) without even considering the possibility—I would consider it a probability—that hm nfkp 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

999In 1996, before collation, T. had proposed to read {tmrn . álknl . mfrtm} (AfO 42-43, p. 270), after collation tmrn álkn nmrt (here, p. 540, with no indication of word-dividers).
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).

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 LH˚M, ‘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 LH˚M1001). 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’1002 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 Dānî ‘ilu would ‘eat’ and ‘drink’, rather than ‘giving eat and drink to the gods’, appears to me to be out of the question.1003 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


1003Idem, 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 hiqdiš, ‘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 {aqtl}, hence /′aqattil-/, 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., ywp®n in RS 2.[008]+ 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 ātwt, ‘she arrived’ /′atawat/ (RS 2.[008]+ 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 /≥aqattal-/ as secondarily derived from /*≥uqattal-/ because of the consonant /≥/ should have been expressed in terms of a general phonetic rule. That would, however, have been difficult for if anything /≥/ is more common with /u/ in Ugaritic than in the other Northwest-Semitic languages (compare Ugaritic °¬b≤ /≥u¬ba≤u/ with Hebrew ≥e¬bª≤, Ugaritic °m /≥ummu/ with Hebrew ≥´m, Ugaritic °dm≤t /≥udma≤ªtu/, with Hebrew d@mª≤ºt, 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 {twln} in RS 15.008:12 (KTU 2.16) to be certain.1004 As T. observes (p. 549), the new reading {tdln} proposed in CAT is epigraphically unacceptable,1005 but he considers {trhl} 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 {twln}, 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,1006 there is no particular reason to doubt that the /YQTL/ would have had the basic form /yiwwatil-/1007 Taking the form as D-stem, T. is constrained to see it as irregularly intransitive. So radical a solution appears unnecessary.

1004Pardee, 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}.

1005He diplomatically terms {twln} “wahrscheinlicher als” {tdln}, though the sign in question cannot possibly have ever been {d}.

1006In the appropriate section (pp. 536-39 [§74.333]), T. cites no clear examples. He considers {ynp′} in RS 3.322+ ii 16 (KTU 1.19) to be an N-stem from YP′ (p. 537), but that analysis may not be considered certain: it is in parallel with {yp′} 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 RII 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′ (T. is a bit harsh in referring to this option as making appeal to a “Phantomwurzel vnp′,” 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.

1007The D-stem shows /w/ (cf. p. 195 [§33.322.1b]), and the N-stem may, therefore, have done the same.
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 /≥/ 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-/ ← /ya’bVd-/). As noted above, I am loath to accept such reconstructions.

In his discussion of the semantics of y±sp 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 subject in this passage where the subject is plausibly plural. T. cites the passage as
“x[x]hm yâsp hmt,” which he translates “lhr(e) …(?!) möge das Gift (völlig) tilgen,” with no
reference to my reading {¯•˘[l]hm . yâsp [.] hmt} and interpretation as ‘may (all) the
divinities gather the venom’. 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 {¯•˘[l]hm} is correct, that form is either singular + enclitic-m or dual.

T. parses the finite form in the phrase ±l t±pq ±pq (RIH 78/20:12 [CAT 1.169])
as D-stem because t±pq, with {±}, appears to him to be a D-/YQTL/ form. ±pq 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.

It is remarkable that T. does not even mention this interpretation when
he himself proposes various grammatical analyses but never a translation.

As noted above in the remark to {yrgbb‘l} (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:+4 (KTU 1.106) should not,
therefore, be parsed as “PKKi” as it is here.

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.
I have subsequently collated the tablet and concur with their judgment that the comparison of

---


the form, though with a different contextual interpretation, has recently been accepted by Ford (*UF* 34


1011 *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 ānšq in RS 3.367 iv 4’ (KTU 1.2) is from NSQ, ‘to burn’,1012 than from NSQ, ‘to kiss’, for there are other 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 NSQ, ‘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 ąqrbk ābh b‘l, ‘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 ābh 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 √NSQ 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 ṭl yyll, ‘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 tmḥṣ and tsmt 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 Kǒṭaru-wa-Ḫaṣšu, 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/)?

1012Caquot and Sznycer, Textes ougaritiques I (1974) 135 note i.
Archiv für Orientforschung 50 (2003/2004) online version

— p. 555 (§74.412.27). T. follows Dietrich and Loretz\(^{1013}\) in taking \(\text{y} \text{š} \text{l} \text{m}\) in the phrase \(\text{ml} \text{k} \text{n} \text{y} \text{š} \text{l} \text{m} \text{ l} \text{i} \text{bh}\) (RS 24.247:54’ [\(\text{K} \text{T} \text{U} \ 1.103^+\)]) “the king will make peace with his enemy,”\(^{1014}\) 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.’\(^{1015}\)

— p. 555 (§74.412.27), p. 812 (§85.8b). T.’s primary interpretation of RS 24.255:22-23 (\(\text{K} \text{T} \text{U} \ 1.111:23-24\)) involves gender incongruence (in \(\text{m} \text{t} \text{h} \text{ l} \text{ t} \text{š} \text{l} \text{m}`In, the noun \(\text{m} \text{t}, \text{‘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 be read with the first three of line 23 to give the word \(\text{bh} \text{m} \text{t} \text{h}: \{\text{bh}(22)\text{m} \text{t} \text{h} \ . \text{t} \text{š} \text{l} \text{m} \ . \ (23) \text{I} \text{\i} \text{l} \text{n} \ .\}, \)”(from) her (22) own cattle, she is not required to repay anything on this account.”\(^{1016}\)

— 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 \(\text{abd}\) and \(\text{y} \text{d} \text{y}\) in RS 24.244:5 et passim (\(\text{K} \text{T} \text{U} \ 1.100\)) as /\(\text{Q} \text{T} \text{L} \text{a}\)/ optatives or futures (!) rather than as the imperatives that most scholars have seen here.\(^{1017}\) The case for the /\(\text{Q} \text{T} \text{L} \text{a}\)/ 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 /\(\text{Q} \text{T} \text{L} \text{a}\)/ form was /\(\text{q} \text{a} \text{t} \text{t} \text{i} \text{l}\)/, positing on the sole basis of {\(\text{s} \text{a} \text{–} \text{l} \text{i} – \text{m} \text{a}\)}, 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).\(^{1018}\) 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 /\(\text{q} \text{i} \text{t} \text{t} \text{a} \text{l}\)/ or /\(\text{q} \text{i} \text{t} \text{i} \text{t} \text{a} \text{l}\)/ because I-guttural forms show only /\(\text{i} / in the

\(^{1013}\)Mantik (1990) 151.

\(^{1014}\)Pardee, Ritual and Cult (2002) 140.


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

\(^{1018}\)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 /\(\text{q} \text{i} \text{t} \text{t} \text{a} \text{l}\)/, Huehnergard assumes /\(\text{q} \text{a} \text{t} \text{t} \text{i} \text{l}\)/ 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 lead both Huehnergard and Tropper to an incorrect reconstruction of the proto-Hebrew process.
first syllable (cf. /mê‘én/, ‘he refused’ [“compensatory lengthening”], and /bi‘ér/, ‘he burned’ [“virtual doubling”]) while the vowel of the second syllable is variable (cf., for one and the same root, both /bêrak/ and /bêrêk/, ‘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.  

1019 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: {•hb} in RS 94.2168:11 may be taken as 3 m.s. D-stem /QTLa/ /, ‘he loved (intensely)’, to be vocalized ‘/ihhaba/. This form appears to confirm that {•hbt} in RS 16.394:53’ (KTU 2.31:49), not treated by T. in this grammar, is indeed a verbal form.  

1020 — 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.’  

1021 — 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’, 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.  

1022 — p. 559 (§74.414.2). T. takes khdnn in RS 29.093:13 (KTU 2.70) as a /QTLa/ form 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’.  

1019This 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 τ “ Verbs in the Derived Patterns,” ZAH 14 [2001] 34-42).  

1020The new text rules out the explanation of {•hbt} 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.  


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

1023AAAS 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)’. As a verb of movement for which the marked causative is attested in other Semitic languages, 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 ūlm (KTU 4.342) as his one and only example of a “quittel”-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. T.’s analysis of the verb as passive and his analysis of the phrase as a whole lead him to take the following word, ‘ṣr, for him a singular absolute, as a collective; the editors took ‘ṣr as in the construct state and hence unsusceptible to analysis for number.

— pp. 571-72 (§74.432). Of six roots providing possible tD-stem /YQTL/ forms, only one contains a ‘/ 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 {ytṣṭl}; 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 /hiṣṭammér/ ← */hiṭšammar/). 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 Ṣ-L, 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.

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]+ 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.

T. objects to tmz‘ in RS 3.322+ 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 Puגדatu, has gone nowhere during the events narrated in the intervening lines.\textsuperscript{1028}

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\textsuperscript{1029} 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. ߺb‘b, ‘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,\textsuperscript{1030} that is less true of geminate roots, where D-stem factitives are reasonably well attested (e.g., ḥillēl, ‘to praise’, ḥillēl, ‘to profane’). Moreover, /qīlēl/ and /qōlēl/ forms are occasionally attested for the same root, with different meanings.\textsuperscript{1031} All this being the case, T.’s classification of all Ugaritic C₁C₂C₂ 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

\textsuperscript{1028}Pardee, Context I (1997) 351-52.

\textsuperscript{1029}GKC §55b.

\textsuperscript{1030}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.).

\textsuperscript{1031}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.1032

— 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,1033 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‘izz 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’.1034 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 /taqâninu/, rather than /taqninu/ or /tiqnanu/ as I first proposed. To explain the Ugaritic form, I appealed to Arabic √QNN which, in the VIIIth 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.1035 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)1036 or else show that the root is in fact hollow, as T.

1032 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 y†ll in RS 3.322+ i 41 (KTU 1.19).


1034 The formula is attested twice, once in RS 1.018:6 (KTU 2.4) (‘t‘izzli[k]’, again in the practice letter RS 16.265:4 (KTU 5.9).

1035 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.

1036 That such a proposal is not implausible is proven by the existence of the feminine verbal noun ḫnt from the root ḪNN (RS 2.[004] i 16’ [KTU 1.17]); qtt 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*).  

— p. 578 (§74.511a). Though it may be judged the more likely, the analysis of {tḥbb} 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 {tḥb b} and the resultant verb form analyzed as G-stem.

— 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, *ykllnh* in RS 2.[008]+ 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 /qaṭāl-/ forms of the root YMN, a denominative verb from /yamīn/, ‘right hand’. If this etymology be correct, the writing of the participial form without {y} shows that the base form was /maqtaṭāl-/: /maymanīn-/ → /mēmanīn-/.  

— 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’ rather than from a root PNN that T.

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

1037 On p. 677 (§75.66), T. holds quite correctly that *qn* cannot be a G-stem verbal noun of the type /qaṭāl-/, but his conclusion that *qn* does belong to a geminate root cannot be accepted as necessary until it be proven that only the /qaṭāl-/ 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.


1039 Pa’el or Pi’el in Biblical Hebrew (GKC §55d).

1040 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).

refuses to identify etymologically. 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.

— p. 579 (§74.511b), p. 678 (§75.673). As with [y′zz] in RS 24.247+ (KTU 1.103+) (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. In this case, Hebrew provides an analogy for the L-stem ofŠDD 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.

— 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]mmk in RS 92.2005:28 (RSO XIV 49) is in all likelihood D-stem, as the editors propose, not L-stem as T. would have it.

— 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+ iii 25 [KTU 1.15]), r ymll, ‘he kneads mud’ (RS 3.325+ v 28 [KTU 1.16]), and tgd, ‘she swells’ (RS 2.[014]+ ii 25 [KTU 1.3]). All four of these are, therefore, plausibly L-stem intensives rather than G-stem forms on the /YvQVLvL-/ pattern; this analysis appears by far


1043 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).


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


1047 T.’s suggestion on p. 585 (§74.53—cf. p. 788 [§83.115b]) to analyze the form as tL (“mögen sich dir gegenüber g[e]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 \textit{yhr} (if the restoration of a second \{r\} be correct) and \textit{tgd}, 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 \textit{tmm} in RS 2.0[22]+ iii 13, 27 (\textit{KTU} 1.5) other than as the L-stem imperative of a geminate root \textit{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 \textit{tmm} as an expanded form of the adverb \textit{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.\textsuperscript{1048} 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, \textit{mdllkm} in RS 1.002:21' \textit{et passim} (\textit{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,\textsuperscript{1049} 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). \textit{mllm}, ‘those who purify’, in RS 24.266:23' (\textit{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,\textsuperscript{1050} 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 \textit{Hll} also appears in RS 24.260:6 (\textit{KTU} 1.115) in the following sequence of signs: \{\textit{wshhl} \textit{l} \textit{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 \textit{w š hll}, ‘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 \textit{Hll}, and it thus appears more plausible to divide the signs to read \textit{w š hll ydm}, ‘and purify the hands’.\textsuperscript{1051} 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.).


\textsuperscript{1050}Pardee, \textit{Les textes rituels} (2000) 667, 678 (with previous bibliography in note 96).

\textsuperscript{1051}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 trr (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 tdmn(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 ḫrd ydl (the last word is plausibly emended to ydl) as “wird der König samt seiner Ḫrd-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 ydl 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 Ḫridu-troops’). As for tdln 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, has little to recommend it. Attached to line 7, as I believe more likely, the form tdln is best taken as D and as factitive: “the weapon of the king will lay it (the land) low” (mrʾ mlk tdln).

— 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 ḫr thr l ḫt (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 thr as L-stem active (‘the bird roasts on the fire’) or to take the

1052 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 {yddll} may be a token of the tl stem: /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.


1054 Idem, AfO 33 (1986) 130-31, 145 (the transcription “tdln” 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.

direct address to the deity ‘Ilu as continuing here (‘you roast a bird on the fire’), in which case the verb would be factitive, hence D-stem. To the extent that T.’s interpretation of the form hr in RS 2.022+ 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 hr does not explicitly express a factitive, it appears plausible to take the word as an L-stem verbal noun:

- nhr b’l b kbdh  Ba‘lu will enter his liver (i.e., his insides)
- b ph yrd  will go down his mouth,
- k hrr zt  like a ripe olive (lit. like the roasting [= becoming ripe] of an olive),
- ybl arš  (like) the produce of the earth,
- w pr ‘sm  even (like) the fruit of the trees’.

— p. 585 (§74.53). T.’s “[n]ou(e) [l]esung” [y[‘]n] (RS 2.008+ iii 10', in place of [y[t]b] in CAT 1.4) is hardly qualifiable as new since it goes back to the editio princeps.

— 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, in Aramaic many lexical Shaphels were retained after the productive causative prefix shifted from /ʃ/ to /h/’, in Hebrew only one such form was retained in a full paradigm (the Št of HWY /hišṭañwåb/, ‘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 IXth stem (/’istafa’ala/) is in fact the old Št-stem.


— 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 {āšqtl}, hence /’ašaqtil-/, see above, introduction, general remark on vocalization, and the general remark to the D-stem (pp. 544-46 [§§74.412.1-16]).

---

1056 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 šhrtt, the parallel term in this passage, being factitive (as I took it in Context, viz., I translated both šhr and šhrtt as ‘(you) roast’).

1057 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 {‘}).

1058 T. follows CAT in indicating {āšisp} in RS 16.402:12 (KTU 2.33) as a certain reading, though such is in fact not the case.

Archiv für Orientforschung 50 (2003/2004) online version

— 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 āšhlk šbk dmm that occurs in various states of preservation in RS 2.[014]+ 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 (šbk) 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 “/ašahliku/” on p. 589, “/ašahlik/” on p. 625.

— p. 589 (§74.622.3), p. 864 (§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 accusatival 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.1060

— p. 591 (§74.622.3), p. 691 (§76.343a). On p. 591, yšlhmnh, ‘he causes him to eat’, in RS 2.[014]+ 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 dabringen”) 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 ‘ešlāh in Biblical Hebrew, to designate the effecting of a holocaust-type offering and (b) in at least two of the

1060 Caquot, Ugaritica VII (1978) 125, 391.
three cases cited here above, the object of the verb is an inanimate object. 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 dght, 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.

— 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., “yuṭṭìb.” On p. 596, the corresponding imperative form is vocalized “ṭṭìb.” 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., /yaṭṭìb/ (cf. Hebrew /yāśēb/ and /wayyaśēb/ ← /yahašēb/ vs. /yāśīb/ and /wayyaśīb/ ← /yahašīb/) and /ṭṭìb/ (cf. Hebrew /hāśēb/ ← /hašēb/ vs. /hāšīb/ ← /hašīb/).

— p. 594 (§74.622.3), p. 651 (§75.527g). In the first section cited, yṭṭb 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 yṭṭb has largely disappeared, and there is hence no way of being certain that the root in question is not YṬB. 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. In fact, the sign in question is badly damaged and the restoration of rgm is uncertain though possible. Finally, it should have been noted that the idiom yṭṭb + rgm, i.e., with a 3rd 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 tṭṭb l ‘bdh,

---

1063 Dietrich and Loretz, Die Elfenbeininschriften (1976) 51; this representation is maintained in KTU/CAT.
1064 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 (ūmy ‘my tğıb 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 qds yśr, ‘and the qds- official shall sing’), it is unlikely that the subject of the verb in w rgm gırîn 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 passive1065 or as an active, with the gırîn as the subject (an alternative mentioned by T.).1066

— 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 ślm w yttb ly ślmk, ‘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 Ś-stem forms of vT(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 śtn 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 śtn [ly?] b spr, ‘and send (lit. have given) [to me?] (word of that) in a letter’.1067

— p. 595 (§74.623). T. recognizes that from the signs {tłmy} in RS 17.117:6 (KTU 5.11) must be extracted the word tlłmy, ‘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 t} 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 TŴY, 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.1068

1066The 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).
1068On this text, see Pardee, ibid., p. 45, 47-48.
p. 595 (§74.623). On p. 595, T. glosses šškm in the phrase šškm m‘ mgn rbt āṭrt ym (RS 2,[008]+ 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, 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 ‘Āṭiratu of the Sea in the form of a gift’ (āṭrt ym would be the direct object of the verb, mgn an adverbial accusative).1070

— p. 595 (§74.623). Here T. presents as the only parsing of šqrβ in RS 1.002:26′ (KTU 1.40) that of 2 m.pl. impv. (/šaqrıβ/) . 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.1071 More recently, in his attempt to disprove the existence of second-person forms in the ritual texts, T. has proposed that šqrβ here might be an infinitive, 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 {‘ṣr . ‘ṣr . ‘fb .’ -- ‘ , 1 bt 1l1m1 1 kbkm} (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”).1073

1069 CAD S, pp. 69-70.


1073 Ibid., p. 684.
I do not find the argument that nominal syntax is common in the ritual texts 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’ 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 ṣḥb 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 (§74.623bis), p. 652 (§75.527g). On p. 696, T. cites ṣḥb 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 ṣḥb 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 {t(fr)km₁}, 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 /šiqtala/, see above, remark to p. 518 (§74.231). In favor of /šaqtila/ one may cite the common tendency in the Northwest-Semitic languages (Hebrew, Aramaic, and Phoenician) to show /i/—which in Hebrew has become /≠/ 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

---

1074Ibid.
1075Ibid.
and Aramaic. Ford has provided much comparative material for the interpretation by ŠNW; I have argued that the two verbs in this line may be participial in form. Because neither /yaytin-/ nor /yawtin-/ would give /'âtîn-/, 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 (/'atîn-/, cf. Hebrew NTN and Akkadian NDN) or else it behaved like other I-y ← I-w roots in reflecting a biconsonantal root (/'atîn-/). It is equally unlikely that the Š-stem /QTLa/ would have been /sîêtîn-/ (← /saytîn-/), 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., /hōwšîh/ ← /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 /sôtîn-/ (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 /suddunu/ 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 /yattîn/, 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 /'atîn-/ or /'aytîn-, whether {sîtn} represents /sîtîn-/ or /santtîn-/.

— p. 597 (§74.624), p. 640 (§75.517e). T. parses {sîtn[t]} in RS 17.434+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

1076 I see no reason to doubt the presence of this verb in RS 2.[003] iiii 45 (KTU 1.14), though T. would emend the form āšîlw, 'I shall rest', to tšîlw, 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]).


1079Pardee, BiOr 34 (1977) 6-7. The vocalization /'atîn-/ 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 āṭb, '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 /'âtîn-/ (1 c.s. G-/YQTL/), p. 639 8a, /yûtân-/ (3 m.s. G-passive /YQTL/), p. 639 8c, /nâtana/ (3 m.s. N-/QTLa/), p. 597 /sôêtîn/ (m.s. impv. Š-stem), p. 640 /'asêôtîn-/ (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.

1080The 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. (Puduḫepa would be chiding Niqmaddu for having
sent gold to the Hittite king alone, none to her).1081
— 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 √ŠKN,1082 not the Š-stem of √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 ḏḥ w nʿm, 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.1083
— 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 ššʿn in RIH 78/3+:24 (CAT 2.81), viz., as a simple
strengthening of the G-stem (‘to pay’ from NSʿ, 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 ššʿn […] ) 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]’);1084 (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 mšṅqt in RS 3.343+ ii 28’ (KTU 1.15) as a Š-participle f.s.
(“mušēnīq(a)τ-”), 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 makes this explicit.)
— p. 600 (§74.625). In Biblical Hebrew, hiqṭil 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 šmr in RS 24.244:4 et passim (KTU

1081 Pardee, AfO 30 (1983-84) 325, 328.
note 2 and p. 255 with note 176.
1084 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)’. 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 nbš // šmr nbs ‘qšr, ‘My incantation (against) serpent-bite, šmr of/(against) the scaly serpent’, where šmr could either be parallel with ntk nbš 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 {ṭb} in RS 24.250+:32 (KTU 1.106) as an infinitive and emend {ṭb} in line 23 to {ṭ<ṭ>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 {ṭb} in line 23 as G-passive or infinitive (the solution preferred by T. on p. 652) and {ṭb} in line 32 as an imperative addressed to the officiating priest.

— p. 602 (§74.623.3d). T. is constrained by the structure of the passage to admit that šmr 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 (šmr ʿṭkl would mean “das Geschnittene(?) der Trauben”). Far easier, as most scholars have seen, to take šmr as an imperative: lit. ‘cause a bunch of grapes to be cut’.

— p. 605 (§74.632), p. 673 (§75.61e), p. 679 (§75.676). On p. 605 T. very correctly remarks, with regard to {y-sdkmm} 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 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 HRK, “mettre au feu”), 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.

1085 This interpretation goes back to the first major philological interpretation of this text, Astour, JNES 27 (1968) 16.
1087 Ibid., pp. 149, 151, 159 (with previous bibliography on this interpretation in note 29).
1088 Bordreuil and Caquot, Syria 56 (1979), p. 296 “qu’il le mette au feu et chauffe.”
1089 Ibid.
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 yšḥmri may be in the active voice. (4) T. suggests that the subject of {yšḥmri} might be “der Beschwörungspreister,” 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 “Beschwörungspreister”; it may have been a strictly medical text in the same sense as the hippocratic texts or the prose section of the ‘drunkenness of ‘Ilū’ text (RS 24.258:29-31 [KTU 1.114]).

— p. 607 (§74.642), p. 609 (§74.646). T. argues that the Št forms of the root ḤWY (yšṭḥwy, 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 ṭšḥwy is in parallel with ṭpl, ‘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 ḫhr Ṧrîd, ‘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 ḫhr Ṣrîd is an unmarked adverbial, and the verb-complement combination means something like ‘to do obeisance with regard to the gathered assembly.’

— p. 611 (§75.212.11). T. parses {tḥdn[...]} 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,

---

1090 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.

1091 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 *tüdn* in RS 92.2014:8 (RSO XIV 52) from an unexplained hollow root than as a denominative from */'udnu/, 'ear', which would mean 'to hear' (cf. Arabic with a G-stem denominative, and Hebrew *he'ezîn*, i.e., Hiphil). The presence in the following lines of *hw*/*t*, '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.).

1092 Perhaps T.'s preference is owing to the fact that in the manuscript made available to him, Bordreuil and I vocalized the form /ti≥dan¨/,1093 according to the hypothesis that {°} was an alternative orthography for syllable-final /'; and to the fact that, if the root is ʼDN, this would be a jussive form with negative ʼl rather than the expected ʼal. 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 (/huwātâ/, 'word', in lines 9, 10). If so, the vocalization would have been something like /ta≥udun¨/ (← /*ta≥dun¨/),1094 a jussive form with negative ʼl, meaning 'they absolutely must not listen to the word of X' (on ʼ+ 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 ʼôr, 'give light', that Ugaritic ʼôr developed from a /qtal/ base, viz., he assumes the development /ʼwar/ to /ʼâr/, as he would represent it (and actually does for this form, which is 2 f.s. and hence “ʼân†”).1095 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/qtul/qtul/: 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.

1092 T.'s oblique reference to {tûzn} 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 ʼDN (← ʼDN), 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.

1093 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).

1094 This vocalization has been put forward in Bordreuil and Pardee, Manuel (2004) II 69.

1095 P. 448 (§73.243.1), p. 646 (§75.523).
— p. 615 (§75.212.4). T. here describes as certain the interpretation of ʿuḥd in RS 19.096:4, 51, 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 ʿuzr 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 ḏd} in RS 3.322+ iii 14, 28 (KTU 1.19) as {w ṭdū}, 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 (ḥrgb, ‘Hirgabu’, father of the hawks). T. appears to consider nowhere the possibility that {w ḏd} would in fact be the correct reading and that the singular and plural forms would have been distinguished by vowel length (/duʿuy/ → /duʿu/ and /duʿuyū/ → /duʿû/). The problem with this solution is that the preceding form is apparently a 2d 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 (daʿāyu/ → /daʿû/). Of course, at some point the tablet must be collated to determine, if possible, what the scribe actually wrote.

— p. 616 (§75.223). As a G-imperative of LʿK, ‘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 ḏad≠/ < *sa/i ḏad≠/ 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 /qātāl/).

— p. 617 (§75.224). T. cites RS 18.134:13 (KTU 2.44) as an example of {ḻik}, 3 m.s. /QTLa/ of LʿK, ‘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 {ḻik} anything more than a guess.

— p. 617 (§75.224). RS 16.401:3’ (KTU 2.32) is cited as an example of {ḻikt} 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 /raʾā/ ← /raʾaya/. 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 following1096 make the analysis of rā as a m.s. imperative + /-a/ just as likely (the

---

1096 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 *tertiae infirmae* 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 /râ‘âh/ (3 m.s. /QTLa/) shows that proto-Hebrew had /a/ in the second syllable, not /â/.

— 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. /ša‘îl‘kā‘/, ‘and he will ask you’, or /ša‘îl‘šîlîhû/, ‘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, tbū, /tubū‘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 tbū 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 /taglî/ ← /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 /tagli/ (T.’s normalization of the ‘short’ form),1097 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 “PKL 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 ensuing ‘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

1097On my disagreement with this vocalization, see above, remark on p. 190 (§33.311.5) and below, remark to 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 morphosyntactic 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-’ 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 /yṣi/ ← /yṣi’/. 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 /ybu/ represents /yubû’ul/, one might consider that it represents /yubû’/ rather than /yubû/ or /yubû/.

Particularly problematic in this section is the idea that {yṣû}, ‘he lifts’, could represent /yisṣā/ ← /yisṣa’/ (on the particular problem posed by this verb, see further remark below to p. 621).

— pp. 620-21 (§75.232). T.’s vocalization of jussive/perfective forms of III-’ roots generally shows the stem vowel as short, e.g., {yṣi} = /yṣi’/ 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]).

— pp. 620-21 (§75.232). The two 3d-person forms of III-’ roots that are written with {±} as the last sign are here first vocalized with /-â/ according to the theory that Ugaritic did not have a 3d-person /YQTLa/ form (i.e., {±} 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 in taking {tṣû} in RIH 78/20:2 (CAT 1.169) as a 2 m.s. /YQTLu/ form (/tasi’u/), translated as an unmarked volitive (“du muß hinausgehen”), without mentioning the possibility of taking the form as 3 m.pl. jussive /tasi’û/.

---

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

1099 Equally problematic is the /¨/ of “tabû-¨nna,” for, in spite of the hyphen, T. is in fact proposing that the form should consist of /tabû¨nna/, which would have to have become /tabû’unna/ in proto-Ugaritic.


— p. 621 (§75.232). T., the opponent of 2d person forms in the prescriptive ritual texts (see remark above to p. 211 [§41.12], etc.), parses āl tšū in RIH 77/2B+:19' (CAT 1.164) as 2 m.pl. jussive without considering the possibility that it may be 3 m.pl.\footnote{Pardee, Les textes rituels (2000) 836 (translation), 840 (arguments in favor of the analysis as 3d person ).}

— 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 [ū], apparently an indicator that these are all long forms, i.e., /yśū/ /tšū/, etc. Only one class of these usages is noted specifically by T., those that introduce speech ("jeweils PKL in Rede einleitung," 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.\footnote{This explanation also fits the two occurrences of the Gt of this root in poetic texts: yśū in RS 2.[004] v 6' (KTU 1.17) and in RS 3.322+ i 21 (KTU 1.19) precedes the verb YT B, 'to sit', and may express the inception of this act.}

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, tšū and tšā are used to express identical inception of action (one would expect tšūn and tšān to correspond to the third-person singular forms yśū/tšū).

When one verifies the passages, however, one sees that there is in fact only one such irreducible form: in RS 2.[022] ii 16-17 (KTU 1.5) the sequence tšā ghm w tšē constitutes a narrative break and an introduction to speech. In contrast, if one be willing to take tšū 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'),\footnote{Smith apud Parker, ed., Ugaritic Narrative Poetry (1997) 100; Pardee, Context I (1997) 246.} this form would be identified as a jussive rather than as a perfective. In the third example listed here (RS 3.322+ ii 40 [KTU 1.19]), tšā is preceded in the same poetic unit by tmyn, 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 tšā in RS 2.[022] ii 16 legitimately be emended to tšān 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- y root if the data from III- y roots were more systematic but that is, 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!'.

---

\footnote{Pardee, Les textes rituels (2000) 836 (translation), 840 (arguments in favor of the analysis as 3d person ).}

\footnote{This explanation also fits the two occurrences of the Gt of this root in poetic texts: yśū in RS 2.[004] v 6' (KTU 1.17) and in RS 3.322+ i 21 (KTU 1.19) precedes the verb YT B, 'to sit', and may express the inception of this act.}

\footnote{Smith apud Parker, ed., Ugaritic Narrative Poetry (1997) 100; Pardee, Context I (1997) 246.}
— p. 622 (§75.234). T.’s analysis of qrân in RS 2,[022]+ i 23 (KTU 1.5) as G-stem /QTLa/ 3 m.s. will not meet universal acceptance: recently Smith1105 and I1106 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 {yhg/mb/š} whereas T. reads {yhm/gs/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).1107

— p. 625 (§75.32), pp. 677, 678 (§75.673). On p. 625, yhrrm 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 yhrrm 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., /lik-/, all behave precisely like one category of I-y roots, the yĕšêb type. For Ugaritic, this is illustrated by the 1 c.s. /YQTL/ form, written {ālk} (∼ {āt̄b}, ‘I sit’, ∼ Hebrew ēlēk/ēšēb), all of the imperative forms, which show only the second and third root consonants ( {lk} ∼ {ṭb}, ‘sit!’, ∼ Hebrew lēk/šēb), and the aforementioned verbal noun ( {lkṭ} ∼ Hebrew léket/šēbet). 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., /yâṭib-/, 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 YT˙B, 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

1105 Apud Parker, ed., Ugaritic Narrative Poetry (1997) 142


1107 Pardee, “RIH 77/27, RIH 77/12, RIH 78/26 et le principe de l’écriture cunéiforme alphabétique,” forthcoming in Syria 79.
attested twice in Biblical Hebrew, the /YQTL/ form in both cases shows the full root HLM whereas Ugaritic shows only the ylm pattern. The imperfective type is also different from that of yēšēb, viz., /yaqtil/ rather than /yaqtul/. T. assumes that the Ugaritic form is /yalum-/ and that it is derived directly from /yahlum/, but the differences just mentioned plus the fact that the imperative of HLM in Ugaritic is hlm (the imperative of this root is not attested in Biblical Hebrew) lead to the conclusion that the pattern of this root in Ugaritic more plausibly resembled those I-y roots in Hebrew which show doubling of the second root consonant in the /YQTL/, e.g., yisšōr, ‘he will form’. This the Ugaritic /YQTL/ form may well have been /yallum-/, rather than /yalum-/. Only by ignoring the important differences just discussed is it possible to assume that ylk and ylm showed identical patterns in Ugaritic simply because the second root consonant of both is /l/ (p. 624 [§75.31]; above, p. 160 [§33.142.3b], T. observed that other I-h roots with /l/ as the second radical do not so behave).

— p. 625 (§75.331a). T. classifies here three tokens of the writing {hlkt} as 3 f.s. /QTLa/. The first two occur in contexts too damaged to allow a distinction between /QTLa/ and participle (RS 1.008:5 [KTU 1.45] and RS 2.021:4 [KTU 1.62]). The third appears in a well-preserved passage (RS 22.225:1 [KTU 1.96]) and the analysis as a participle is just as likely, if not more so (see above, remark to pp. 596, 597 [§74.624] on the parallel verb šnwt).

— p. 625 (§75.332a). Because of the broken state of RIH 77/8A, there is no way of being certain either that {ylk[...]} in line 33' (CAT 1.166:25) is complete or that it is to be parsed as 3 m.s.

— p. 625 (§75.332b). lk in RS 2.022+ iii 13, 14, 20, 27, 28 (KTU 1.5) is confidently parsed as a m.s. imperative with no statement regarding the relationship of this word to the word ilm that follows immediately in lines 14 and 20. Others have seen the imperative as addressed to these ‘gods’ (the form could be either dual or plural). Unfortunately, the tablet is here too damaged to permit any certainty—all the more reason for the grammarian not to classify such forms precisely and with no sign of doubt. Even more dubious is the case of RS 3.340 i 27 (KTU 1.18), where lk immediately precedes ilk in a broken passage: the parallelism of an imperative and a jussive/imperfective is always possible, but far from certain, and lk may here, therefore, be the preposition l + pronominal suffix. In RIH 78/20:10 (CAT 1.169), the latter analysis may even be judged preferable to T.’s analysis as an imperative of HLK—he should in any case have indicated that his interpretation is not universally accepted.

— p. 626 (§74.42). {ydb} does not appear in just the name ydbil in RS 24.246 (KTU 1.102) as T. asserts here, but also in ydbbb‘l and in ydbhd in the same text. Because √DBB, ‘to speak’, appears in Ugaritic only in malam parte, T.’s first etymology of the form, viz., NDB, ‘to offer voluntarily’, is more plausible than this one. T. does not enter into the discussion

regarding whether this and the other verbal forms in the proper names of this list are /YQTLu/ or /YQTLØ/: 1111 he simply parses this form as /YQTL/, without any specific identification among the sub-forms.

— p. 627 (§75.42). T. qualifies the reading of {tqr} in RS 16.402:13 (KTU 2.33) as “sehr unsicher.” My collation shows the reading not only to be uncertain but to be unacceptable. The first two signs may in fact be {lb₁}.

— p. 627 (§75.42). The /YQTLØ/ form of the hollow root Nh, 3 f.s., cannot be “tanû hå” (cited here are RS 2.009+ iii 19 [KTU 1.6] and RS 2,[004] ii 13’ [KTU 1.17]), for the second syllable is closed. 1113 Vocalize /tanûh/. Curiously, in the continuation of the same section on the next page, T. vocalizes another hollow root form, yns, ‘may he (not) flee’ (RS 02,[008]+ iii 5’ [KTU 1.4]), as “yanus,” said to be derived from “yanûs” (the macron may be a simple typographical error since the very same form is said on p. 644 [§75.522] to be derived from “yanûs”—on the problem of the circumflex vs. the macron, see also above, seventh general remark).

— p. 629 (§75.46). T. parses nqh in RIH 78/20:5 (CAT 1.169) as G-imperative f.s. without mentioning that the form has also been taken as G-stem /QTLa/ 3 m.s. 1115

— pp. 630-32 (§75.511d-g), p. 641 (§75.519). One cannot accept T.’s identification of /YQTL/ I-y forms in Biblical Hebrew of the yêšêb type as based on a true I-y root, i.e., as developed directly from /yayšib/, for the simple reason that these forms are virtually never spelled plene, as would be expected if the historical syllable were a diphthong (contrast yêšêb with yîšan or bêvî). As regards his reconstruction of the corresponding Ugaritic forms, T. chooses to go against comparative Semitic evidence by vocalizing them with /î/ (/yîda≤-/ “he knows’) or /â/ (e.g., /yâ®ib-/ “he sits’), that is, to note in his reconstruction of the form that a contraction has taken place of the vowel of the preformative and the first root consonant resulting in a long vowel (/yiwda≤-/ → /yîda≤-/ or /yaw®ib-/ → /yâ®ib-)—neither Hebrew nor Arabic shows any trace of such a development. 1116 The vowel quality of each

1111Pardee, AuOr 16 (1998) 255-60; cf. here above on yrgbb‘l in this text, remark to p. 32 (§21.31), and on yarš-, remark to p. 547 (§74.412.21).

1112Les documents épistolaires (in preparation).

1113See above, seventh general remark, and below, remark to pp. 641-53 (§75.52).

1114The form is vocalized “tanûh-“ on p. 644 (§75.522), i.e., with the hyphen by which T. preserves the virgin length of the contracted vowel (here it is only parsed as 3 f.s., without an explicit statement as to whether the form is /YQTLØ/ or /YQTLu/).


1116One is thus at a loss to explain what he means by: “Aus sprachvergleichenden Gründen darf postuliert werden, daß Formen der G-PK I-y immer einen Reflex des ersten Radikals aufweisen, der sich in der Regel in einer Kontraktionslängung des Präfixvokals manifestiert” (pp. 630-31) when he himself cites Arabic and Ethiopic as showing /yaqil-/yiqal-/ forms. Below, p. 632, he cites Amorite and Akkadian, but both sets of data are a dubious basis for reconstructing Ugaritic because of absence of texts in the first case (Amorite is known principally from personal names) and because of the ambiguities of the writing system in both cases (Amorite is attested only in Sumero-Akkadian syllabic script; the length of the first vowel in the
type is established by the Ugaritic consonantal writing of the 1 c.s. form: e.g., {id\'}, 'I know', and {âtb}, 'I sit'. The pertinent questions are, however, what the quantity of the vowel represented by {â} was and what both the quantity and the quality represented by {i} were. The problems with T.'s reconstructions are: (1) that already alluded to, viz., that other Semitic languages do not prefer these forms (the Hebrew evidence speaks directly against the reconstruction of {id\'} as '{\ıd\′}/ in that /i/ is attested for certain roots, e.g., /y\ı\š\ı\n/ → /yi\ı\šan/, but not for the root YD\', which is /y\ı\’\ı\d\′/); (2) the assumed diphthong contractions are irregular: one could argue about /iw/ → /î/, but I know of no reason to believe that the passage of /aw/ to /â/ was regular (a rule-ordering explanation would be required, of course, wherein /aw/ → /â/ would be differentiated from the standard /aw/ → /ô/). The hypothesis according to which the Ugaritic /YQTL/ forms follow the Arabic pattern by showing no trace of the original weak consonant, viz. that they are /yida\'-/ and /ya\’\ı\b-/ must, therefore, be preferred. In its own way, Hebrew attests to the same pattern. The corresponding forms are /y\ı\d\′/ and /y\ı\’\ı\b\ı\ªb/; the first may represent a simple passage from proto-Hebrew /yida\'/ with secondary assimilation to the /y\ı\’\ı\b\ı\ªb/ pattern as concerns irreducibility of the /\ı/ while the second apparently shows vowel harmony, perhaps by dissimilation from the Hiphil (proto-Hebrew /ya\’\ı\b/ should have become /y\ı\’\ı\b\ı\ªb/ and it would, of course, have been identical to the proto-Hebrew form that became the Hiphil jussive and 'wâw-consecutive', viz., /yaha\’\ı\b/ → /ya\’\ı\b/ → /y\ı\’\ı\b\ı\ªb/ and /wayy\ı\’\ı\b\ı\ªb/).  

— p. 632 (§75.511h). T. does not explain why he prefers to vocalize šq, 'pour' (RS 2.[003]+ ii 18 [KTU 1.14]), as /šaq/ on the basis of the corresponding anomalous form in Biblical Hebrew (2 K 4:41) rather than as /suq/ in conformity with the /YQTL/ /ya\’\ı\s\ı\q/, which is the normal correspondence in the Semitic languages (viz., the theme vowel of the imperative will correspond to that of the /YQTL/). The existence in Hebrew alongside šq of a 'strong' imperative form y\ı\ª\ı\q (Ezek. 24:3) hardly appears to be sufficient evidence to posit that all the Northwest-Semitic languages showed only /a/-imperatives following aphaeresis of the first root consonant.

— p. 633 (§75.512). The form td, claimed here to be a 2 m.s. form of WDY\,1, “niederlegen, ablegen,” and listed as attested in RS 2.[008]+ ii 34’ (KTU 1.4) appears to be a ghost form. It is, in any case, not present in the text cited. Moreover, in the text index (p. 989), only this page and p. 660 are cited as containing references to KTU/CAT 1.4 ii 34, but when one turns to p. 660 one finds no reference there either to this text or to a td 2 m.s. It would appear that a mistaken reference to this text was eradicated from p. 660, but not from p. 633, only after corresponding forms in Akkadian is disputed and is in any case different from Ugaritic, cf. /isim/ and /ubil/). For the cases of Hebrew and Arabic, see below.

1117 This irreducibility counts against the /\ı/ representing the simple lengthening of /i/ to /\ı/ in Hebrew, which should occur only in tonic and pretonic open syllables.

the index had been completed. (Below, p. 660 [§75.532], at the entry for this root as III-γ,
only one token of td from this root is cited, one which appears in RS 19.054:1 [KTU 1.93]
and which is parsed on both pages as 3 f.s.)

etymological basis, and I am not aware of any, for a root YDY ← WDY that would mean
“Sünde/Schuld bekennen’ (alt.: Lobpreis verrichten),” or, as he translates on p. 367, “führen
den Lobritus/Bannritus.” He classifies this root as WDY₃, but the only text in which it would
appear is RS 24.266:22’ (KTU 1.119) where the form is tdn. Others have seen there the
root DNY to which I referred above in a remark to p. 514 (§74.222.3), etc.1120 This appears
to be a case of preferring to cite a root for the existence of which there is no comparative
evidence over the analysis of the verb as 2 m.s. (see remark above to p. 211 [§41.12],
etc.).1121

— p. 633 (§75.512). T. claims the existence of a root WDN that would mean “(ein Tier)
antreiben’ but cites no etymological data for the existence of the root. The two attestations
that he claims (ydn in RS 3.322+ ii 12, 19 [KTU 1.19]) may more plausibly be derived from
DNY, ‘to approach’1122 (cf. remark above to p. 514 [§74.222.3], etc.).

— p. 633 (§75.512). T. indicates that the /YQTL/ theme vowel for YD‘, ‘to know’, is /i/,
though he vocalizes forms below with /a/. It is uncertain whether the classification is a
simple error or whether he is saying that the root belongs to the /ya®ib-/ type (see above,
remark to pp. 630-32 [§75.511d-g]) with secondary shift of /i/ to /a/ because of the guttural in
final position. If the latter is the case, this is no place to indicate the theory, for the shift, if
/yadi‘-/ ever existed, had occurred already in proto-Semitic.

— p. 633 (§75.512). Because of the {-y} on °my in the phrase °my td‘ ky (RS 15.008:6-7
[KTU 2.16]) the noun may be in the vocative (/‘ummiya/ and hence the verb 2 f.s. jussive
(/‘idda≤≠/), rather than 3 f.s., as T. parses it here. The {-y} may, of course, be enclitic,1123
like the one on {ky}, in which case T.’s analysis would be correct (/‘ummiya tida‘ kīya/).

1119There appears to be no case of {td} listed in J.-L. Cunchillos and J-P. Vita, Banco de Datos Filológicos
Ugaríticos en Morfología Desplegada (Madrid: Instituto de Filología, 1995), vol. 2, p. 1974, that would fit
the parsing in question.


1121In UF 33 (2001) 687-88, T. identifies the root in question as one cognate with Hebrew hōdāh, ‘to give
praise’—a semantic leap that requires a great leap of faith to accept. T. claims that the verb can mean
“Sünden bekennen” in Hebrew, but the dictionaries cite only one verse where such a meaning would be
found, Ps. 32:5b, which reads ḥattā(’)tiy ‘ōw di‘y ‘ākā wa‘awōwniy lō(’)-kissiyyi ‘āmarīy ‘ōwdeh ‘ālay
p‘ās’ay layhw® wayattāb nāsā(’)tā ‘āwōn ḥattā(’)tiy. In line with many other usages of the verb and with
the sense of the entire verse, the principal complement of ‘ōwdeh must in the second line be layhw®: ‘(On
account of) my sins (which you have forgiven) I will praise you, for my iniquity I (have not attempted to)
hide; I have said: I will give praise, (on account of) my transgressions (which you have forgiven), to the
Lord; you have forgiven my iniquitous sins’.


1123Not a mater lectionis, as T. analyses it on p. 51 (§21.341.21)! See remark above to pp. 37-38
(§21.322.5), etc.
(On the analysis of the {-y} of ky as the enclitic particle rather than a mater lectionis, see above, remark to p. 52 [§21.341.21c], etc., and below, remark to p. 800 [§83.24]).

— p. 634 (§75.512). Ugaritic and Hebrew clearly show different forms of one I-y root: [ârt], ‘I will take possession of’ (RS 3.367 i 19’, 35’ [KTU 1.2]), shows the Ugaritic form to have been /yariṭ-/ whereas the Hebrew form is /yīrāś/. This is apparently what T.’s notation “TV /i/” (i.e., the theme vowel is really /i/) is meant to express, for, as we shall see, Hebrew is the odd one out among the other West-Semitic languages. The existence in Hebrew of two imperative forms, /rēš/ and /rāš/ (only attested in pause as /rāš/), shows that the /YQTL/ in proto-Hebrew may well also have been /yariṣ-/. If so, it may have re-formed by dissimilation to the /QTLa/ which was /yariṣa/, as is shown by forms with suffixes, e.g., wiṭrēšūwka, ‘and they will take possession of you’ (Ezek. 36:12), in agreement with Aramaic (/ywrêt/, /ywrēt/ in Syriac) and Arabic (/warīṭa/). The attestation of [ârt] in Ugaritic appear to require the hypothesis that the /YQTL/ form had not there undergone this dissimilation and that the theme vowel was, therefore, identical in the two conjugations (/yariṭa/yariṭ-/). This is the case in Arabic where the two forms are, however, much more clearly distinguishable because the root is I-w (/warīṭa/ vs. /yariṭ-/), and in Aramaic, where the situation is more like the Ugaritic one would have been (/ywrēt/ vs. /yērēt/).

— p. 634 (§75.512). If the /YQTL/ form of the verb YŠN (← WŠN), ‘to sleep’, was indeed /yīšan-/, as T. proposes, it is hard to believe that it developed directly from /yiwšan-/ as he also proposes, i.e., that the form is not built off the secondary root YŠN (/yīšan- ← /yišan-/). There is no internal evidence from Ugaritic on the vocalization of the form, and there is no way of knowing whether it followed the pattern that gave /yīyšan/ in Hebrew or another. Arabic shows the /yawqal-/ pattern expected there. To account for the Hebrew patterns, one must posit that, when the I-w to I-y shift took place, the /YQTL/ forms re-formed on three patterns: all /yaqtil-/- forms plus YD‘ (all of these were I-w) formed on a /yatil/yital/ pattern, the other I-w roots re-formed their /YQTL/ on the new I-y root, while some if not all of the original I-y roots patterned on I-n roots (e.g., /yasṣur-/, ‘he forms’, and /yasṣiq-, ‘he pours’—nowhere does T. discuss the reconstruction in Ugaritic of I-y/w forms that in Hebrew show /u/ as the stem vowel of the /YQTL/ and, frequently, a doubled second radical). As a corollary of this hypothesis, one must infer that the Barth-Ginsberg law took effect after this I-w to I-y shift and that monophthongization took place subsequently to both of these developments: /wašina/yawšan- ← /yašina/yayšan- ← /yašina/yiyšan-/ (Barth-Ginsberg law) → /yāšen/yīšan-/.\textsuperscript{1124}

— p. 634 (§75.512). In a long list of forms written {yṭb} that are classified as 3 m.s. /YQTL/, for some the indication that the form may alternatively be taken as /QTLa/ is added while in other cases no doubt is expressed. The classification of RS 1.003:7 (KTU 1.41) as one of the first group may not be considered a likely one, for in these prescriptive ritual texts virtually all finite forms are imperfective and the chances that yṭb in this text is perfective must be judged to be very small indeed.

— p. 635 (§75.512), p. 746 (§81.22h), p. 797 (§83.213). Out of the blue and without a question mark or any other indication of the novelty of his proposal or of the irregularity of

\textsuperscript{1124}On the case of YRT, see preceding remark.
the phonetic equivalence, T. etymologyzes {y®q} in RS 24.244:6 et passim (KTU 1.100) by Akkadian “wasāqu” and translates “stärken, aufrichten.” In the other two paragraphs cited, he translates by “fesseln,” in accordance with the line of interpretation and etymological identification, viz., Arabic watqa, ‘to bind’, that have been broadly accepted since the publication of the text in 1968.1125

— p. 635 (§75.512). In an “Anm[erkung]” to the entry devoted to /YQTL/ forms of the verb YP’, ‘to arise,’ T. cites the personal name {ya-a-pa-â-u} attested at Ugarit (RS 19.042:9 [PRU VI 79]) as well as {ia-ap-pa-a[h-d][M]} attested in the Amarna text 97:2, with the remark that neither shows adherence to the Barth-Ginsberg law (i.e., /yaqtal-/ → /yiqtal-/), but without an explanation for the anomaly. Two come immediately to mind: (1) the names are neither Ugaritic nor Canaanite—this is one of the principal isoglosses separating certain Amorite names (hence, apparently, dialects) from Ugaritic and Canaanite; (2) the forms are not /YQTL/ but /QTLa/ (the [a] in the syllabic writing could be a phonetic indicator for the correct reading of the [PI]-sign rather than an indicator of vowel length; as for the Amarna example, orthographic gemination does not necessarily correspond to grammatical gemination).

— p. 635 (§75.512). As is shown by three other /QTLa/ forms in RS 13.006 (KTU 1.79), viz., lq” in lines 4 and 6 and db” in line 7, ytn in line 2 is in all probability also /QTLa/,1126 not /YQTL/ as T. parses it here. Curiously, he parses the same form in RS 15.072:2 (KTU 1.80), a text which shares many features with RS 13.006,1127 as /QTLa/.

— p. 635 (§75.512). If one does not admit the necessity to emend {≤ly} to {<b>≤ly} in RS 16.402:26 (KTU 2.33) (see remark above to p. 61 [§21.354.1c]), then the verb ytn at the beginning of the line is not /YQTL/ but /QTLa/ (/lêma la yatanahumu malku ≤alayya/, ‘Why has the king surely imposed them upon me?’).1128

— p. 635 (§75.512). RS 1.032 (KTU 2.9) is too damaged to allow certitude that ttn in line 5’ is 2 m.s., as T. parses the form here, with no indication of doubt. Somewhat ironically, but more properly, he places {yd‘} in the following line among examples of uncertain parsing of the root YD‘ (p. 640 [§75.518]).

— p. 635 (§75.512). The same is true of the same form in RS 2.[026]:2’ (KTU 2.9), where all that is preserved of the line is {[...]¯p˘ß . ttn[...]} . Though the restoration ‘to the Sun you must give’ is certainly plausible, it is anything but certain.

1125Cf. Pardee, Les textes para-mythologiques (1988) 210, with bibliography; more recently, Parker, apud Parker, ed., Ugaritic Narrative Poetry (1997) 220; Wyatt, Religious Texts (1998) 380-84. At one point, Dietrich and Loretz explained the Ugaritic form by Akkadian šaqû, ‘to be high’ (UF 12 [1980] 160), an explanation ruled out by the fact that the Arabic equivalent of the Akkadian verb is šQY rather than TQY; more recently (Studien [2000] 328), they have returned to the explanation by Arabic WTQ, choosing the nuance “stärken” of that root (in the course of the remark, they credit T. in this paragraph of the grammar with the comparison with Akkadian wasāqu, in spite of the phonetic difficulty that the comparison presents).


1127Ibid., pp. 428-38.

— p. 635 (§75.512), p. 722 (§77.322b). The reading {ttn} in RS 18.113A+B:32' (KTU 2.42:20) must be accepted as the only plausible one, for it clearly corresponds to the scribe’s final wish.1129 In the first paragraph cited, T. indicates {tttn}, but one finds “t'ttn” in the second and a reference to KTU/CAT’s reading, following the editor,1130 of {ättn}.

— p. 635 (§75.512). T. parses tttn in RS 17.117:20' (KTU 5.11) as 3 m.pl. with no indication of doubt, choosing to ignore that others have taken the form as 2 m.s.1131

— p. 635 (§75.513). T. parses d‘, ‘know’ (RS 17.139:28 [KTU 2.34:30]), as G-imperative 2 m.s. The letter is, however, addressed to a woman, as he recognizes elsewhere (e.g., p. 489 [§73.525]), to the point of having her give birth (see remark above to p. 257 [§51.41h], etc.).

— p. 635 (§75.513), p. 902 (§97.21). On p. 635, T. does well to reject the new reading of RS 16.402:21 (KTU 2.33) in CAT (‘yd’t’), but on p. 660 allows for the restoration of the [y] (he there gives the reading as [{x}d’t’]). In both cases, he fails to mention my epigraphic remarks on the reading of the first of these signs, to the effect that the most plausible reading is {l}, viz., that of KTU.1132 T. first parses the signs {d’} as a G-imperative, 2 f.s., then on p. 902 considers both that analysis and the restoration/analysis as 3 m.s. to be possible. If the first sign ends with a vertical wedge, as I have claimed, this latter interpretation is impossible and the first may only be said to exist in a vacuum until the first sign is explained.

— p. 636 (§75.513). T. parses four tokens of the sign sequence {tn} in RS 17.117 (KTU 5.11) as certain examples of the G-imperative 2 m.s. of YTN, ‘to give’ (ll. 9, 15, 17, 20), but fails to mention that others who have studied the text do not agree on this analysis of all four examples.1133

— p. 636 (§75.513). Few will accept T.’s analysis of the two tokens of tn, ‘give’, in RS 24.244:73 (KTU 1.100) as 2 f.s., for the speaker is usually taken as female and the addressee as male.1134

— p. 636 (§75.514). T. presents the only options of analyzing {yd‘t} in RS 2.[026]:3' (KTU 2.9) as 1 c.s. or 2 m.s., but the text is too damaged to rule out the analyses as 2 f.s. or 3 f.s.

— p. 636 (§75.514). In spite of having stated above (p. 465 [§73.331.3]) that the vocalization of the 2 m.s. pronominal element of the /QTLa/ could be /-ta/ (in agreement with all the West-Semitic comparative data), and there leaving the question open, T. here

1129Pardee, UF 19 (1987) 208 (line 21’).

1130Virolleaud, PRU V (1965) 14, 15, copied and transcribed {ättn}. This reading was followed by Dietrich and Loretz in Die Elfenbeininschriften (1976) 21 and by Dietrich, Loretz, and Sanmartín in both KTU and CAT. In CAT, however, a note has been added to the effect that one might read {tttn}, but without citing my comments in UF 19 nor the manuscript which I sent to them in August of 1994, in which this reading was indicated. Nor does T. cite either of these sources for the reading {tttn}.


1133Cf. Pardee, AfO Beiheft 19 (1982) 43-45, where the author’s analyses are compared with the editor’s (Caquot, Ugaritica VII [1979] 389-98).

vocalizes this form of the root YRD as "yaradtā" (see remarks above to p. 143 [§33.115.11] and to p. 207 [§41.1]).

— p. 636 (§75.514). I see no certain way of determining whether yṭb, 3 m.s. of the verb YTB, 'to sit', in RS 2.002:8 (KTU 1.23) is /YQTL/ or /QTLa/—T. parses as the latter, with no indication of the motivation of his choice. Having admitted the uncertainty, however, I would observe that all three verbs in the following verse (yzbrnn, yṣmdnn, and yṣq̄) are /YQTL/ and this fact certainly favors the same analysis of yṭb.

— p. 636 (§75.514). T. parses yṭb in RS 24.245 (KTU 1.101) as 3 m.s. /QTLa/ of the verb YTB, 'to sit', with no mention of the fact that the form has been taken as /YQTL/1135

— p. 636 (§75.514). I see no way of determining with certitude that yṭb in RS 24.258:14, 15 (KTU 1.114) is each time /QTLa/. No reference is made to the fact that the forms have previously been analyzed as /YQTLu/1136 (T. parses the form first as /QTLa/, mentioning the other analysis as an option).

— p. 636 (§75.514). T. parses yṭbt in RS 34.124:21 (KTU 2.72) as 3 f.s. /QTLa/, with no indication of uncertainty. The analysis as a G-participle, f.s., is at least as likely, if not more so.1137

— p. 637 (§75.514). T. remarks that the reading of {ytn} is not clear in RS 18.[507]:5’ (KTU 4.573), but nevertheless includes this as a certain example of /QTLa/, 3 m.s., of YTN, ‘to give’. In point of fact, the fragment is so small that no such precise analysis can be considered certain. The same must be said of RS 19.100A:6 (KTU 4.637), also listed as a certain example of the same form.

— p. 637 (§75.514). ytn in RS 24.292:2 (KTU 4.728) is here listed as a certain example of /QTLa/, 3 m.s., in spite of the facts (a) that it was translated as a plural on p. 235 (§43.12) and (b) that its subject was specifically analyzed as a plural there and on p. 262 (§51.43k). On the interpretation of this passage, see above, remark to p. 235 (§43.12), etc.

— p. 637 (§75.514). Because RIH 78/04 (CAT 1.173) belongs to the category of prescriptive rituals, it is far more likely that ytn in line 17' belongs to the /YQTL/ than to the /QTLa/, T.’s preferred analysis here.1138

— p. 637 (§75.514). After offering grammatical classifications with no sign of doubt for several tokens of {ytn} found in very fragmentary contexts (see several of the preceding remarks), T. classifies ytn in RS 18.140:17' and 20' (KTU 2.45:18, 21), a relatively long and well-preserved passage, as /QTLa/, unless the form be /YQTL/ in one case or the other (“sofern nicht G-PK 3.m.sg.”). Here the context allows for the parsing of the first as /QTLa/, the second as /YQTLu/: (16’) {w . mlk. ššwm . n’mm (17’) ytn . 1f. 1 ’bl’dyr’l(h) (18’) w . mlk . zm l . št ln . ššwm (19’) n’mm . lk . l.ttım l [.] lw l . ãt (20’) ngt . w . ytn . hm . lk}, ‘Now

1135 Pardee, ibid., p. 124, 130.
1136 Pardee, Les textes para-mythologiques (1988) 21; see here remarks here to p. 648 (§75.524), p. 711 (§76.524.5a), and p. 713 (§76.525).
1138 Pardee, Les textes rituels (2000) 852 (the editors’ translation was ambiguous: Bordreuil and Caquot, Syria 57 [1980] 354: “il place”; the same is true of the other treatments that I cited under “principales études” on p. 850 of Les textes rituels).
the king has given good horses to ‘Abdiyariḫu and the king (of?) ZM has had good horses sent for you (in the number of) sixty. Just go to (the land of) Nuḫaššē and he will give them to you.’

— p. 637 (§75.516), p. 667 (§75.536b). On p. 637, T. parses ydy in RIH 78/20:1 (CAT 1.169) as a G-infinitive of the /qatāl/ type, functioning as a “Verbalsubstantiv”; on p. 667, the same form is classified among “Verbalsubstantive” of types other than /qatāl/. Though an analysis as a verbal noun is certainly possible, T. might have mentioned the equally plausible analysis as a /YQTL/, 3 m.s., with the incantation itself as the subject: ‘(This) will drive out…’.

— p. 638 (§75.517a). T. vocalizes the G-passive /YQTL/ of YŠQ as /yûšaqa/-, assuming that /yuysaq/- would have contracted in this fashion. If, however, the G-active was /yassuq/-, as is the case in Hebrew (cf. above, remark to p. 634 [§75.512 YŠN]), the G-passive would have been /yuṣṣaq/. Moreover, a large portion of the forms cited here are from the hippiatric texts, where yṣq is more plausibly active than passive (see above, remark to p. 511 [§74.222.2], etc.).

— p. 640 (§75.517e), p. 865 (§93.342). On p. 640, T. parses [štn] in RS 17.063:9 (KTU 5.10) as 3 m.s. without mentioning the fact that the form has been analyzed as f.s. impv. The editor had read the third word of the line [‘zn], ‘Uzzīnu (personal name), and made this ‘Uzzīnu the subject of the verb štn; he was followed by KTU in this reading. I read the word [tzn] and was followed in this reading by CAT; I made it the object of the verb, taking tzn as designating a commodity of some sort (it would be the masculine variant of tznt, attested twice in economic texts). On p. 865, T. leaves the question of the reading open (“‘/tzn’”) and maintains Caquot’s analysis of the word as a personal name. He does not, however, discuss the reading, of which I judge the {t} to be almost certain.

— p. 640 (§75.518). This paragraph is devoted to “Nicht sicher deutbare Formen.” As should be clear from remarks here above, many forms should be removed from certain classifications and placed here.

— p. 640 (§75.518), p. 679 (§75.676). In an “Anmerkung” to each of these sections, T. claims that the root WDD, to love’, (which would, of course, have become YDD in Ugaritic), is only attested by the nominal derivatives mdd(t) and tdd, each time qualifying the assertion by “offenbar.” Above, however, he listed yd in RS 2.[008]+ iv 38’ (KTU 1.4) and RS 2.[014]+ iii 6’ (KTU 1.3) as a /qatāl/ form of that root (p. 253 [§51.41a]). This analysis is,

---


1140No vocalization is provided for the G-active (p. 635 [§75.512]), and no discussion is provided in the introduction (pp. 630-33 [§75.511]) of the problems involved in the reconstruction of I-y/w roots with stem vowel /u/ in the /YQTL/. On T.’s (dubious) vocalization of the G-imperative or YŠQ as /ṣaq/, see above, remark to p. 632 [§75.511h]).


1142Caquot, Ugaritica VII (1979) 392.
indeed, more plausible than taking it as the word meaning ‘hand’, 1143 because it is in both cases paralleled by åhbt, ‘love’.1144

— pp. 641-53 (§75.52). For a general statement on T.’s presentation of hollow roots, see above, seventh general remark. Pedagogical questions aside, the principal problems are (1) the inconsistency of presentation of the stem vowel in closed syllables as long or short and (2) the absence of a theory differentiating vowel “contraction” that resulted in a vowel identical to an original pure long vowel (e.g., “/û/” = /û/) from monophthongization of diphthongs that produced a new and invariable vowel (e.g., /aw/ → /ô/). An additional frustration that the reader may feel while working through the section on the /YQTL/ (pp. 643-46 [§75.522]) is T.’s reticence to parse forms as ‘short’ (/yaqul/) or ‘long’ (/yaqûlu/, or, as T. notes it, /yaqûlu/). Sometimes he does, sometimes he does not, and no explanation is offered for why one form is completely vocalized, another not. Just one example: in the first line of the entry on PWQ, ‘to acquire’ (p. 645), a passage is cited from the KirIa epic in which there seems to be no particular reason to doubt that ypq would have been perfective, viz., /yapûq/, ‘he acquired’ (RS 2.[003] + i 12 [KTU 1.14]), then three lines are cited from omen texts where there seems to be no reason to doubt, judging from third-person plural forms written with {-n} in the principal text (RS 24.247+ [KTU 1.103+]) and the general semantics of the genre, that ypq would have been imperfective indicative, viz., /yap¨qu/, ‘he will acquire’. T. indicates only “yapûq-” as the vocalization for all the passages cited.

— p. 642 (§75.521c). In his discussion of whether hollow roots had a “Bindevokal” between the verbal stem and the pronominal element in the /YQTL/, T. presents the vowel in question as /ä/. Such a stance appears to show more reticence than is habitual with T., for Akkadian shows /ä/ in this slot (resulting from paradigm alignment on the 1 c.s. form1145) and Hebrew shows /ô/, which can only have come from /ä/ (e.g., /h aq≠mªta/, “you caused to rise’ ← /haq≠mªta/). See also remarks below, to pp. 647-48 (§75.524) and to p. 664 (§75.534), on the matter of T. actually representing this “Bindvokal” in Ugaritic as /ä—I.e., without even indicating the possibility of /ä—, a reconstruction that must, on the basis of the comparative data, be rejected out of hand for Ugaritic.1146

1144Cf. del Olmo Lete and Sanmartín, ibid. They also include under this heading RS 5.194:8 (KTU 1.24).
1145See the chart in Gelb, Sequential Reconstruction (1969) 176.
1146In T.’s theoretical study of the phenomenon (“Die Endungen der semitischen Suffixkonjugation und der Absolutivkasus,” JSS 44 [1999] 175-93), he concludes that both proto-Akkadian (p. 183) and proto-Arabic (p. 184) had a short vowel between the verbal and the pronominal element, e.g., “šālim-a-tā,” and that the /ä/ in this slot in proto-Hebrew and in non-paradigmatic Arabic forms was secondary (pp. 185-86). This is on the one hand a very precarious reconstruction—since all the explicit data from the Semitic languages are for the vowel being or having been /ä/—and on the other ignores Gelb’s hypothesis developed in Sequential Reconstruction (not cited in the bibliography though two others of Gelb’s works are) that plurality was associated in proto-Akkadian with vowel length (i.e., according to Gelb’s hypothesis, by lengthening of the singular morpheme). In Gelb’s view, the proto-form of the singular pronominal elements would have contained a short vowel following the consonant (i.e. /ta/ instead of /tā/), which happens to agree with the data from the three principal branches of the Semitic languages (e.g., 2 m.s. /-ta/ in both East and West.
— p. 642 (§75.521c), pp. 647-48 (§75.524). On p. 642, T. vocalizes /QTLa/ forms of the root M(W)T, ‘to die’, as showing either /â/ or /û/ as the stem vowel (e.g., “mâ/û’tu’’); on pp. 647-48, he proposes only /â/. No explanation is offered for why Ugaritic would differ in this lexical item from Hebrew and Aramaic, which both show the proto-base /mît-/.

One may assume that he expects the form to show anything but /î/ because the /YQTL/ is /yamût/- and the original root would have been MWT (cf. the Arabic pattern /qâla/qumta/ for II-w roots), but theory cannot displace data. An additional remark on forms of this type from this root: on p. 642, T. reconstructs the forms that show two tokens of {t} with the linking vowel /â/ but on pp. 647-48 he indicates “mât(a)tu” for the 1 c.s., “mât(a)tâ” for the 2 m.s., reconstructions that can only be described as wanting in all respects: (1) it is unlikely, judging from Hebrew and Aramaic, that the first vowel was /â/; (2) the linking vowel must have been /â/, not /a/; (3) the form written with two tokens of {t} cannot have had a linking vowel because /matta/ would be written with only one {t} (T.’s appeal to historical writing as an explanation [p. 642] finds no parallels in Ugaritic orthography); (4) all comparative evidence for the vowel of the 2 m.s. pronominal element shows that it must have been short (see above, remarks to p. 207 [§41.1] and p. 465 [§73.331.3]).

— p. 642 (§75.521c), p. 647 (§75.524). On p. 642 without a question mark but on p. 647 with a question mark, T. parses {mtt[...]} in RS 3.367 iv 1’ (KTU 1.2) as 1 c.s. G-stem /QTLa/ of the root M(W)T, ‘to die’, without noting that there is a break after these three signs. The break means, however, that any analysis is purely hypothetical. 1 c.s. /YQTL/ forms characterize the following passage, but there is no way of ascertaining whether this and {[[...]]ttt} (on which see below remark to p. 675 [§75.64]) in this line are /QTLa/ forms in the same grammatical person.

— p. 642 (§75.521c), p. 648 (§75.524). I fail to see on what grounds T. classifies {sšt} in RS 17.434+:47’ (KTU 2.37:5) as a 1 c.s. form, when the entire context is too badly damaged to allow for analysis of any kind, let alone as a specific verbal form. The question mark attached to the analysis is insufficient to convey to the reader how totally arbitrary the classification is.

— p. 642 (§75.521d), pp. 648-49 (§75.525). T. remarks that the participle of hollow roots shows a ‘weak’ form, i.e., without the hypothetical middle radical, compares Hebrew, and reconstructs the Ugaritic form as “qâm-” ← “*qâw(i)m.” Since he chose to compare Hebrew, he might have attempted an explanation for why, if proto-Hebrew also had /qâm-/, the /â/ did not go to /ö/, or, if proto-Hebrew was /qâm-/, the qames of the form in Biblical Hebrew is

Semitic, /-ka/ in Ethiopic). All the available data—certainly for West Semitic!—indicate therefore that the proto-form would have been /QTL-âta/ and there is at present no basis on which to propose any other reconstruction for Ugaritic.

1147 As is normal in Hebrew, the /QTLa/ forms have all assimilated to the pattern with no vowel between the verbal stem and the pronominal element, viz., /mīt/mītâta/ → /mît/mitta/ → /mêt/mattâ/ (the second of this last set by Philippi’s law). Compare /qâm/qâmâta/ → /qam/qamta/ → /qâm/qamtâ/ and contrast Aramaic, where the entire paradigm assimilated to the /mît/-/qâm/- stems after the rule disallowing long vowels in closed syllables was no longer operative.

1148 Pardee, AoO 30 (1983-84) 323, 326.
irreducible (as in qâmê YHWH, ‘those who rise up against the Lord’). That the vowel is secondary is shown by the corresponding /i/-type participle (e.g., mêtê milhamâh, ‘those who have died in battle’). I know of no process that produces irreducible /â/ in an environment similar to that which produces irreducible /ë/.

In his treatment of G-participle forms on pp. 648-49, T. proposes only one possible form as having an /e/-type vowel, viz., B(Y)N, ‘to understand’, might show the participial form /bên-/ ← /bâyin-. One must remark, however, that, at least in Hebrew, the active vs. stative participles of hollow roots show no correlation with presumed /w/ or /y/ as the original middle radical (as is illustrated by the verb ‘to die’ just discussed, a prime candidate for an original II-w root). T. recognizes this implicitly by vocalizing the participle of Š(Y)R, ‘to sing’, as “šâr-,” not /šér-/ (“bân-” was also his first vocalization of the participle of the root meaning ‘to understand’). Nor is there any reason to expect these two participial forms to be linked with original /w/ or /y/, as stativity is expressed by internal vowel pattern, not by root consonants. There is, therefore, no reason to expect that the /ë/-participles in Hebrew should have come from an original form /qâyil-/; and, in terms of the discussion above in this remark, if the /â/-participles had come from /qâwil-/ that should have occurred at so early a period that the resultant /â/ should have become /ö/ in Canaanite, which did not happen. The problem of the development of hollow-root participles remains not only unsolved here but essentially unaddressed.

— p. 643 (§75.522), p. 801 (§83.24a). In the first reference cited, ybt in RS 16.402 (KTU 2.33) is parsed as /YQTLu/ from the root BYT, “übernachten,” and vocalized accordingly as “yabîtu,” but there is no translation of the form in context. In the second, one finds the translation “die Nacht verbracht hat.” Since /YQTLu/ forms normally do not have preterital value in T.’s view of things (and certainly not in mine), this apparent discrepancy should have been explained.

— p. 644 (§75.522). {tbn} in RS 16.266:11 (KTU 1.83) is indeed a “n[eu] e[esung]” as compared with CAT’s reading of {tb’n}. The reading was proposed by T. in 19961149 and was corroborated by Pitard.1150 (My collation, done in 1981, and again in 1996 in collaboration with Bordreuil, but never published, agrees with the reading of {t} in this word.1151)

— p. 644 (§75.522). {ykn} in RS 34.148:7 (CAT 2.75) is here analyzed as a 3 m.s. /YQTL/ form of the root K(W)N, ‘to be’, but the passage is nowhere translated. When viewed in context, that analysis appears impossible. The text reads {wlñyknpt (8) ‘m. ml. grgsm}. In the editio princeps, Bordreuil and I divided line 7 as w ln ykn pät, “Et pour nous que les marches soient solides …,”1152 but that analysis now appears unlikely to me, for a 3 f.pl. /YQTL/ form (assuming that pät is plural) should not have a {y}-preformative. It appears, therefore, necessary to adopt the division that we considered as an alternative, viz., w lny kn

1151 Cf. my textual remarks on this text, AuOr 16 (1998) 89-90 (p. 89, remark to RS 16.266:4, on this particular correction to CAT).
1152 Une bibliothèque (1991) 163, 164.
pát, ‘and for the two of us (i.e., the writer and the addressee), the border areas are firm (towards the kin-gd>om of Carchemish)’.\footnote{Cf. Pardee, Context III (2002) 105.}

— p. 645 (§75.522), p. 692 (§76.343c). In the first section cited, T. parses t‘n in RS 2,[014]+ ii 23 (KTU 1.3) as /YQTLØ/, whereas in the second he reasons from the form with which it is in parallel, thdy, that it should be /YQTLu/. As regards the etymological analysis of the form, the two sections agree in identifying the root as ‘(Y)N, ‘to eye’, a solution that is much superior to T.’s subsequent proposal to find here a cognate with Hebrew ‘NH, ‘to sing’.\footnote{‘Anats Kriegsgeschrei (KTU 1.2 II 23),” UF 33 (2001) 567-71 (where the fact that Arabic shows GNY for this meaning is glossed over far too glibly—phonological inconsistencies certainly exist but one should not adopt such a solution in a given passage unless standard phonological rules provide no good interpretation).}

— p. 645 (§75.522), p. 699 (§76.423). On p. 645, T. cites a form tšhn that is said to appear in RS 3.322+ ii 40 (KTU 1.19), parses it as “PKI- 3.m./f.du.”, and vocalizes it “tašṭáhnī”; on p. 699 the same reference is given for a form that is said to be tšh and is parsed as “PKKi [/YQTLØ/ functioning as a narrative perfective] 3 m.du.” In neither section does he indicate that the form in question is entirely restored. It is unclear why T. would wish to restore the long form, since on p. 699 he makes a point of the short form being in sequence with another short form (tnšā); moreover, the short form is restored in CAT, though the long form was restored in KTU.

— pp. 645-46 (§75.522). T.’s assumption that the /YQTL/ of QL, ‘to fall’, would have had /i/ as its stem vowel is apparently based on the assumption that the proto-Semitic root was QYL, a conclusion that is consonant with the Akkadian form qišlu. It is thus superior to my first vocalization with /u/,\footnote{AfO 33 (1986) 145.} to my second with /ā/,\footnote{Les textes rituels (2000) 271, 604.} and infinitely superior to my maintenance of /ā/ along with /ā/ in a single publication!\footnote{Ibid., p. 549, where I simply repeated the vocalization proposed in AfO 33 without aligning it on the other two tokens of the verb in that collection of texts. I obviously had not given adequate thought to the problem. Three such forms are properly vocalized with the appropriate /ā/ vowel in Bordreuil and Pardee, Manuel (2004) II 12, 16.}

— pp. 645-46 (§75.522). On the basis of the noun mrym, ‘high place’, the proto-Ugaritic root of the verb RM, ‘to be high’, should be RYM and the stem vowel of the /YQTL/ should be /i/ (T. indicates the root as RY/WM and the stem vowel as /i/ /ā/—though below, on /QTLa/ forms [p. 648, §75.524], he indicates that the root is RYM). Of course, one finds in the other Semitic languages nominal forms from a single hollow root with both /y/ and /w/, or /i/ and /u/, and such may be the case in Ugaritic as well; but mrym does at least provide one datum internal to the language. This means that personal names with the element /yarim/ may be good Ugaritic names, while those with /yarum/ should represent another linguistic tradition
(either a name form from an older parallel tradition that was maintained in usage at Ugarit or a name borne by a person of non-Ugaritic origin, Canaanite for example).1158
— p. 646 (§75.522). It is not clear why \{y\lqn\} (RS 1.009:11 [KTU 1.46])1159 and \{tqn\} (RS 24.253:4 [KTU 1.109]), 3 m.du. forms that appear in the same formula in two texts prescribing a virtually identical ritual, are presented together in the same entry with 3 m.pl. forms with \{t\}-preformative. The plural forms are explicitly parsed but the dual forms are not and the unwary reader could believe that all forms are being presented as plurals. This pair of dual forms provides one of the clearest illustrations of T.’s observation that /YQTL/ dual forms may have either t- or y- as preformative—in contrast with the plural, which has only t-preformative—,1160 and it would have been useful to have the dual forms set apart from the plural forms. Above, p. 438 (§73.223.41.5), T. suggested, as an alternative interpretation, that tqln might be a plural form, but nowhere does he prefer that analysis, and the virtual identity of the two texts makes it, in my estimation, highly unlikely.
— p. 646 (§75.522). T. lists RS 2.002:36 (KTU 1.23) among texts attesting y\ßt, 3 m.s. /YQTL/ of ST, ‘to put’, without noting that the final consonant was omitted by the scribe (the tablet bears \{y\ß\}).
— p. 646 (§75.522). It is not clear why T. parses only y\ßt in RS 24.252:13 (KTU 1.108) as from the verb ST, ‘to put’, taking the other three tokens of this form (ll. 1, 10) and t\ßt (l. 6) as all from STY, ‘to drink’ (p. 662 [§75.532]). This is all the more difficult to understand when one considers that line 13 is badly broken and there is thus no contextual basis for a differentiation in meaning between line 13 and the preceding sections.1161
— p. 646 (§75.522), p. 662 (§75.532). T. gives as his first analysis of \{y\ßt\} in RIH 77/18:15’ (CAT 1.175:13) and of \{[y]ßt\} in line 19’ of the same text a derivation from ST, ‘to put’, only considering STY, ‘to drink’, as an alternative. Two considerations lead me to believe that the priority should be reversed: (1) \{ßty\} appears in line 11’ of this text and, though the context is broken, the explicit \{y\} must be considered as an indicator in favor of the interpretation of y\ßt below by STY; (2) y\ßt b gbh, ‘he shall drink (it) from his cup’ is a far more transparent than ‘he will put it in his body’ (= ‘on his body?’) or ‘he will put it in his cup’ (at the end of the text!). RIH 77/18 appears fairly clearly to be medical in nature and, just as medication was poured through the nostrils of the sick horse according to the hippiatric texts, and just as the person recovering from alcoholic intoxication is to drink the remedy (KTU 24.258:31’ [KTU 1.114]—see remark above to p. 310 [§54.133.1b], etc.) so the sick person may be expected to have been required to drink the medicine. Finally, when on p. 662 T. considers the possibility of deriving y\ßt in RIH 77/18 from STY, he parses it as a /YQTLØ/. Because of the formal parallels between medical texts and omen texts, it is, however, more likely that the application of the remedy was expressed indicatively (viz., the verb in the

1159On the various transcriptions and analyses that T. offers for \{y\lqn\}, see remark above to p. 438 (§73.223.41.5), etc.
1160See above, introduction and note 5.
1161Some scholars have taken the other tokens of y\ßt/tßt as from ST, ‘to put’ (cf. Pardee, Les textes para-mythologiques [1988] 83-84; idem, Ritual and Cult [2002] 194).
omen apodosis is /YQTLu/ rather than /YQTLØ/; {yśt} here appears, therefore, to be best parsed as a contracted /YQTLu/ form from a III-y root (/yištû/ ← /yištayu/). On other cases of {yśt} = /YQTLu/, see remark below to p. 662 (§75.532), etc.; on the vocalization of forms of this type, see remark below to p. 656 (§75.531e).

— p. 646 (§75.522). T. cites RS 16.402:12 (KTU 2.33) as having two certain examples of ást, ‘I shall put’, but the {á} of the first example is restored, and the restoration may not be considered particularly likely.

— p. 646 (§75.522). yṭb in RIH 78/16:6’ (CAT 1.171) cannot be classified with certainty as from the root T(W)B, ‘to return’, as T. does here.1162

— p. 646-47 (§75.523). T. here correctly vocalizes all of the m.s. G-imperatives from hollow roots with a short vowel, e.g. /bin/, ‘understand’.1163 It would have been worth observing that there is no way of determining from the consonantinal script which of these may have been in the extended form, in which case the stem vowel would have remained long, e.g., /bina/.

— p. 647-48 (§75.524). In his listing of /QTLa/ forms of hollow roots, T. indicates the hypothetical proto-West-Semitic form only for the very first: {bāt} is vocalized “bā’at” and said to come from “*bawa’at.” In all vocalizations but one of 2d and 1st person forms, he leaves open the possibility that there may have been a vowel between the verbal stem and the pronominal element: this vowel is always indicated as “(a),” and the reconstruction here differs, therefore, from that indicated in the introduction to the hollow roots, viz. “ā,” and for the worse since /ā/ is far more likely than /a/ in this slot (see remark above to p. 642 [§75.521c]). Finally, in the one case where he does not indicate the possibility of this vowel, he has forgotten to shorten the vowel in the verbal stem: “bāštumû” (‘you two tarry’) would have to have short /a/ in the first syllable (/bāštumª/) for the reason discussed above in the seventh general remark (for the likelihood that the form was in fact /bāšṭumû/, see above, remark to p. 469 [§73.333.3], etc.).

— p. 648 (§75.524). Only in the case of NH, ‘to rest’, does T. consider seriously the possibility that the stem of the hollow-root /QTLa/ in the first- and second-person forms may have had /u/, viz., /nuḥtı/, ‘I have rested’, citing as evidence the Amarna form {nu-uḫ-ti} (EA 147:56). No attempt is made, however, to work the form into his overarching theory of hollow roots, in particular his preference, based on all available Ugaritic data, for the hypothesis that there would have been a vowel between the verbal stem and the pronominal element (see preceding note).

— p. 648 (§75.524), p. 707 (§76.524.2). The comparatively high incidence of participles in the “para-mythological” texts indicates that qî in RS 24.258:21 (KTU 1.114) may be so parsed1164 rather than as /QTLa/.


1163I had not thought through the problem when vocalizing such forms in Les textes para-mythologiques (1988), e.g., p. 183, where “śît” is indicated in lines 6, 7, and 9.

— p. 648 (§75.524). Because the context of qlt in RS 1.032:3 (KTU 2.8) has disappeared, one may not limit the number of possible analyses to 2 m.s. or 1 c.s. of QL, 'to fall', as T. does here. This would be the only occurrence of this verb in letters outside the epistolary formula 'at the feet of X does [the writer] fall', and the absence of context simply makes the analysis, even the identification of the root, uncertain.

— p. 648 (§75.524). With a question mark, T. parses ßtt in RS 17.434+:7 (KTU 2.36) as 2 m.s. from ßT, 'to put'. If, however, the verb is indeed ßT and not ßTY, 'to drink', as others have thought, then the restoration of {m$f$1[mt]}, 'treaty', at the end of the line,\textsuperscript{1165} rather than {m$f$1[rm]}, 'Egypt',\textsuperscript{1166} becomes plausible (T. does not indicate anywhere his reading and interpretation of the whole line). If that be the case, then it is hardly likely that the queen of Ḥatti would be saying that 'you (Niqmaddu) set [X] in the treaty'; far more likely that Puduḫepa is quoting from a previous message from Niqmaddu in which he said that 'you (Puduḫepa) set [X] in the treaty'. Hence the form is more plausibly 2 f.s. (On the difficulty of any parsing of [ßtt] in line 47 of the same text [KTU 2.37:5], see remark above to p. 642 [§75.521c].)

— p. 648 (§75.524). T. parses ßt in RS 2.002:61 (KTU 1.23) as verbal, 3 m.du. /QTLa/ of ßT, 'to put', without mentioning that others have taken the form as nominal, meaning 'lady'.\textsuperscript{1167} On the form of this noun, see remark below to p. 649 (§75.525a).

— p. 648 (§75.524). No question mark is attached to the analysis of ßb in RS 16.402:39 (KTU 2.33) as G-/QTLa/ 3 m.s. of ßB, 'to return', but here the beginning of the line is almost entirely lost, and the identification of the subject is impossible.\textsuperscript{1168}

— p. 649 (§75.525a). If Ugaritic had productive stative verbal adjectives like those of the other Northwest-Semitic languages, it is highly unlikely, for simple paradigmatic reasons, that knm in RS 2.002:54 (KTU 1.23) is a G-active participle, for KN means 'to be' and is, therefore, as stative as can be. The corresponding form in Hebrew is /kén/, listed in the dictionaries as a simple adjective because the G-stem of this root has otherwise fallen out of usage, but corresponding in form to other stative adjectives of hollow roots such as /mêt/ (cf. above, remark to p. 642 [§75.521d], etc.).

— p. 649 (§75.525a). Whether or not the Ugaritic word for 'woman' written {ßt} was originally a G-stem participle from a root ŠWD, it cannot possibly have been vocalized "šâ/ëtt-" as T. proposes here, for the vowel in the first syllable cannot have been long (contrast Arabic sitt-, which T. actually cites).

— p. 649 (§75.525a). T. apparently recognizes that w ßb in RS 1.002:35' (KTU 1.40) cannot be a simple perfective, but, in line with his usual refusal to recognize 2\textsuperscript{d} person forms in the prescriptive ritual texts (see above, remark to p. 211 [§41.12], etc.), he parses the form as an infinitive. On the other hand, he parsed šqrb in line 26 of this text as a m.pl. impv. (p. 595


\textsuperscript{1168}Pardee, \textit{AfO} 31 (1984) 216.
— p. 649 (§74.623). The analysis of {˘ßt} in RS 24.247+:42' (KTU 1.103) as the G-infinitive of ŠT, ‘to put’, as T. indicates here to be a certainty, is far from certain because the partially preserved sign before {št} appears not to be a word-divider.

— p. 649 (§75.527a). T. reconstructs {[…]d˘k} in RIH 77/18:14' (CAT 1.175:12) as {[y]dk} and interprets it as a G-passive in spite of the fact that {dk} is attested in line 4 of this text, where it was interpreted by the editors, correctly in my opinion, as an imperative; the editors extrapolated, again correctly, that reading to line 14'. T. has not noticed that the person to whom the medical instructions are communicated in this text is always addressed directly, i.e., in the second person (see above, remark to p. 605 [§74.632], etc.).

— p. 650 (§75.527c). In my vocalization of RS 24.258:28 (KTU 1.114), I rendered n‘r as /QTLa/ (/na‘ra/), an analysis adopted here by T. Because the verb is in a clause introduced by hn, because it expresses the result of the medical care devoted to ‘Ilu by the goddesses ‘Anatu and ‘Aṭṭartu, and because of the frequent use of participles in these para-mythological texts, I wonder now if the analysis as a participle (/na‘aru/) is not preferable.

— pp. 650-51 (§75.527d-f). In these sections devoted to the L-stem of hollow roots, T. vocalizes all forms but one with /ä/ rather than /ā/, e.g., “yukānin,” ‘he established’ (vKWN); the exception is “mutā(r)āt-” (L-passive participle vTWR). Why did the disappearance of the weak middle radical not produce a contracted vowel in these forms as it did, according to T., everywhere else? (See above, seventh general remark.)

— pp. 651-52 (§75.527g-i). Up to this point in his presentation of hollow roots, T. has been fairly careful in marking what is for him the contracted vowel as short when the vowel appears in a closed syllable. In the presentation of the Š-stem of hollow roots, however, the indicator of contraction/length has been retained in all syllables, closed or open. There are even two cases of so marking the stem vowel in /QTLa/ (“šakînta” and “šaqîlta”),

[§74.623]—see my remark above on this parsing), and it is unclear why he did not continue that line of analysis here. Because of the tight construction of the mirroring masculine-feminine sections of this text, it appears very likely that whatever verb form was used in line 26’ was imitated in line 35'.

— p. 649 (§75.526a). The analysis of {˘-˘ßt} in RS 24.247+:42' (KTU 1.103) as the G-infinitive of ŠT, ‘to put’, as T. indicates here to be a certainty, is far from certain because the partially preserved sign before {št} appears not to be a word-divider.

— p. 649 (§74.511) it is said that the L-stem pattern for hollow roots is “yu1â3i3.”

1169 I have analyzed both forms as m.s. imperatives: Leslau (1991) 1185, 1187; Les textes rituels (2000) 97, 98 (correct /tüh/ here to / tub/ or to /tub/—see above, seventh general remark); Ritual and Cult (2002) 82, 83; cf. Bordreuil and Pardee, Manuel (2004) II 54.

1170 Pardee, Les textes rituels (2000) 544; see the new copy and photograph in Bordreuil and Pardee, Manuel (2004), text 20 in the Choix de textes.

1171 Bordreuil and Caquot, Syria 56 (1979) 296, 297.


1173 Indeed, on p. 577 (§74.511) it is said that the L-stem pattern for hollow roots is “yu1â3i3.”

1174 There is, however, one example of a mistake in the opposite direction: “yVštakinu” (p. 652 [§75.527i]). Judging from the long discussion on pp. 607-8 (§74.642—to which there is no cross-reference on p. 652), where the derivation of this form as Š-stem from vK(W)N or as G-stem from vŠKN is debated, the vocalization with /i/ may be a remnant of the latter analysis, even though the former is preferred.
something that not even the invasion of /i/ into the Hiphil in proto-Hebrew was able to accomplish (the corresponding form is /hiqtaltâ/ in strong roots, /hêqâmâtâ/ in hollow roots).

— p. 652 (§75.528). One may contrast T.’s willingness to parse forms in the most obscure and broken contexts with his unwillingness to prefer a parsing of ykr in the well-preserved context of RS 24.244:62 (KTU 1.100), here said to come from √K(W)R, √KRR, or √NKR.\textsuperscript{1175} It is inconsistent to devote an entire section to this and four other forms when any number of forms that are above parsed more or less precisely are of equally dubious analysis for one reason or another—I have remarked above on the ones that struck me as most dubious.

— p. 652 (§75.529). There are two anomalies in the paradigm entry for the G-stem “Inf[initiv] abs[olut]” of hollow roots: (1) there is no such thing as an “infinitive absolute” in Ugaritic in the same sense as in Hebrew, only an infinitive that appears in the paronomastic figure of speech in which the Hebrew infinitive absolute appears regularly (e.g., /qâтол qâtolî/); (2) the form is vocalized “qûm-” whereas above (p. 649 [§75.526a]) the standard infinitive is vocalized with “â¨” and is derived from the /qatôl/ base, i.e., the standard base of the G-infinitive in Ugaritic—this reconstruction corresponds to Hebrew, where the infinitive construct is /qûm/, the infinitive absolute /qôm/ ← /qâm/ ← /qûtôl/ ← /qatôl/ and it is the latter that appears, of course, in the paronomastic figure of speech (/qôm qâmtî/). That a verbal noun /qûm/- may have existed in Ugaritic is one thing, identifying it as the ‘infinitive absolute’ is quite another.

— pp. 652-53 (§75.529). There is another anomaly in the presentation of L-stem forms in this paradigm: the vowel of the first root syllable is given as “ô/â,” e.g., “yuqô/âmîmu.” As remarked above, /â/ is expected from T. though he indicates /ª/ in §75.527d-f (pp. 650-51). Where the /ô/ comes from here is anybody’s guess since he does not discuss it (he apparently thought at some time that the form should have derived from /qawmama/yuqawmîm-/, i.e., a simple reduplicating pattern /qatlala/, rather than being related to the /qâtala/ stem).

— pp. 653-71 (§75.53). In addition to the general problem of the distribution of III-y and III-w forms mentioned above in the seventh general remark, another problematic aspect of T.’s treatment of III-weak roots may be mentioned here (cf. above, remark to p. 190 [§33.311.5], etc.): he vocalizes /YQTLØ/ in the G-stem and imperative forms in the derived stems with a contracted vowel at the end (e.g., /yâbkî/ ← /yabkiy/ and /hâwwî/ ← /hawwiy/) whereas proto-Hebrew and Arabic had a short vowel in the /YQTLØ/ and Arabic does in the imperative as well. (One may surmise that the /ª/ in the principal verbal stems in Hebrew represents paradigm pressure from the Qal, where the accent was certainly on the final syllable: /bîniy/ → b@nh.) Certainly in the /YQTLØ/ and plausibly in the imperative of the derived stems, where the accent would not have been on the final syllable because one of the preceding syllables was long (e.g., /hâwwî/), I see no reason to doubt that the Ugaritic pattern was similar to that of Arabic. The explanation provided for his decision found on p. 656 (“Da jedoch sichere Anhaltspunkte fehlen, wird die Möglichkeit der Vokalkürzung in der folgenden Vokalisierung der Verbalformen III-w/y nicht berücksichtigt”) is, as nearly as

I can determine, valueless: given that it is difficult to come up with potential sources of internal data which might elucidate the question (certainly not Ugaritic orthography!) and that the West-Semitic comparative data go against the hypothesis, T.’s choice can only be described as showing a preference for theory over data (one would expect /iy/ to give /î/ and the fact that this was manifestly not the case in two of the principal branches of West Semitic is considered inconsequential). In particular, the difference between Hebrew /yʰîîy/ ‘may he live’, and /yēbk/, ‘may he weep’, seems not to have impressed T., for he vocalizes both forms in Ugaritic with an identical final vowel, /î/ (/yahîî/ and /yabîi/). Hebrew /yʰîîy/ must, however, have developed along similar lines as nouns of the type /pʰîîy/, ‘fruit’, viz., through loss of a final short vowel and homorganic shift from /y/ to /î/: /pîryu/ → /pîry/ → /pîrî/ (this stage gave the pausal form /përîy/) → /pʰîîy/—in other words, in the verbal form it is the second root consonant that has created the /î/, not the third (/yahîiy/ → /yahîy/ → /yahî/ → /yʰîî/). There is no reason to doubt that proto-Hebrew also had /yibka/ (from /yabki/ by paradigm collapse on the /yiqtal/ form). An important element of T.’s reconstruction is that he considers that Arabic and Hebrew went through the stage /yaqtî/ on the way to /yaqtî/. That explanation does not, however, account for the forms just discussed and it must be judged more likely that the attested forms represent a simple dropping of the final weak consonant (i.e., /yaqtiy/ → /yaqti/) or perhaps a paradigm based on an historically biconsonantal root (i.e. /yaqti/ ← /yaqti/, not /yaqti/ ← /yaqtiy/). Another indication that proto-West-Semitic never went through the contraction stage comes from what for T. is a conundrum, viz., whether proto-Ugaritic /yîstîy/ went to /yîstê/ or to /yîstâ/ (the latter would be “in Analogie zum Starken Verb” [p. 656]—which, however, does not make a lot of sense because there is no strong-root form with /â/; presumably the analogy is to /yiqtal/, but if so the analogy is poor).1176 Arabic /yaqta/ and Hebrew /yêqt/ demonstrate that the stage of proto-West-Semitic from which these two languages descended had /yaqta/ (the šere in Biblical Hebrew shows proto-Hebrew /yiqtâ/ by the Barth-Ginsberg law; the writing of the 1 c.s. form of this verb as {•ßt-} shows explicitly that Ugaritic had the same historical stem).1177 Neither could have undergone a shortening from /yaqtê/ (← /yaqtiy/) because neither Arabic nor proto-Hebrew had a phoneme /e/. In an e-mail discussion of this point, T. has remarked: “Kurzformen (Präteritum) von Verben III-inf. haben im älteren Akkad. wahrscheinlich einen langen Auslautvokal bessessen. Diese wurden erst später gekürzt”

---

1176 T. does not, however, present the conundrum consistently, for the options are only presented for the root STY; other /yiqtay/ forms are vocalized /yiqtâ/ with or without a question mark (p. 659, BGY; p. 660, HDY) or /yiqtê/ with a question mark (p. 662, PHY—the imperative of this root is given as /pʰîîhê/ without a question mark on p. 663).

1177 It is well known that the singular jussive and preterit forms disappeared from Aramaic. In Old Aramaic, however, the jussive form was still alive, as Degen demonstrated by the use of different matres lectionis in the two forms, he for the indicative, yod for the jussive (GGA 231 [1979] 36-39; on the consistency of this pattern in the Tell Fakhriya inscription, see Pardee, Semitica 48 [1998] 146-47). One must surmise either that this was a rare case of using a mater with a final short vowel, that the historical short vowel had secondarily lengthened, or that proto-Aramaic had developed along the lines proposed by T. for Ugaritic. In any case, the form is no longer attested in the many texts known from the late sixth century on because the jussive had fused with the indicative.
Whatever the case may have been for proto-West Semitic, the agreement between Hebrew and Arabic for the final vowel of such forms having been or being short and the exclusion of an intermediate form /yaqte/ from both languages (the same problem should exist theoretically for /yaqtaw/ → /yaqtô/) constitute impediments to the explanation of the West-Semitic forms by contraction and subsequent shortening of the final syllable. If, however, T.’s view of the early Akkadian data is correct and if a plausible solution can be found for the /yaqtê/yaqtô/ problem, one could posit that a form such as /yaqtî/ could develop to /yaqti/ if accented on the first syllable—this theory would require in turn that the jussive and perfective forms distinguished by accent according to Hetzron’s theory (see note 1118) had fallen together or that the form with final long vowel simply disappeared. Whatever the ultimate solution may be for the problem in proto-West Semitic, I see no reason to posit that the Ugaritic language of the 13th century would have had a long vowel in the final syllable of III-weak /YQTLØ/ forms.

At various points in this section on III-y/w roots, T. mentions “Pleneschreibung” as a possible explanation of forms written with {-y}. There are virtual doublets written with and without {-y} (see remark below to p. 658 [§75.531f]) and an explanation along these lines is certainly to be envisaged; ‘historical writing’, however, might be a more appropriate term, i.e., one would assume that a phonetic shift had occurred in the spoken language but that forms were occasionally still written in an archaic manner. (On the general problem, see remark below to pp. 682-718 [§76].)

T. here cites evidence for III-w roots in Ugaritic, but the quality of the data varies considerably: only two forms written with {w} are cited, while the rest represent T.’s views of diphthong and triphthong contractions, in particular {tdû}, ‘she flies’, is said to come from /tad’uw(u)/ and {t’l}, ‘they ascended’, from /tal’uwū/, i.e., it is assumed that /yaqtul/ forms are built off a III-w root or a III-w variant of a root that is only attested in Ugaritic consonantal writing as III-y—which is the case of both D’Y and ‘LY. This reconstruction appears to be related more closely to Arabic orthographic conventions (where the presence of {w} or {y} may be determined by the preceding vowel, e.g., /yabnuwna/ 3 m.pl. vs. /yabniyna/ 3 f.pl.) than to a full-scale reconstruction of proto-Ugaritic. What is clear from the orthography is that in Ugaritic, as in the other Northwest-Semitic languages, virtually all III-w roots have become III-y, so the system is certainly not identical to the Arabic one, where roots may be classed as III-w or III-y on the basis of the retention of /w/ or /y/ throughout much of the verbal paradigm. What is unclear for Ugaritic is whether there was a systematic use of the imperfective modes in poetry—this description is required because the relationship between writing and phonology is uncertain, i.e., no one has yet come up with a description of the system of usage of {y’n} vs. {y’ny}. That being the case, a systematic reconstruction of the proto-Ugaritic forms that resulted in the forms we encounter in the texts at are disposal is at present out of reach. I see no solid basis on which to determine whether the /yaqtul/ base was still in use with III-weak roots or whether it had already fallen together with /yiqtal/ and /yaqtīl/ as in proto-Hebrew. T.’s principal argument, viz., that the attested forms must come from /yaqtul/ base forms because /yaqtîl/ would give irreducible triphthongs, assumes more about the monophthongization of triphthongs than can in fact be demonstrated. As for the coexistence of III-w and III-y verbal forms of the same
root, viz., that the proto-Ugaritic form of the root was III-\(w\) when preceded by /\(u/\) but III-\(y\) when preceded by /\(a/\) or /\(i/\), T. should demonstrate the plausibility of the existence of such a system in a Northwest-Semitic language before simply assuming its existence in Ugaritic or in proto-Ugaritic. His only positive consonantal data from Ugaritic (i.e., data provided by the presence of \{\(w\)\}) are provided by nominal forms with /\(w/\), viz., \(bnwn\) and \(bnwt\). These exhibit the fluidity of III-weak roots in proto-Ugaritic, something that is clear from all the West-Semitic languages. The principal orthographic argument is the following: though the relatively frequent verbs \(BNY\), ‘to build’, and ‘\(LY\), ‘to ascend’, show various forms with final [-\(y\)], not a single case exists of an imperfective form showing final [-\(y\)] (nine and seventeen tokens, respectively, of /\(YQTL/ forms of the two roots are cited on pp. 659 and 661). It is apparently this fact that has led T. to conclude that these and a few other roots had /\(yaqtuw-/ as their /\(YQTL/ (on the particular cases of \(BNY\) and ‘\(LY\), see remarks above to p. 427 [§73.131], etc.). What would have been needed is a statement regarding the stage of evolution of the Semitic languages at which T. believes a full-blown system of yaqtuw/y-/forms to have existed and the place occupied by Ugaritic in that evolution. T. refers to a III-\(w\) system as “productiver” (p. 654) in Ugaritic, but the data are extremely sparse for so encompassing an hypothesis. To my knowledge, there is only one certain case in Ugaritic of verbal forms from one and the same root showing [\(w\)] and [\(y\)], {\(\text{atwt}\)}, attested once as the 3 f.s. /\(QTLa/ of \(\text{TW/Y} \text{ (RS 2.008}^+ \text{ iv 32 [KTU 1.4]), and \(\text{tity}\), attested twice as the 3 m. pl. /\(YQTL/ of the same root (RS 3.343}^+ \text{ iii 17, 18 [KTU 1.15]).}^{1178} \) The other roots cited by T. would have shown the opposite distribution in the two conjugations, e.g. {‘\(ly\), ‘he ascended’, is attested whereas \(\{y1\}, \text{‘he ascends’, is said to come from */yal‘uw-/}. As regards the orthographic data, it is important to note that the number of imperfective forms where one would expect the third consonant to be written according to T.’s norms of monophthongization if the root were III-\(y\) is much lower than the total of twenty-six tokens that are cited, indeed only three forms in six tokens ([\(t‘ln\]}, ‘they ascend’, attested four times, \{\(n1\]}, ‘we will ascend’, in RS 24.266;33 (KTU 1.119), and \{\(tbnn\]}, ‘they build’, in RS 2.[008]}^+ vi 16).^{1179} It is not impossible that such a small number of forms is owing to the vagaries of monophthongization or of orthographic practice among the scribes. That possibility is proven by the existence of \{\(tśtn\}, ‘they drink’ (twice in RS 24.258 [KTU 1.114], identified by T. as contracted from /\(tiśtay‘īna/[p. 663]) and \{\(tbkn\}, ‘they weep’ (in RS 3.343}^+ v 12, 14 [KTU 1.15], identified on p. 459 [§73.273.5] as contracted from /\(tabkiy‘īna/ but in this section [p. 659, §75.532] as a 2 m.s. form, with no suggestion for identifying who

---

1178The case of {‘\(rwt\) in RS 2.[003]}^+ i 7 (KTU 1.14) and {‘\(ryt\) in RS 18.031:25 (KTU 2.38) is resolvable by epigraphic means. The latter form is quite plausibly a /\(qatîl/ or /\(qatîl/, rather than a finite verbal form (see above, remark to p. 195 [§33.322.2b], etc.) and from the root ‘\(RY\), ‘to be naked’ (said of a ship stripped of its sails). On the other hand, {‘\(rwt\) is almost certainly not the correct reading in RS 2.[003]}^+ i 7 (against KTU/CAT) because the editor was correct in seeing the right tip of the first sign as having the form of a horizontal wedge, not that of a {‘} (the tablet was collated in June of 2003).

1179Another form cited by T. on p. 654 is irrelevant: \{\(t‘l\]} in RS 16.402:37 is probably 3 f.s. (with \(mlākty/ as subject) rather than 3 m.pl.
that singular subject would be). At the end of the day, it appears legitimate to query whether the data are sufficient to posit the existence in Ugaritic of a productive /*yaqtuw-*/ paradigm. One possibility is that the forms adduced in favor of that hypothesis are frozen forms dating back to an earlier period, when full paradigms existed for both /yaqtuw-/ and yaqtuy-/. In this respect, it must be observed that T.’s reconstructions taken to their logical extreme would mean that there was no such thing in proto-West Semitic as roots distinguished by whether they were III-w or III-y, but only III-weak roots with variable /w/ or /y/ that was determined by the preceding vowel. Such an hypothesis goes against the data from Arabic, where III-w roots often exist alongside III-y roots and each has a distinct meaning. Would T. argue that all such cases are inner-Arabic developments and that the Arabic system was absent from proto-West Semitic? An hypothesis just as plausible as T.’s would say that /yaqtuw/y-/ had fallen together with /yaqtiy-/ under the influence of the shift from III-w to III-y in all other forms, i.e., that representative forms of the paradigm were /yabnî/ (3 m.s. jussive ← /yabniy/), /yabnû/ (3 m.s. indicative ← /yabniyu/), /y/tabnû/ (3 m.pl. jussive, a remnant of earlier /y/tabnuw/yû/ or a contracted form of the more recent /y/tabniyû/), and /y/tabnûna/ (3 m.pl. indicative, also either a remnant of earlier /y/tabnuw/yûna/ or a contracted form of the more recent /y/tabniyûna/). Finally, as in so many other details of this grammar, consistency has not been achieved, e.g., yh in RS 2.[012] i 35 (KTU 1.12) is on p. 144 vocalized yaḥḥî but on p. 661 yahḥû. — pp. 653-54 (§75.531b), p. 659 (§75.532). It is highly unlikely that {tbnn} reflects “/tablû-nna/,” for the orthography represents the perception by the scribe of the form as a single word. It must, therefore, represent either /tablunna/ i.e., with loss of length in /u/ and hence of the information which that vowel length carried regarding plurality, /tablun/, also with loss of vowel length, or else /tablûna/. T. has apparently chosen the -nna/ ending because he prefers it over -n/ for unstated reasons (see above, second remark to pp. 497-506 [§73.6]) and because of the homonymy produced by attaching a simple -na/ to verbal forms that end in a long vowel, for that ending would be identical with the indicative marker (/YQTLû/ indicative, /YQTLû/ jussive—see above, seventh remark to pp. 497-506 [§73.6]). As observed above, eighth general remark, the use of the dash in such forms constitutes essentially a refusal to come to terms with the necessity to posit the loss of the marker of plurality in forms of this type—we all know that the dash represents segmentation of the morphemes, but, when vocalizing Ugaritic forms, the Ugaritic vocalization should be given, not a non-existent hybrid located somewhere between the proto-form and the actual reconstructed form. Curiously, T. admits this necessity on p. 654, but only in a non-existent form: “Auf der Basis des Paradigmas III-y wäre eine Schreibung *tbny (= tabniyunna ← *tabniyû-nda) zu erwarten.” Below, in the list of III-w/y /YQTL/ forms (pp. 658-664 [§75.532], {tbnn} is again vocalized “/tablû-nna/” but {iṣṭn} (RS 16.265:16 [KTU 5.9 i 16]) is vocalized correctly “/iṣṭannā” as a derivation from /iṣṭaya + nna/ (cf. also p. 656 [§75.531e]). Consistent representation of the presumed Ugaritic vocalization would be far more intellectually satisfying and far less confusing to the users of the grammar.

1180 T. refers to the “konventionelle Deutung” of the form as 2 m.pl., but offers no explanation for his breaking from convention.
— p. 654 (§75.531b), p. 661 (§75.532). Though my analysis of t‘l in RS 16.402:37 (KTU 2.33) as 3 f.s. jussive with the following word th as subject may have to be abandoned, for there is as yet no proof that th was a feminine noun,1181 T.’s analysis as 3 m.pl. is even less likely, for there is no basis on which to analyze t‘l as 3 m.pl. jussive (“let them arrive in th”). If, therefore, th is not the subject, the only other plausible candidate is mlâkty, ‘my messenger party’ (l. 35). The phrase would mean ‘it (my messenger party) will arrive in a dangerous situation (or whatever meaning be established for th)’.

— p. 655 (§75.531d), pp. 664-65 (§75.534). There can be no certainty that the contracted form of the 3 m.s. /QTLa/ of III-w/y roots showed a long vowel (“/‘alâ/ < *‘alay/wa ”): though this vowel in Aramaic and Arabic is long, Hebrew shows qames, which normally corresponds to /a/ in proto-Hebrew. (The Hebrew 3 f.s. tells us nothing because it reflects a mutation: ‘âlâth consists of the secondary feminine morpheme /â/ ← /-at/ which has been attached to a proto-Hebrew form which would have been similar to the Ugaritic form, viz., /‘ alat/ or /‘ alât/ ← /‘ alayat/.) In this matter of contracted and uncontracted forms (e.g., {‘1} vs. {‘ly}), the possibility must be mentioned that the latter may represent historical writings or consciously archaic forms, viz., that only /‘ ala/ or /‘ alâ/ existed in the spoken language of Ugarit. It is certain that the uncontracted third-person forms do not belong only to the archaic language of poetry, for some are attested in prose; the data do, on the other hand, conform to the theory that they should represent a later stage of the language, for no contracted forms are yet attested in poetry.

— p. 656 (§75.531e), p. 661 (§75.532). As is shown by the jussive form {tûdn} in RS 92.2014:8 (RSO XIV 52), {t‘l} in lines 4 and 6 of the same text may be parsed as jussive as well (rather than as indicative with contraction), in spite of the preceding negative particle l (see above, remarks to p. 514 [§74.222.3], etc., and to p. 612 [§75.212.12]).

— p. 656 (§75.531e). When it comes to reconstructing the /YQTLu/ forms of the III-y /yiqtal/-type in which the root letter /y/ is not present in the writing, T. plugs in his rule for vowel contraction, according to which /ayu/ should go to /û/ (p. 199 [§33.232.2b]). Thus the 2 m.s. form would be identical with the 2 m.pl. jussive/perfective, viz., /tiphû/ ← /tiphayu/ and /tiphayû/ (expressed in terms of the 2d person because of the near absence of y-preformative 3 m.pl. forms). It is clear that Arabic did not follow this pattern, for the corresponding forms, here expressible for 3d person, are /yaqta(y)/ (3 m.s.) and /yaqtaw(≥)/ (3 m.pl. juss. ← /yaqtayû/), and the later Northwest-Semitic languages also eliminated such 3 m.s. forms through paradigm leveling (e.g., Hebrew yišteh). That Ugaritic followed neither of these patterns appears, however, to be indicated by {tlû} in RS 24.244:68 (KTU 1.100), which should be /til‘û/1182 ← /til’ayu/ (3 f.s. indicative of LʿY—unless the final vowel here reflect the fact that this root was III-w in proto-Ugaritic, viz., /til’awu/). By analogy, my vocalization of the G-passive form tp in RS 24.103+:19 (KTU 1.103+) as

1181The phrase p mgy th is now attested in RS 94.2406:14 (see Bourreil and Pardee, Manuel [2004], text 31 in the Choix de textes), but the context is broken; perhaps th is adverbial there, as it may also be in RS 16.402.

/tuppâ/ from */tunpayu/ is perhaps to be abandoned in favor of /tuppû/ (so T. p. 513 [§74.222.2], p. 656 [§75.531e], p. 668 [§75.537a]), my vocalization of ykl in RS 19.015:1 as /yiklâ/ should certainly be changed to /yiklû/ (see remark above to p. 512 [§74.222.2], etc.), and my vocalization of y‘ in RS 24.277:23 (KTU 1.127:30) should be corrected from /ya‘lî/ to /ya‘lû/ (see remark above to p. 126 [§32.146.33a], etc.). If [tlû] does reflect a general pattern, one should perhaps consider that there was, in addition to the rule of contraction to which there are too many exceptions for it to be entirely convincing, a psycholinguistic basis: expression of the verbal mode (in this case the /u/ of the indicative) was important enough to outweigh the disadvantage of homophonous forms.

— p. 656 (§75.531e), p. 663 (§75.532). In the first section cited, T. proposes two analyses and vocalizations of the 3 m.pl. /YQTL/ form tûtn in RS 24.258:3 (KTU 1.114), either as /YQTLu/ (/tiûtna/ ← /tiûtyûna/) or as /YQTLØ/ plus energetic ending (“tiûtûnna” ← “tiûtyûnna”); in the second, only the first of these options is indicated. The latter vocalization is, of course, impossible, since the vowel would have to shorten in the closed syllable. This shortening would produce a form identical with 3 f.s. and 2 m.s. indicative + energetic (according to T.’s system), viz., /tiûtyûnna/, and, moreover, T. would expect the /ayu/ triphthong to contract (pp. 198-99 [§33.323.2], here and on p. 657 a series of exceptions are cited), which would further reduce the form to /tiûtûnna/. If, however, one posits here an energetic ending /-(a)na/, that and the indicative forms would have been identical—hence apparently T.’s avoidance of that possibility (see above, seventh remark to pp. 497-506 [§73.6] and remarks to p. 500 [§73.611.2d] and to pp. 653-54 [§75.531b], etc.). Whatever the upshot of the discussion regarding energetic forms may be, RS 24.258 seems to show a rather consistent use of /YQTLu/ forms, and that appears to be the better solution for tûtn here (the vocalization is thus plausibly /tiûtûnna/ ← /tiûtyûnna/). See also below, remark to p. 662 (§75.532) on yšt in lines 16 and 31’ of this same text.

— p. 657 (§75.531e). The list of verbal roots that do not show monophthongization of /-iyû/ is incomplete, as a perusal of the relevant forms on pp. 658-63 will show.

— p. 657 (§75.531e), p. 663 (§75.532). In his second interpretation of {štûn} in RS 2.[009]+ iv 18 (KTU 1.6) as consisting of a verbal form + energetic ending (instead of šì yn, ‘pour out wine’), T. does not take vowel shortening into consideration for the question of monophthongization, viz., he vocalizes, as is so often the case, by segmentation rather than indicating the final form: “šïyînna.” The penultimate syllable would, of course, have become /in/, and the presence of so many /i/ vowels, one of which according to T.’s hypothesis would have been ultra-short, casts doubt on the plausibility of the reconstruction.

— p. 658 (§75.531f), p. 659 (§75.532), p. 692 (§76.344). As a possible explanation of {tgl} in RS 2.[008]+ iv 23 (KTU 1.4) and RS 2.[009]+ i 34 (KTU 1.6) over against {tgl} in

1185Ibid., p. 715.
1186Idem, Les textes para-mythologiques (1988) 21; Bordreuil and Pardee, Manuel II (2004) 46 (the first token is mistakenly indicated as “tištûnna” but the second is correctly “tistûnna”).
the identical formula in RS 2,[014]+ v 7 (KTU 1.3), all three in Baal-Cycle texts,\textsuperscript{1187} on p. 658 T. uses the term “Pleneschreibung.” His other explanations are: the {-y} would be the enclitic particle or, preferred over the other two, the poet would have purposely chosen the perfective aspect in the one case, the imperfective in the other. On p. 692, he cites first the two cases of {tgly}, then remarks that the imperfective aspect is not obligatory in this expression because {tgł} is also attested. Nowhere does he confront the fact that the parallel verb is in all cases {tbù}, i.e., /tubù’u/, a marked imperfective form, which requires the grammarian to ask whether the sequence perfective-imperfective is plausible for sequential acts in narrative poetry. Is ‘She uncovered the threshold, then she begins entering’ a plausible aspectual interpretation?\textsuperscript{1188} Note that if the explanation of the form {tgly} as a plene writing were adopted, this form would everywhere be perfective. More plausible, it appears to me because contracted /YQTLu/ forms are reasonably well attested, is the analysis of {tgł} as such a contracted form (/tagłū ← /tagliyu/). But that conclusion in turn raises the question: did the uncontracted and contracted forms exist side-by-side in the language or does {tgly} represent historical writing? That question has already been raised with respect to demonstrably different writings of grammatically identical nominal forms (see remark above to p. 51 [§21.341.21a], etc., on {āḥy} vs. {ūḥy}/[īḥy]). On the parallelism of tgl(y) and tbù in the context of the use of III-’ roots in reconstructing the Ugaritic verbal system, see remark above to pp. 620-21 (§75.232); on T.’s attempt to solve the code of the poetic verbal system by aspect theory, see below, remark to pp. 682-718 (§76).

— p. 658 (§75.532), p. 674 (§75.62a). I first noted on p. 658 the use of the abbreviation “PKKi” which is not indicated in the list of abbreviations on p. 917. It apparently stands for ‘short prefix conjugation, preterit function’, i.e. /YQTLØ/. This form and function are normally noted by PKKi, where “i” stands for “indicativische Funktion,” a term which, by the way, goes against traditional usage where ‘indicative’ is used, correctly in my opinion, for the /YQTLu/ form which expresses the imperfective with no modal variation. On p. 674, I remarked for the first time “PKKPi,” which appears to be related to the former (it is placed in contrast with “PKL,” that is, /YQTLu/) but which I am at a loss to explain.

— p. 659 (§75.532). I do not see how T. can be sure that {ybky} in RS 24.282:2’ (KTU 1.129) is 3 m.s. /YQTLu/ in so small a fragment where very little context is preserved. Any possible analysis of this sign sequence must be left open.

— p. 659 (§75.532). T.’s alternative analysis of {ibgy}, ‘I will reveal it’ (four attestations in mythological texts), as N-stem can hardly be considered likely because of the pronominal suffix: transitive N-stem forms, though known, are not commonplace in any of the Northwest-Semitic languages, certainly not in Ugaritic.

\textsuperscript{1187}{tgly} is attested twice more in the same idiom in texts from the ‘Aqhatu cycle.

\textsuperscript{1188}See above, remark to pp. 620-21 (§75.232), where this possibility is broached. T. accepts the existence of a category of imperfective usage for marking inception of action, in particular for the verb NŠ’ when it expresses ‘lifting the voice’ (p. 695 [§76.348]).
Another unregistered abbreviation is found here in describing the /YQTL/ forms of HDY, ‘to incise (one’s flesh in mourning)’: the forms are vocalized as /YQTLu/, which would normally be “PK,” but are here described as: “PLKF.” I have attempted to explain the form as D-stem, factitive, and indicative (hence as a contracted form of /YQTLu/) ‘he will enable you to attain (what you have requested).’ If that analysis be rejected, then a different set of restorations for the passage should be proposed. A similar problem is encountered in RS 23.334:8 (KTU 2.2), where an uncontracted form of the same verb is encountered (‘ymgyk’); T. parses it also as 3 m.s. G-stem, but again the context is damaged and there is no sure way of ascertaining either the verbal stem or the subject of the verb. Above, p. 437 (§73.223.14.14), T. explained the pronominal suffix here as having a datival function; he also recognized implicitly there that the subject, lost at the end of the preceding line, is unknown—that being the case, it cannot even be judged certain that the function of the pronominal suffix here is datival. I once judged that to be the case of the final {y} of ‘ymgyy in RS 16.402:31, but I now believe that T.’s analysis here of that sign as representing the enclitic particle is more likely as is his analysis of the verb as G-stem rather than D-stem.

— p. 660 (§75.532), p. 698 (§76.421a), p. 700 (§76.427). T. accepts my vocalization of {ymg[-]} in RS 24.272:10 (KTU 1.124) as /yamgi/, interpreting it as a /YQTLØ/ form. Because, however, this text is basically in prose, where the /YQTLØ/ perfective was no longer regularly in use, and because {ymgy} appears in line 1 of this text, it may be preferable to read {ymg[y]} (or to see ym as a contracted /YQTLu/ form, not the better solution because all other such forms in this text are written with {-y}). There are two problems here, the actual reading on the tablet (the surface to the right of the signs {ymg} is damaged) and the aspect of the verb. This verb is preceded by direct speech which may contain an example of /waQTLa/ with imperfective function (‘w ſt b bt w pr[í] hy ḥlh, ‘... and put it in the house and it will bring his illness to an end’)—though pr[í]t in this “para-mythological” text is perhaps better parsed as a participle. But, at least as I have interpreted the text, it then refers to the arrival of the messenger (‘w ym[ ]’) and to his reception of the message, expressed as a /QTLa/ form (lqḥ). If this were Biblical Hebrew, w ym would be a paradigmatic example of “wāw-consecutive” + /YQTLØ/: ‘Your messenger has arrived ...

1189 Les textes para-mythologiques (1988) 81, 82, 113. In fact, in my vocalization of line 19’ on p. 81, I erroneously indicated the uncontracted form “yamaggīyuka,” though I did better on the contracted form in line 20’ (“yamaggīka”—by the considerations offered above in the remark to p. 656 [§75.531e], however, the contracted /YQTLu/ form should have been vocalized /yamaggīuka/).


1191 Context III (2002) 106: “Those 2000 horses must arrive here” (the form is probably plural, however, rather than dual, as T. parses it, i.e., it agrees with the real plurality of the number rather than with the dual of the numeral-noun that means ‘1000’).

he has received …’. But that morpho-syntax is not yet well attested in Ugaritc and one must ask oneself whether that is the best interpretation here.

— p. 660 (§75.532). T. parses {ymgy} in RS 24.257:7 (KTU 1.113) as 3 m.s /YQTLu/ form. Because, however, this is the only word preserved in the line and because the literary genre of this part of the text is undetermined, the analysis of the signs may not be limited to this one possibility.

— p. 660 (§75.532), p. 694 (§76.347). Here {ym[gy]} in RS 2.[003]+ iv 34 (KTU 1.14) is parsed either as a 3 m.s. /YQTLu/ form or as /YQTLØ/ with plene writing and {ymgy} in line 47 of the same text is parsed as 3 m.s. /YQTLu/ (without the plene option). I see no reason to believe that in the first case the expression would be perfective, in the second imperfective. On p. 694, both forms are cited as imperfectives following ahr. Though the Kirta cycle presents several problems in the area of verbal morpho-syntax for which no ready solution is available, the possibility may nonetheless be worth considering that the forms in question are in fact the 3 m.pl. short form (/yamgyi/) which would have as subject Kirta and his army—this would also be the case, then, of ylk in line 44. The principal indicator that the forms are indeed singular is that immediately preceding plural forms were written with {t}-preformative ({tškn} in line 29 and {tlkn} in line 31). At least the second of these forms indicates, however, that—unless the {-n} be the enclitic particle—the poet is here using ‘long’ forms not ‘short’ forms. So, unless one be willing to admit that the army’s movements were first expressed by imperfectives with {t}-preformative, then by perfectives with {y}-preformative, it appears necessary to parse all these forms as imperfective, first plural then singular (viz., in agreement with T.’s stance that there are no 3 m.pl. /YQTL/ forms in the Kirta cycle that show {t/- as the prefix). I must confess that I see no convincing explanation from aspect theory for the presence of the imperfective ymgy in these two passages, since the verb is fientive and expresses what I would expect the narrator to have viewed as complete: at the end of a part of (l. 34) or the entirety of (l. 47) his voyage, Kirta ‘arrives at’ a specific place. Is it the fact that the arrival is viewed as only a prelude, in each case, to the important events narrated immediately thereafter? In the first case only the adverb tm is inserted between this clause and the next, in the second this clause is followed by an asyndetic verbal clause. (On T.’s attempt to explain the usage by the preceding particle, see remark below to p. 694.)

— p. 660 (§75.532). Though the passage is damaged, it does not appear particularly likely that ymgy in RS 2.[004] ii 46’ (KTU 1.17) has been correctly parsed here as 3 m.s. What has just been recounted is Dānī’ilu’s wife having safely traversed the months of pregnancy and there is no clear masculine singular subject for the verb—indeed the verb is immediately preceded by yrhm, ‘months’, though that noun may not have been the subject of the verb.

— p. 660 (§75.532). The option of taking {ymgy} in RS 24.252:9 (KTU 1.114) as a perfective with plene writing appears remote at best: all /YQTL/ forms in this text appear to be /YQTLu/, not /YQTLØ/. (On the apparent exception of trpā in line 28’, see above, remark to p. 438 [§73.223.41.4], etc.)

— p. 661 (§75.532). Though T. prudently places a question mark after his analysis of {tmgy} in RS 3.427:5’ (KTU 2.1) as 3 m.pl. /YQTLu/, it must be observed that the context
is so broken that any one formally possible analysis of these signs is just about as plausible as any other.

— p. 661 (§75.532). In the analysis of \{tm\yyn\} in RS 3.322+ ii 40 (KTU 1.19), one encounters another abbreviation not included in the list on p. 917, viz., “PKL p”; this appears after the preferred analysis, “PKL 3.m.du.” Because the form should be dual and because it appears in sequence with a /YQTLØ/dual form (\{tšā\}, ‘they lifted up’), one might expect the alternative analysis to be as /YQTLØ/ with energetic ending or enclitic-[n]. Both analyses would run up against the problem discussed above (seventh remark to pp. 497-506 [§73.6] and remarks to p. 500 [§73.611.2d], to pp. 653-54 [§75.531b], etc., and to p. 656 [§75.531e]) of creating homophonic forms: (1) if the energetic morpheme -(a)nna/ was present, the expression of duality by vowel length would have been lost (/tamgiyā + nna/ → /tamgiyanna/) as it would have been if the energetic ending -(a)n/ was present (/tamgiyā + n/ → /tamgiyan/); (2) the energetic morpheme -(na/ (if there was such a thing), the enclitic morpheme /-na/, and the marker of the long form /-na/ are, of course, formally indistinguishable.

— p. 661 (§75.532). The reading \{y[≤n\} in RS 2.[014]+ v 10 (KTU 1.3) is presented here as a “n[eue] L[esung].” T. first proposed the reading in 1995,1193 I confirmed the proposal in my review of the article in question, 1194 and it was—as is to be expected—present in the transcription of Ugaritic texts collated by Bordreuil and myself that was put at T.’s disposal when this grammar was in its last stages of preparation.

— p. 661 (§75.532). T. proposes two analyses of \{y[≤ny nn\} in RS 24.272:4 and 13 (KTU 1.124): /YQTLu/ or /YQTLØ/. The second analysis would entail the conclusion that the \{y\} is historical writing—all the more so since the vowel would have been short: /ya‘ninnannu/).1195 Since ‘long’ forms are attested in this context and there is no reason to doubt that the function of these forms was imperfective, the former analysis is certainly more plausible. But a third formal possibility is omitted, viz. that the ending corresponds to the Arabic/proto-Hebrew Energete I with the resultant vocalization /ya‘niyannannu/ (/ya‘niyanna + nnu [← an + hu]/).1196 (On the question of whether the energetic forms are independent modes or consist of morphemes attached to the other principal modes, see above, remarks to pp. 497-506 [§73.6].)

— p. 662 (§75.532), p. 691 (§76.343a). On p. 662, yšqynh, ‘he causes him to drink’, in RS 2.[014]+ i 9 (KTU 1.3) is analyzed as either /YQTLØ/ or /YQTLu/ whereas, on p. 691, the parsing as /YQTLu/ is given as certain. (For the similar case of yšlhmnh in line 5, see above, remark to p. 591 [§74.622.3].)

1195T. does not vocalize the form but, by his theories, it would presumably be /ya‘nīnVnnu/, i.e., /ya‘nî + nVn [his Energetic III] + hu/.
“yšrʃ” in RS 18.147:14 (KTU 2.46) should have been qualified as a new reading for KTU/CAT indicate [yšrʃn][…] and my collation confirms this reading (against [yšrʃr]) in the editio princeps.\footnote{Virolleaud, PRU V, text 61.}

— p. 662 (§75.532), p. 698 (§76.421a). yšt in RS 24.258:16 (KTU 1.114) is parsed as a /YQTLØ/ form and vocalized /yištâ/ê/. On this hesitation, see above, remark to p. 656 (§75.531e) on diphthong/triphthong contraction in III-weak roots. On the likelihood that the form in question should be /yištā/ rather than either of T.’s options, see general remark to this section (pp. 653-71 [§75.53]). I also vocalized the form as /YQTLØ/ in Les textes para-mythologiques (p. 21), but the many examples of triphthong contraction cited by T. and the facts that there is not a single provable /YQTLØ/ form in this text and that this very root shows a /YQTLu/ plural form with contraction (tšt, twice in line 3—see above, remark to p. 656 [§75.531e]) lead me to believe now that the form should be vocalized as a /YQTLu/, viz., /yištû/. The same is probably also true of the same form in line 31’ and of yšt from the hollow root ŠT (line 29’), both of which I vocalized as jussives in Les textes para-mythologiques—this new analysis is based on the formal similarity between medical texts and omen texts (see above, remark to p. 646 [§75.522], etc.). Yet another case is tšt in RIH 78/20:7 (CAT 1.169): in Les textes rituels (p. 877) I vocalized both this form and the parallel verb tla’m as jussives, as does T. here, but the writing without {y}, one must conclude from T.’s many examples of contracted long forms, says nothing at all about whether the form is jussive or indicative and, since the context does not dictate the analysis as a jussive, the indicative is to be preferred. This applies equally to yšt in RIH 77/18:15’, 19’ (CAT 1.175:13, 17)—see remark above to p. 646 (§75.522), etc.

— p. 663 (§75.533), p. 685 (§76.322), p. 790 (§83.123b), p. 830 (§89.25a). T.’s treatment of pn in RS 24.258:12 (KTU 1.114) has two major failings: (1) the analysis as a m.s. imperative that has been strengthened into a particle fails to convince; (2) he ignores pn in translating the verse. On p. 663, he parses pn as a m.s. imperative of PNY, “sich wenden, sich abkehren,” then comments “(zu Partikel erstarrt [= "nicht doch!"])”; on p. 790, he cites Garr and Watson as proponents of the analysis of pn as consisting of the conjunction p expanded with -n then refutes them by asserting that “pn in 1.114:12 ist als (erstarrter) Imp. der Wz. √pnny zu deuten (vgl. he. penn 'damit nicht').” In context, pn could not be functioning as a m.s. imperative, because two goddesses are addressed;\footnote{This consideration is the basis Wyatt’s interpretation of pn as meaning “Look!” (addressed to the goddesses): Religious Texts (1998) 409; this interpretation goes back to the editor, who translated « Prenez garde » (Virolleaud, Ugaritica V [1968] 547). Wyatt observes that this interpretation requires that the verb be explained as denominative to pnm ‘face’ because the normal Ugaritic verb for ‘to see’ is PHY. In my estimation, Wyatt is correct in this qualification, for Hebrew PNY, to which Virolleaud appealed and which he translated “être attentif,” means ‘to turn’ and the various passages often cited as supporting a translation ‘to look’ clearly reflect a basic meaning ‘to turn (toward)’.} T. appears, therefore, in spite of his explicit parsing on p. 663, to believe that pn is indeed functioning as a particle in this Ugaritic text—he is not simply providing an etymology for the Hebrew particle. Choosing a verbal etymology that makes appeal to an irregular imperative form (Hebrew pen shows no formal similarity with a m.s. impv. from a III-weak root) as the basis for a

\footnotetext[1197]{}
conjunction when a conjunction exists of which *pn* would be an expanded form does not appear to me to be a plausible solution to the problem posed by the lexical specificity of *pn*, viz., the fact that it expresses a negative purpose clause when the simple conjunction *p* expresses only a strong linkage between two clauses. T. translates the passage twice (pp. 685, 830), treating it both times as though the clause began with the word following *pn*, e.g., p. 685: the text is cited without *pn* and translated “Einem Hund gebt ihr ein Lendenstück …?”. If, however, the syntactic unit begins with the conjunction *pn*, the verbs can only be in the third person, not the second: 1199 “… that they should not prepare for a dog a *nšb*–cut *…”* 1200 — pp. 664–65 (§75.534). T. shows a great deal of hesitation in vocalizing /QTLa/ forms as /qatala/ or /qatila/, and this reflects his summary statement (p. 469 [§73.352]) to the effect that fientive verbs may be /qatala/ or /qatila/; the stem /qatula/, said in the section just cited to be reserved entirely for stative verbs, is proposed only as a second option to explain ‘rwt in RS 2,[003]+ i 7 (KTU 1.14)— a form that probably does not exist (see above, note 1178). As for the hesitation concerning /qatala/ vs. /qatila/, T. was apparently driven to it by the fact that all III- forms show the /qatila/ form and all are transitive (šīl, ‘he asked’, lik, ‘he sent’, and sid, ‘he served (food)’). One may, however, doubt that the inverse is true, viz., that verbs that belong semantically in the stative camp would have been /qatala/ in form, e.g., ḤYY and DWY: the proto-form of Ḥāyya, ‘he lives’, is here reconstructed as /Ḥayi/aya/. I can see no more reason for hesitation regarding the vocalization of the proto-form of this stative verb as /qatila/qatula/ than in the case of DWY, ‘to be ill’ (see above, remark to p. 195 [§33.322.2d], etc.). On the other hand, for the next form listed from ḤYY, viz., the 2 m.s., he presents only the stem /Ḥayiy-/, which is as it should be. (On this form, see also following remark.) — p. 664 (§75.534). T. vocalizes Ḥyt, ‘you (m.s.) live’, as /Ḥayîta/ (from /*Ḥayiyta/) or as /Ḥayyata/, with no explanation. If the vocalization reflects the idea that geminate roots may have had a vowel between the verbal stem and the pronominal element, that vowel must have been long (cf. Hebrew /qallotā/, Akkadian /parsāta/—see remark above to p. 642 [§75.521c]). Given, however, that in the other Northwest-Semitic languages, the root ḤYY behaves not like a geminate root but like a III-weak root (Hebrew /Ḥayītā/ ← /Ḥayyi/ta, not /Ḥayyōta/ ← /Ḥayiyāta/), the first option for reconstruction must be judged the more likely.

1199Nowhere in the grammar does T. parse these verbal forms.

1200Pardee, *Les textes para-mythologiques* (1988) 21, 22, 52-53; idem, *Context* I (1997) 304; idem, *Ritual and Cult* (2002) 169; Bordreuil and Pardee, *Manuel* (2004) II 45, 46. For Wyatt and Virolleaud, who take *pn* as a fully functioning imperative (see note 1198), the following verbs are also in the 2d person (Religious Texts [1998] 409). B. Margalit, on the other hand, takes *pn* as a fully functioning imperative, but the verbs as 3d person (“The Ugaritic Feast of the Drunken Gods: Another Look at RS 24.258 (KTU 1.114),” *Maarav* 2 [1979-80] 65-120, esp. p. 72). Note that, even if one prefers the analysis of *pn* in this passage as a verb (which I do not) and the particle *pn* disappears from the Ugaritic lexicon (for it is only attested here), two observations may still be made with regard to the particle: (a) the origin of Hebrew *pen* remains to be explained and (b) the very existence of the particle in Hebrew may be taken as a sign that the particle may also have existed in Ugaritic.
— p. 664 (§75.534). Given the apparently rather high incidence of participles in the verbal system(s) of several of the “para-mythological” texts, m÷y in RS 24.244:67 (*KTU 1.100*) may be a participle, rather than /QTLa/ as T. classifies it.1201

— p. 664 (§75.534). Though his question mark is certainly apropos, T.’s classification of [m÷y [...] in RH 78/21:10’ (*CAT 2.80*) as 3 m.s. is in fact nothing more than a guess, for the tablet is too fragmentary to allow any inference from the context. The same analysis of the following form cited, [m÷[y]] in RS 11.772+:3 (*KTU 3.1*), has more going for it, viz., the plausible comparison with Akkadian edicts, but here also the text is so fragmentary that a question mark would have been in order.1202

— p. 664 (§75.534). It is not certain that the two tokens of m÷t in RS 17.434+:8, 10 (*KTU 2.36*) are both to be parsed as 2 m.s.: the first may be self reference on the part of the author of the letter.1203

— p. 664 (§75.534). T. parses ‘ly in RS 16.402:25 (*KTU 2.33*) as verbal, either /QTLa/ 3 m.s. or an infinitive; he precedes the entry with a question mark but provides no indication that others have seen in those signs the preposition ‘/ + 1 c.s. pronoun.1204

— p. 664 (§75.534). It is altogether unclear why T. proposes that the 3 m.s. /QTLa/ of PHY would be /pahiya/ when appearing alone, but /pahiya/ or /pahaya/ when followed by a pronominal suffix.

— p. 665 (§75.534). Whatever the precise meaning of m÷t in RS 17.139:30 (*KTU 2.34:32*) may be, it is far from certain that štt in the same line is a form of ŠTY, ‘to drink; ŠT, ‘to put, place, settle’, appears a more plausible candidate (see remark above to p. 266 [§51.45e]).

— pp. 666-67 (§75.536a-b). For a general statement regarding the morphology and morpho-syntax of III-I infinitives, see remark above to pp. 486-87 (§73.523a-b), etc. Here one is at a loss to explain why examples of both [bk] and [bky], verbal nouns from BKY, ‘to weep’, are listed in the two sub-sections cites, which are devoted to /qatâl-/ infinitives and to other forms of verbal nouns. As stated above, since at least one datum showing that the infinitive in the syntagmeme b + infinitive was of the /qatâl-/ type, it borders on the perverse to place some examples of [bk] and [bky] under /qatâl-/ but other examples of [bky] preceded by the preposition under the heading “Verbalsubstantive (außer MphT {qatâl}),” especially when one token of b + bky is located just a few lines from the datum just mentioned. I refer to RS 2.003 i 31-32 (*KTU 1.14*) bm bkyh w yâin // b dm’h nhmmt, ‘As he wept he slept, as he shed tears he slumbered’ (lit., ‘in his weeping he went to sleep, in his shedding tears (there was) slumber’ /bima bakayihu wa yišan bi damâ‘ihu nahamâmatu/), which is followed in lines 37-39 by w yqrb b šâl krt mn krt k ybk, ‘He (‘Ilu) came near, asking Kirta: Who is Kirta that he should weep?’ (lit. ‘he approached in asking Kirta…’ /wa yiqrab bi šâ‘ali kirta mîna kirta kî yabkiyu/). Another example of the same syntagmeme is

1202On the reading, restoration, and historical interpretation of this text, see my article in Semitica 51 (2001) 5-31.
1203In my preliminary study of this text (*AfO* 30 [1983-84] 325), I parsed both forms as does T., but additional reflection has led to the analysis indicated above (Les textes épistolaires, in preparation).
found near the beginning of the next column that shows a contracted form \((b\ bk\ krt\ //\ b\ dm\ 'n'\ mn\ \ddot{g}lm\ il)\), but, as was observed above (remark to pp. 486-87 \([\S 73.523\alpha-\beta]\), etc.), it would have been necessary to prove that the contraction of \(/\ddot{q}atl/\ddot{q}itl/\ddot{q}utl/\) was more likely than that of \(/\ddot{q}at\ddot{a}l/\) to make of the former the preferable reconstruction.

— p. 669 (§75.537d), p. 693 (§76.345). T. parses \(\dddot{y}kly\) in RS 3.367 iv 27' \((KTU\ 1.2)\) as a simple long form \(/\dddot{Y}QTLu/\). If that analysis is correct, the form may express iteration, inception and continuation of action, or simply reflect the non-systematic use of \(/\dddot{Y}QTL\ddot{O}/\) and \(/\dddot{Y}QTLu/\) forms in poetry. One may also consider the possibility, however, that behind this writing is a \(/\dddot{Y}QTLa/\) form expressing an indirect volitive, i.e., ‘with the intention of …’.

The problem is reflective of the general difficulty in determining the precise morphology of a given form or sequence of forms: here there is no way of knowing whether the first two verbs in the sequence are \(/\dddot{Y}QTL\ddot{O}/\) or \(/\dddot{Y}QTLu/\) (and, one might add, there is not even certainty that \(\{\dddot{y}kly\}\) does not represent historical writing of a \(\{\dddot{Y}QTL\ddot{O}\}\) form). The possibility being aired in this remark is that the sequence was \(\dddot{y}qa\ddot{u}t\ …\ \dddot{y}a\ddot{s}it\ …\ \dddot{y}akalliya/\), ‘he grabbed … he dismembered … he intended to finish off’.1205

— p. 669 (§75.537d), p. 686 (§76.323). T. translates \(k\ \ddot{g}z\ \ddot{g}zm\ t\ddot{d}br\ w\ \ddot{g}rn\ t\ddot{t}wy\) in RS 3.325+ vi 31-32 and 43-44 \((KTU\ 1.16)\) as “Wenn Angreifer angreifen, weichst du zurück; und Raüber veranlasst du zum Bleiben/nimmst du gastlich auf” (p. 686), allowing two possible meanings for the D-stem of TWY. Since the accusation is of improper conduct, the stronger of the two interpretations is surely preferable.1206 This fits the semantics of TWY which in the G-stem means ‘to be a guest’ or, transitively, ‘to provide a guest with something’ (see remarks above to p. 110 \([\S 32.144.12b]\), etc., to p. 211 \([\S 41.12]\), etc., and to p. 595 \([\S 74.623]\), and which should be factitive in the D-stem, ‘to take in a person as a guest’.

— p. 669 (§75.537d). On the analysis of \(\{n\ddot{t}'\ y\}\) in RS 1.002:24', 32', 41' \((KTU\ 1.40)\) as 3 m.s. \(N\)-stem \(/QTLa/1207\) rather than as 1 c.pl. \(D\)-stem \(/YQTL/\) as T. parses here, see above, remark to pp. 211-13 \((\S 41.13)\), etc. T. does not consider worthy of mention the alternative interpretation, which has been preferred by various scholars at least since 1949.1208

\[\text{\textsuperscript{1205}}\text{On p. 693, T. classifies this verse as an example of “Inzidensschema,” defined as “eine Gleichzeitkeitsrelation von SVen [Sachverhalten]” (see remark below to this section). My translation reflected an emphasis on inception of action: “Ba’\ddot{u}lu grabs Yammu and sets about dismembering (him), sets about finishing Ruler Naharu off” (\textit{Context I [1997]} 249); cf. Bordreuil and Pardee, \textit{Manuel} (2004) II 10-12: “Ba’\ddot{u}lu se met à traîner Yammu, commence à le dépecer, à achever Chef Naharu” (in agreement with T., all three verbs are vocalized as \(/YQTLu/\) forms). In context, the point of the imperfectives would be that this verse does not describe the complete demise of \textit{Yammu} but the inception of the process, though the nuance of \(\dddot{y}kly\) may be iteration.}\]

\[\text{\textsuperscript{1206}}\text{My gloss “detain,” which was based directly on the Arabic lexicon, and my analysis of the verbal forms as plural (\textit{Context I [1997]} 342) both now appear unfortunate to me (the long form plural of the first verb would have been written with \{-n\}).}\]


\[\text{\textsuperscript{1208}}\text{Bibliography in idem, \textit{Les textes rituels} (2000) 125 nn. 141, 142.}\]
— p. 670 (§75.537d). Why should ykly in RS 24.277:8 (KTU 1.127) be D-passive and mean “vollendet werden” rather than D-active, with the meaning ‘to consume’?  
— p. 670 (§75.537f), p. 694 (§76.347), p. 710 (§76.524.42). On p. 670 tššqy in RS 2.[004] v 29’ (KTU 1.17) is explicitly parsed as /YQTLu/, on p. 694 it is included among examples of /YQTLu/ following the adverb āpnk, whereas on p. 710 it is included among examples of /YQTLØ/ as response to a preceding imperative.  
— p. 671 (§75.538). T. follows KTU in reading tkl in RS 3.427:7’ (KTU 2.1), which he takes as a possible form of KLY, ‘to deplete’. The first sign is, however, probably {à}, as the first editor proposed and as has been accepted by the authors of CAT. (The reading {˘kl} was indicated in the transcription that I made available to both the editors of CAT before that work appeared and to T. before this grammar appeared.) Only the first few signs at the beginnings of eight lines of this text are preserved, however, and no interpretation is possible.  
— p. 671 (§75.539). The representation of the proto-form of the m.s. impv. of the root PHY, ‘to see’, as “phiy” can only be an error for ‘phay’, for here and above the verb is regularly treated as /yiqtal/.  
— p. 672 (§75.61b). Regarding geminate roots, T. states a bit more positively than the data allow that “Im Grundstamm herrscht bei formen mit vokalischer Endung (nach dem dritten Radikal) immer die schwache Bildungsweise vor, z.B. PK 3.m.sg. ysb = /yasubbu/, SK 3.m.sg. sb = /sabba/, SK 3.f.sg. sbt = /sabbat/.” The first two forms are attested as cited, though the analyses and hence the vocalization of the second are not certain (see below), while the third is not attested for the root SBB. As is well known, Hebrew tends to show bi- and trisyllabic third-person forms for active roots where the base was /qalala/, as in sābab, ‘he surrounded’, sāb‘bā, ‘they surrounded’, but mono- and disyllabic forms for stative roots where the base was /qalila/qalula/, as in tam, ‘he is/was mature’ (i.e., /tamima/tamuma/ → /tamma/ → /tamm/ → /tam/ by the standard rules of development from proto-Hebrew to Biblical Hebrew), tāmmū, ‘they were/are mature.’ Only the latter stem-form is used in the second and third persons, where for both types there is a linking vowel between the verbal stem and the pronominal element, as in sabbōṭā and tammōṭā. When one checks T.’s list of /QTLa/ forms, one discovers that the certain third-person forms are either stative or may be passive forms of transitive roots. The latter is very likely the case of rs in RS 2.[003]+ i 12 (KTU 1.14), as most commentators have seen, and may well be the case of sb in RS 2.[008]+ vi 34 (KTU 1.4) as well (on these two forms, see remark below to p. 676 [§75.64]). According to T.’s reconstruction of the G-passive /QTLa/, viz., /qutila/, as in Arabic, the geminate forms might be expected to have behaved as /qalila/ statives did in proto-Hebrew, hence to have become /qulla/ in the G-passive and to be written {ql}. For

1209Bibliographical discussion in ibid., pp. 719-20.
1210Virolleaud, Syria 19 (1938) 343-44.
1211nd in RS 3.362+ iii 16’ (KTU 1.10) and ndt in RS 3.340 i 26 (KTU 1.18) would make T.’s point nicely if his interpretation and, in the second case, the reading were certain. T. takes them, respectively, as 3 m.s. and 3 f.s. /QTLa/ from the root NDD, ‘to flee’, but the texts are too damaged to bear the burden of establishing the morphology of an entire category of verbs.
reasons that are unclear, what might be considered a clear case of a transitive verb which cannot be taken as a passive and which is written with only one token of the second consonant is omitted from T.’s list of /QTLa/ forms. I refer to sl in RS 19.011:6 (KTU 2.61), which T. parses below as /QTLa/ but for which he never proposes an etymology (see remark to p. 702 [§76.521.1], etc).

— p. 673 ([§75.62]). T. confidently analyses both tokens of ybd, ‘he chants’ (RS 2,[014]+ i 18 [KTU 1.3] and RS 2,[004] vi 31’ [KTU 1.17]), as /YQTLu/ forms. The latter parsing is apparently extrapolated from the III- form yṣqynh that appears asyndetically just before ybd, but no such indicator is present in the context of the first example. It is likely that the long forms in RS 2,[004] reflect the fact that they appear in a series of clauses introduced by k, ‘when’, that express the recurring events associated with the annual resurrection of Ba’lu and the promise that ‘Aqhatu may enjoy an immortality like Ba’lu’s. yṣqynh also appears in RS 2,[014]+ i, but several lines away and separated from ybd by III-weak verbal forms that do not show the third radical (tpnh in line 14 and t’n in line 15). The case for ybd here being /YQTLØ/ is thus weaker here.1212

— p. 673 ([§75.62]). Above, p. 659 ([§75.532], T. parsed tbk in RS 3.325+ i 30 (KTU 1.16) as /YQTLØ/ form. Here he analyzes the immediately following form tdm as either /YQTLØ/ or /YQTLa/. Because the verbs express sequentially two future acts of a feminine protagonist that are desired by the speaker (as all recent interpreters have seen), the two are plausibly identical, probably jussives. T.’s openness in admitting the possibility that tdm may be a /YQTLa/ volitive is commendable, but this analysis would imply that tbk is probably the same, something that T. would not wish to admit but that cannot be ruled out, given the irregularity of triphthong contraction as represented in writing.

— p. 673 ([§75.62a]). With no sign of doubt, T. analyzes {y”r} in RS 24.647:4’ (KTU 1.151) as 3 m.s. /YQTL/ from HRR. I can find nothing, however, in the very damaged context to allow for so definite an identification of these signs.

— p. 673 ([§75.62a], pp. 672-73 ([§75.61c], p. 675 ([§§75.63-64), cf. p. 680 ([§75.72a]). These are the sections in which T. assumes the existence of a root MRR which in the G-stem would mean ‘drive out’ (see remarks above to p. 333 [§54.423d], etc., p. 500 [§73.611.2d], etc., and to p. 601 [§74.626.3b], etc.) and another root MRR which in the G-stem would mean ‘strengthen, bless’ (see above, to p. 540 [§74.35], etc.). I once myself accepted this etymology of the forms ámr and mr, the latter form associated with the name áymr, in the mythological text RS 3.367 iv 2’, 19’ (KTU 1.2).1213 But further study has convinced me that this interpretation does not square with the Arabic verb usually cited as etymological support, for Arabic marra is an intransitive verb of movement. If that root is represented in Ugaritic,

1212I once vocalized all /YQTL/ forms in this passage as /YQTLØ/ (Trial Cut [1988] 2), but that was more out of desperation than based on a comprehensive theory of the verbal system in Ugaritic poetry. I also vocalized yṣqynh as /yaṣqiyannahu/, viz., as Energic I; given the uncertainties surrounding the forms and functions of the energetic forms in Ugaritic (see above, remarks to pp. 497-506 [§73.6]), one must consider the possibility that the {y} in that form is present owing to the linking of energetic form and pronominal suffix rather than being a /YQTLu/ indicative.

it is in the reduplicated form MRMR attested in RS 92.2014:2 (RSO XIV 52), which was discussed above in a remark to p. 333 ([§54.423d], etc.). On the other hand, Arabic does show a III-y root that functions transitively, and the forms in RS 3.367 could just as well be derived from such a root as from a geminate root:

\[
\begin{align*}
(19') & \text{šmk} . \text{át} . \text{āymr} \quad \text{You, your name is 'Ayamiri;} & \text{šumuka 'atta 'āyamiri} \\
\text{āymr} . \text{mr} . \text{ym} \quad & \text{O 'Ayamiri, expel Yammu,} & \text{'āyamiri miri yamma} \\
\text{mr} . \text{ym} (20') & \text{lxsih} \quad \text{expel Yammu from his throne,} & \text{miri yamma lē kussa 'ihu} \\
\text{nhr l khṭ} . \text{drkth} \quad & \text{Naharu from his sovereign seat.} & \text{nahara lē kaḥtē darkatihu}
\end{align*}
\]

The forms containing MR(R) that have caused so much controversy are thus plausibly explained as follows:

MRY ‘to drive out’ (in the passages just mentioned);\(^{1214}\)
MRR (*MYR) ‘to bless (concretely)’ (see above, remark to p. 540 ([§74.35], etc.);
MRR ‘to be bitter’; SMRR ‘bitterness, venom’, the latter derived from the unattested finite verbal form SMRR ‘to cause bitterness’ (see remark above to p. 601 ([§74.626.3b], etc.).
MRR ‘to pass (intransitively)’, attested in the factitive form MRMR ‘to make move back and forth’ (see remark above to p. 333 ([§54.423d], etc.).

— p. 674 (§75.62a). T. derives \(tśr\) in RS 2.[003]+ iii 29 and vi 10 (\(KTU\ 1.14\)) from the root \(ŠRR\), which he translates “einschließen, belagern.” Comparing the Ugaritic datum with other West-Semitic languages, most commentators would say that the geminate root would have had the less military connotations of ‘harass, vex’, that it was the hollow root that would have had the specific connotation of ‘besieging’.\(^{1215}\)

— p. 675 (§75.63). Though the case of \(ġTT\), ‘to be desolate, in a shattered state’, is more ambiguous than that of \(ḤNN\) (see remark above to p. 220 ([§41.221.2], etc.), one must nevertheless reject T.’s vocalization of the imperative form \(ḥt\) as /ḥutt-/ . The structural reason here is the opposite of the case of \(ḤNN\): the latter verb is transitive, but \(ḤTT\) is stative. The imperfect paradigm in Biblical Hebrew reflects this fact with no exceptions, for the base is always /\*yiḥatt-/ . Here it is the imperative that creates the ambiguity, for it is only attested in one verse of the Hebrew Bible (Isa. 8:9), but there it is repeated three times, each time vocalized by the Massoretes as /ḥōttūw/ (i.e., with ḥolem as the first vowel). Given the semantics of the root and the unambiguous testimony of the imperfect forms, this is in all likelihood to be understood as an error for /ḥāttūw/ (i.e., the first vowel should have been qames according to Massoretic norms—historical /a/ in a closed accented syllable can become /ā/). Whatever the proper explanation of the Hebrew form may be, unless T. wishes to propound a theory explaining why some geminate roots would have had different stem vowels in the G-imperative and /YQTL/, thereby going against the standard pattern in Semitic, he must reconstruct the stative form in Ugaritic with /a/ rather than /u/.

— p. 675 (§75.64). With no sign of doubt, T. parses \{[…]ḥṭṭ\} in RS 3.367 iv 1' (\(KTU\ 1.2\)) as 3 f.s. or 1 c.s. G-stem /QTLa/ of the root ḤTT, ‘to be desolate, in a shattered state’. As we have seen to be the case, however, with the following signs (\{mṭt[…]\)—see remark above

\(^{1214}\)Obermann, \(JAOS\ 67\) (1947) 203, n. 31; Smith, \(Baal Cycle\) (1994) 343.

\(^{1215}\)E.g., Wyatt, \(Religious Texts\) (1998) 195 note 92.
to p. 642 [§75.521c, etc.] the break means that any specific analysis is nothing but a guess. No indication is provided for why the preferred analysis of this form is 3 f.s. whereas the only analysis offered for the other was 1 c.s.

— p. 676 (§75.64), p. 713 (§76.525). In my general remark above on the reconstruction of the /QTLa/ of geminate roots, two forms were mentioned as constituting possible examples of 3 m.s. active forms of geminate roots that show the /qalla/ pattern rather than /qalala/, sb in RS 2.[008]+ vi 34 (KTU 1.4) and rš in RS 2.[003]+ i 12 (KTU 1.14). T.’s interpretation of the latter is highly novel in the context of Ugaritic scholarship and may not be considered literally acceptable. The text reads krt ḥtkn rš // krt grdš mknt. T. translates “Keret hatte die Herrschaft zerschlagen; Keret hatte die Machtstellung zerbrochen” (p. 676) and “Keret hatte (selbst) die Herrschaft zerschlagen, Keret hatte (selbst) die Stellung zerbrochen” (p. 713), taking ḥtkn and mknt as direct objects of the two verbs. On p. 676, he comments ”d.h. Keret hat selbst sein Königtum verspielt” and this view of things is worked into the translation on p. 713. Analyzing the verbs as passive, the two nouns as adverbial accusatives, and Kirta as subject, however, is just as plausible and avoids the idea that Kirta had gone about destroying his own household, a notion that is nowhere else expressed in the text, indeed that is belied by the following account of how his attempts to gain offspring were thwarted by other agencies. The literal translation would be: ‘Kirta, as regards the family, he was crushed // Kirta was destroyed, as regards the home’.1216 As for sb, T. claims on p. 534 (§74.32) that it is stative in the passage cited (sb ksp l rqm // ḫṛṣ nsb l lbnt, “Das Silber war zu Platten geworden; das Gold hatte sich zu Ziegeln verwandelt”—identical translation p. 713). If such be the case, one must assume either that Ugaritic, like Arabic, could express a single verbal root as active /qatala/ and as stative /qatila/ or that the root SBB showed a semantic makeup in Ugaritic that was different from that of the same root in Hebrew, where the verb belongs to the class of active verbs that express both a form of movement and transitivity with regard to where the movement is exercised. Thus SBB means both ‘to perform a circuit’ and transitively ‘to go around (an entity)’;1217 and ‘BR means both ‘to perform a passage’ and transitively ‘to pass through (an entity)’; of these two examples, both take complements marked by the definite direct object marker ’et in the G-stem and the N-stem is attested for both, the function of the latter being either middle or passive. In the Ugaritic passage in question, the G-stem form could be active (“the silver turned to plaques’) or it could be passive (“the silver was turned into plaques’) while the N-stem nsb could be either middle (“the silver became bricks’) or also passive. This single form sb is not, therefore, sufficient to demonstrate that proto-Ugaritic /qalala/ forms had become /qalla/ in Ugaritic. That conclusion or its refutation must await the discovery of unambiguous data (on

1216Most translators have avoided so literal a translation, including myself: “Kirta—(his) family was crushed, Kirta—(his) home was destroyed” (Context I [1997] 333; cf. Bordreuil and Pardee, Manuel [2004] II 19). But T.’s way of dealing with the absence of gender concord of grdš with mknt is not the only one available.

1217One may conclude that the semantic range of the verb was similar in Ugaritic on the basis of sentences like ysb pált bṣql yph b pált, ‘he went around his cracked fields, he saw something green in the cracked fields’ (RS 3.322+ ii 12-13 [KTU 1.19]), where SBB + pált is zero marked whereas PHY + pált requires the preposition b.
the problem posed by šl in RS 19.011:6 [KTU 2.61], see remark below to p. 702 [§76.521.1], etc.).

— p. 676 (§75.64). T. parses 'z in RS 3.367 iv 17' (KTU 1.2) as a certain example of the G-stem /QTLa/. Because all the other narrative verb forms in this passage are /YQTLØ/ perfectives, with no example of the /QTLa/-perfective, it appears just as likely that 'z was the stative verbal adjective, in this context 'azzu rather than /azzal/. The form is followed immediately by one of the /YQTLØ/ forms ('z ym l ymk, 'Yammu is strong, he does not collapse'), and the use of the verbal adjective, which bears no aspect marking, in such a sequence might have been considered more appropriate to express Yammu's power than a form marked for perfectivity. Judging from T.'s grouping this passage together with a similar sequence from Ba'lu's battle with Môtu (RS 2.[009] vi 17-20 [KTU 1.6]), his parsing of the form in RS 3.367 appears to have been based in part on the fact that the culmination of the other battle is expressed by the phrase mt ql b'l ql, 'Môtu falls, Ba'lu falls', structurally parallel to 'z mt 'z b'l, 'Môtu is strong, Ba'lu is strong', repeated three times in the preceding lines. That parallelism does not appear, however, to be a sufficient criterion, for ql is a verb of movement, hence fientive rather than stative.

— p. 676 (§75.65). No stative verbal adjective is listed under “Formen des G-Partizips” of geminate roots. This is in keeping with T.'s standard practice of not classifying this adjective with the other verbal adjectives (see above, remark to pp. 471-77 [§§73.4-73.427]). In the case of the geminate roots, however, it would have been essential to separate the stative verbal adjectives from the active and passive ones, because they were written differently: /gāzizu/, 'shearer', /baruru/, 'purified', but /rabbu/, 'great', ← /*rabibu/ or /*rabubu/).1220 Above, at /qall/ base substantive from geminate roots (p. 253 [§51.41a]), T. lists only nouns, no adjectives; at /qatil/ and /qatul/ base substantives (pp. 258-59 [§51.42c,d]), he lists both nouns and adjectives but no forms derived from geminate roots. Thus what was almost certainly an important grammatical category (/qall/ stative adjectives are common across the board in the Semitic languages and, as stated in the remark to pp. 471-77, /qatil/qatul/ stative adjectives were apparently productive in proto-Northwest Semitic) has slipped entirely through T.'s fingers. This blind spot appears also to have been instrumental in leading T. to categorize certain /qall-/ forms as /QTLa/ when they may in fact have been verbal adjectives (see preceding remarks to various forms that T. has identified as /QTLa/).

— p. 679 (§75.7). T.'s argument that the transformation of bi- or triconsonantal roots into quadriconsonantal stems by reduplication of one of the radicals was no longer productive is

1218The last certain /QTLa/ form in the narrative was in line 6, in the formulaic phrase [b] ph rgm l yṣā, 'the word had hardly left his mouth'.


1220The analysis of zb, 'it foams', in RS 92.2014:1 (RSO XIV 52) as a stative verbal adjective (/zabbu/) might be preferable to the analysis as a /QTLa/ form that I proposed in my preliminary edition of this text (Les textes rituels [2000] 830-31); the adjectival interpretation is preferred in Bordreuil and Pardee, ibid., p. 69. This analysis avoids seeing the particle ū that precedes this verb as having the 'consecutive' function that I proposed in my preliminary study (on the unlikelihood that this particle has the meaning 'Woe!', see above, note 741).
based solely on the rarity of the patterns in Ugaritic. Because such forms are attested in all the West-Semitic languages and because the etymological underpinnings of many forms are relatively clear, it appears improper to argue directly from rarity to non-productivity and from there to lexicalization of such forms as quadriconsonantals ("Verben mit vier Radikalena").

His two other arguments are of no more value, viz., that the reduplication patterns "keinem einheitlichen Muster folgen" and that the underlying roots are not attested in Ugaritic. Taking the second argument to its logical conclusion would mean that any Ugaritic verbal form for which the G-stem and derived nouns are not attested must be classified as having lexicalized in Ugaritic in whatever form is attested. Cross-Semitically, the various reduplication patterns do not follow simple or unified patterns; in Biblical Hebrew alone, where the corpus is small but larger than that of Ugaritic, one finds a similar spread of forms derived from both strong and weak roots. And T.'s claim that none of the cited forms is attested as a "Grundwurzel" in Ugaritic is directly related to his perception of what constitutes a root: he admits that q®q® and mrmr are related to what are probably geminate roots in Ugaritic, but what he wants is the 'original' biconsonantal 'root'—too much to ask for judging from the corresponding Arabic forms (/QLQL/ forms are often derived from geminate roots in Arabic lexicography). He sees one case of a form /QLQL/ that would be related to a hollow root (see second following remark). One could perhaps argue for Hebrew as well that the corresponding forms were no longer productive, but moving them on that basis from the category of derived verbal stems to that of quadriconsonantal roots borders on the nonsensical. Does the relative rarity of occurrence of these verbal forms make them so different from, say, nouns produced by prefixing of /m-/ (most of which go back to some proto-stage of the language in question, many clearly to proto-Semitic), that we must classify the former as quadriconsonantal but the latter as bi- or triconsonantal with a prefixed morpheme?

— p. 680 (§75.72a). T. proposes that the vowel of the prefixed syllable in the /YQTL/ of the reduplicated stems would be /u/ "analog D/Š-PK." If, however, the vowel was not /u/ in this syllable in either the D-stem or the Š-stem, and such was certainly not the case of the 1 c.s. (see above, general remark on vocalization and remarks to pp. 544-46 [§74.412.1-16] and to pp. 587-88 [§74.622.1]), then there is no analogy.

— p. 680 (§75.72a). T. takes the signs {tgrgr} in RS 2.002:66 (KTU 1.23) as a single word, viz., the verbal 'root' GRGR which would be comparable ("vgl.") to the common Semitic root G(W)R, 'to dwell (somewhere as an alien)'. Neither this derivation nor T.'s interpretation thereof ("als Fremde(r) wohnen," viz. as an intensive of the G-stem) is implausible, but he might have mentioned the possibility of dividing the signs into two forms of the G-stem, the jussive followed by the imperative:1221

| ℓm . tgr | There you must dwell as resident aliens, | ūamma tagūrû |
| gr . ʾābnûn . | dwell among the rocks, | gūrû lē ‘abanîna |
| w l . ʾšm . | and among the trees. | wa lē ‘išîma |

1221It is fair to say that the use of the word-divider in this text does not show sufficient consistency to give any weight to its absence here (i.e., after {tgr} according to the lexical/grammatical division that I am proposing) for the correct division of the signs.
This is opposed to two other possible poetic divisions, viz., as a bicolon
\[ \text{tm} \ . \ tgrgr \ . \ l \ \bar{a}bnm \ . \ // \ \text{w l} \ . \ \text{šm} \]
or as a tricolon
\[ \text{tm} \ . \ tgrgr \ . \ // \ l \ \bar{a}bnm \ . \ // \ \text{w l} \ . \ \text{šm} \]
— p. 680 (§75.73). T. takes šhrrt in RS 2.002:41, 45, 48 (KTU 1.23) as intransitive, which now appears to me to be correct (see remark above to p. 584 [§74.522b], etc., in particular note 1056).
— p. 681 (§75.74b). On the necessity of taking grds in RS 2.[003]+ i 11, 23 (KTU 1.14) as passive, rather than active as T. parses it here, see remark above to p. 676 (§75.64) on rš // grds in this passage.
— p. 681 (§75.75). It is unclear to me why T. classifies prš in RS 2.[008]+ i 35’ (KTU 1.4) with “Vierradikalige Verben anderer Struktur” for he only presents two options of interpretation: (1) the root is PRŠ’, in which case the form belongs to §75.74, quadriconsonantals with /r/l/ as second consonant; (2) the root is PRŠ and the { â } is a mater lectionis for the final /-a/ of the 3 m.s. /QTLa/ form (on this analysis, see T., p. 51 [§21.341.12]).
— pp. 682-718 (§76). T. has done more in this section to come to terms with the Ugaritic verbal system(s) than has anyone before and any criticisms voiced here and below must be taken in that context. Most basically, I agree fully with T.’s basic position that the Ugaritic verbal system was aspectual in nature and believe that this approach to explaining the variety of forms that one encounters is the only one that has any chance of succeeding. First, one relatively minor qualification: I believe strongly that it is necessary to see the marking of aspect as limited to the finite forms. On p. 718 (§76.6), T. observes that “Bei nominalen Kategorien … läßt sich keine einheitliche aspektuell-temporale Funktion feststellen,” but then he goes on to claim that the G-stem active participle “ist imperfectiv ausgerichtet,” while the G-stem /qatal/- infinitive “ist demgegenüber offenbar perfektiv ausgerichtet.” Such formulations are, to the extent that they correspond at all to linguistic reality, open to misunderstanding and it is preferable to say simply that all verbal adjectives and nouns of all the verbal stems were unmarked for aspect and any aspectual or temporal translation value is derived from context.1222 One major criticism of the introductory sections (pp. 682-84

1222 Pardee, Semitic Languages (1997) 138; idem, Encyclopedia of the World’s Ancient Languages (2004) 303-4, 307-8. I also believe that there is a basis for disagreeing with T.’s derivation, in his study of the Biblical Hebrew verbal system, of the perfective-aspect form /QTLa/ from a proto-Semitic /qatil/qatul/qatal/ perfective form (best known in Akkadian, where the predominate form was /qatil/— see Tropper, ZAH 11 [1998] 153-90). It appears to me that a case could be made for the Akkadian, and hence the proto-Semitic, form being unmarked for both aspect and tense, that is, as an adjectival form, it did not express aspect. Thus, in Akkadian, the attachment of pronominal elements to adjectives and nouns (maršāku, ‘I am sick’ = šarrāku, ‘I am king’) was only the first step towards what became in West Semitic one of only two forms expressive of aspect. The fundamental difference between the two systems is revealed by the fact that the pronominal elements could no longer be attached to nouns in West Semitic. If this perspective be admitted, one could argue that, as the Hebrew /wayyiqtol/ form retains an old perfective function from proto-West-Semitic /YQTLØ/, so /wqatal/ retains an old proto-West-Semitic non-aspectual function of /QTLa/ that has secondarily taken on an imperfective function by grammaticalization in Hebrew. T. D. Anderson’s approach along similar lines (“The Evolution of the Hebrew Verbal System,” ZAH 13 [2000] 1-66) is far more sophisticated than either T.’s or mine, for he proposes tense/aspectual marking for all forms at all periods. I
and of the following presentation of the imperfective and the perfective aspects is that T. makes no explicit statement here in his introduction to the effect that the verbal systems of prose and poetry differed in one major respect: there is not a single certain instance of the use of the /YQTLØ/ form to express perfective aspect in prose (though there may be some rare cases of that pattern when preceded by w, viz., what became the systematic ‘wāw-consecutive’ form of Biblical Hebrew). Thus the language of every-day expression at Ugarit in the thirteenth century had already evolved from an early West-Semitic one in which /YQTLØ/ was regularly used to express acts viewed as complete and to express the jussive and in which /QTLa/ functioned, as in Akkadian, as an adjective marked for person, to one where imperfectivity was marked by /YQTLu/, perfectivity by /QTLa/ whereas /YQTLØ/ was reserved primarily for the expression of volitivity. This being the case, T.’s valiant effort at providing a linguistically valid explanation for the distribution of the /YQTLØ/ and /YQTLu/ forms (see following remark) is in fact an attempt to deal with the apparent inconsistent use of these forms in poetry—there are virtually no such problems in prose. Anyone who knows the texts will pick this fact up from the texts that are cited; but a grammar such as this will be consulted by people who do not know the texts and who may not know that “[KTU/CAT] 1.23” and “[KTU/CAT] 1.114,” for example, are poetic, while “[KTU/CAT] 1.41” and “[KTU/CAT] 1.116,” for example, are in prose. T. does not make clear his views on the absence of /YQTLØ/ perfectives in prose until the introduction to §76.4 and nowhere does he outline his views on the verbal system in use in prose.1224 Surely it is not enough in a grammar with the pretensions to theoretical and empirical exhaustiveness of this one just to say that one of the major forms visible in poetry is not present in prose. Another major criticism: in §76.13 (pp. 682-83), T. points out correctly that verbal systems are not usually marked only for tense or only for aspect, but express both in varying ways. He never addresses, however, the issue of the simplicity of the archaic West-Semitic verbal systems, viz., the fact that they have only two finite forms. That being the case, they do not have the luxury visible in the Slavic languages, in Greek, or even in Akkadian or in Syriac (in its secondary development of a verbal system expressive

wonder, however, if considering that the proto-West-Semitic nominal forms were not specifically marked for either tense or aspect is not a better solution.

Another criticism would apply to the following section (§77), which treats the “Modalsystem.” Because, as T. himself recognizes (p. 719 [§77.21]), the expression of mode is limited in the West-Semitic languages to the /YQTL/ system, the modes should be presented as a sub-set of the aspectual system, rather than as a separate category (cf. Pardee, Semitic Languages, §2.6.5; idem, Encyclopedia of the World's Ancient Languages (2004) 304-5).

On p. 696, T. does claim that if narrative prose were attested in Ugaritic, we might find that the /YQTLØ/ form was used more extensively in prose than we now know to be the case. He cites as a basis for this hypothesis, the prevalence of /wayYQTLØ/ in Hebrew narrative prose. One must object that the form is also common in Hebrew direct speech, as attested both in the Bible and in the extra-biblical inscriptions in the Judaean dialect, whereas such forms are extremely rare in Ugaritic prose, if attested at all. /wayYQTLØ/ clearly belongs to the verbal system used in Hebrew prose and is not genre-dependent (or, more precisely, it was used in both strata of the language but its use was systematized in prose in a way that is more easily identifiable than is the poetic usage) and one might expect the non-use of /YQTLØ/ in Ugaritic prose also to have been systematic.
of tense), where a more complicated verbal system permits certain forms to specialize in the expression of aspect, others in the expression of tense. From the perspective of grammatical marking, it appears necessary to hold that the two finite forms of the archaic West-Semitic verbal systems express aspect not tense; the latter is not in the purview of the verbal system and is only expressed to the extent that real-world temporal relationships fit the aspectual system. As we shall see in the next remark, T. organizes his presentation in terms of how the system expresses tense, when he might have done better to organize it in terms of what the aspectual system was expressing, i.e., the various categories of ‘completion’ and ‘incompletion’.

— pp. 684-701 (§§76.3-4). The following types of explanations for the apparently inconsistent uses of /YQTLu/ vs. /YQTLØ/ may be evoked: (1) aspectual variation that appears non-intuitive to the modern reader; (2) ‘historical writing’ (III-y roots, viz. /y‘ny/ = either /ya‘niyu/, ‘he answers’, or /ya‘ni/, ‘he answered’); (3) enclitic-{y} (III-y roots, viz., /y‘ny/ = either /ya‘niyu/, ‘he answers’, or /ya‘ni + ya/, ‘he answered’); (4) enclitic-{n} (2 f.s., duals, and plurals, viz. /tqtln/, for example, = either /taqtulîna/, ‘they answer’, or /taqtulî + na/, ‘they answered’), or, most radically; (5) there was no true ‘system’ in narrative poetry, that is, the proto-Ugaritic verbal system in which /YQTLØ/ and /YQTLu/ expressed opposite aspects was no longer understood and the forms were used ‘stylistically’ rather than according to a grammatical ‘system’ in poetry. Attenuated forms of this last hypothesis would say (a) that the Ugaritic poets knew the archaic dialect and played on the forms ‘stylistically’ (T. sometimes resorts to this mode of explanation—see remark below to p. 689 [§76.342]) or (b) they knew the dialect but made an occasional error or (c) they no longer knew the archaic dialect but had passed the poems down orally from generation to generation and an occasional ‘incorrect’ form had slipped into the tradition.1225 (It should be noted that, unless one be willing to admit the existence of quiescence and accompanying use of /º/ as a mater lectionis, the explanation by historical writings will not solve the problems of III-y roots, viz., /yšû/ in RS 2,[003]* ii 46 [KTU 1.14] vs. /yšî/ in the following line.) As regards T.’s general approach, it should be noted that he attempts in these sections to classify the verbal forms in poetry, whether part of the narrative or in direct speech embedded in the narrative, according to whether a given form in fact occurs in a real-world time frame of ‘present/future’ (‘Gegenwart’/‘Zukunft’) or ‘past’ (‘Vergangenheit’). Four comments/questions are in order: (1) one encounters many fewer problems in direct speech than in the surrounding narrative; (2) Why were temporal categories chosen as the organizing principle for a language

1225Cf. Pardee, Encyclopedia of the World’s Ancient Languages (2004) 303. Such permutations in poetry are, of course, linked to the question of oral poetry in antiquity: did the ancient poem reinvent the poem every time it was told (in which case he had to ‘know’ the dialect) or did he only repeat a poem learned by heart (in which case he would not need to ‘know’ the dialect and his own language might interfere with that of the poem learned by heart)? An analogy for the first situation may be found in modern American religious circles: in conservative denominations where the King James version is venerated, some members are capable of extemporaneously producing long prayers in which the conventions of seventeenth-century English are reproduced reasonably well. Most such persons will have had no formal training in that dialect of English and will have learned to pray in it by imitating their predecessors and from long exposure to the King James version itself.
admitted to be primarily aspeccal?; (3) Are temporal categories appropriate for reflecting the linguistic structure of poetic narrative?; (4) It is clear from several of the examples cited below that by “Gegenwart” T. is referring to the German present tense as a translational representation of Ugaritic forms, not as a true tense opposed to ‘past’ and ‘future’: this becomes obvious from his classification of some such forms as ‘gnomic’ or from his inclusion of the prose example il mshmr dt tgm npš špš mlk, ‘the gods of Egypt who guard the life of the Sun, the king’ (RS 16.078:22-23 [KTU 2.23])—they do not do this presently but permanently. Some of these questions/problems could have been answered/solved by rigorously separating prose from poetry and, in poetry, direct speech from narrative. The basic presentation might have better served the Ugaritic language by being organized according to aspeccal expression rather than to temporal categories, which function here as much as a translational device as one for grammatical categorization. (I noticed more categorization by translational categories in these sections than anywhere to this point in the grammar, e.g., §77.5 “Die modalen Nuancen ‘müssen’, ‘dürfen’ and ‘können’”—it is well known from Hebrew, for example, that the ‘indicative imperfect’ can be used to express a weaker form of volition than the imperative or the jussive and this usage should have been separated out from others that may have similar translation values.1227)

— p. 685 (§76.321). T. cites arî tzg l ʿghl bn hpt l ʿwm ʿln km k tn dpmsg (RS 3.343+ i 5'-7' [KTU 1.15]) as his very first example of the “PKL für Sachverhalt der Gegenwart,” which in this case is said to be “PKL in sprichworhafter [gnomischer] Funktion,” but he does not say to which of the two verbs in the passage the analysis applies. He translates “(Wie) eine Kuh nach ihrem Kalb ruft, (wie) Jungtiere, die sich verlaufen haben, nach ihren Müttern (rufen), (genau) so klagen die Udumäer (um die Prinzessin Ḥry).” tzm is the only verb in the proverbial section and its marking as a “long’ or as a ‘short’ form is, at best, ambiguous; elsewhere T. analyzes it as one of his /YQTLu/ forms from a III-w root,1228 but only the

1226That T. is not unaware of this issue is revealed by his comment on the use of the imperfective in introductions to direct speech: “Erzähltechnisch werden durch den Gebrauch der PKL wörtliche Reden aus dem Erzählfluß hervorgehoben, wodurch die Erzählung als Ganze lebendiger gestaltet wird” (p. 695 [§76.348]). On the other hand, on p. 696 (§76.412), he explicitly rejects the hypothesis that Ugaritic poetry would be present-oriented rather than past-oriented. He might have taken an entirely different tack, however: the issue is to a certain extent moot if the verbal system expresses aspect, not tense. We know from Hebrew, to name only the most closely related corpus, how an aspectual verbal system functions to narrate what is presented as ‘history’. But the question deserves asking and being answered of whether the Ugaritians viewed their myths as ‘history’. Though one might on some philosophical basis argue that the Baal Cycle was not viewed as ‘history’, it appears more difficult to do so for the Kirta and ‘Aqhatu cycles, in particular the former, which may in some sense have functioned as the founding narrative for the dynasty in power at Ugarit in the late 13th-early 12th centuries (the presence of the qbs dtm, ‘the assembly of Ditānū, in both this text and the royal funerary ritual [RS 34.126, KTU 1.161] leaves little doubt that the Kirta story was seen as ‘history’). If this be admitted, then what we have is ‘history’ being recounted in an archaic poetic dialect, viz., T.’s “Vergangenheit” described in an aspectual system of which the two primary forms are /YQTLØ/ and /YQTLu/ (see below, remark to p. 696 [§76.411]).

1227For one example, see remark above to p. 223 (§41.221.52c), etc., on ylmdnn in RS 92.2106:42’ (RSO XIV 53).

1228P. 200 (§33.323.5), p. 654 (§75.531b), p. 656 (§75.531e), p. 659 (§75.532).
most assiduous reader could be expected to go to the index to ascertain this (neither explanation nor cross-reference is supplied here) and, as has been pointed out above (remark to pp. 653-54 [§75.531b]), it is one of T.’s more dubious grammatical categories. If correctly derived from a hollow root, tn₃₃n is marked in the writing as a long form by the {-n}, whether it be 3 f.du. (parsing údm₃₃ as dual, referring to the two sections of the city indicated in the text as rbt, ‘great’, and t₃rrt, ‘well-watered’) or 3 m.pl. (referring to the inhabitants of the city, which should, however, be written {údmym});¹²²⁹ but this part of the sentence expresses the application of the gnomic phrase and the long form refers to the hypothetical situation that the speaker is depicting as arising if Hûray leaves her native city.¹²³⁰ It would appear, then, that the passage can only have been cited with reference to tzg and one must conclude that, to the extent that this section is intended to correlate morphology and syntax,¹²³¹ the example is not particularly illuminating, even dubious (since, even by T.’s standards, {tzg} could be historically either /YQTLØ/ or /YQTLu/).

— p. 685 (§76.321). The basis for translating bn hpt (line 6 of this same text) as “Jungtiere, die sich verlaufen haben” might have been provided here, or at least a cross-reference provided to the explanation proposed elsewhere (p. 487 [§73.523c]: “Jungtiere, die sich verlaufen haben (w.: Jungtiere der Verirrung ?)”). The question mark is revelatory of the problem.¹²³²

— p. 685 (§76.322). The example of /YQTLu/ in an interrogative sentence cited from RS 24.258:12-13 (KTU 1.114) is probably not valid since the utterance is more plausibly a prohibition expressed as a negative purpose clause (see remark above to p. 663 [§75.533], etc.).

— p. 686 (§76.324), p. 715 (§76.533). In both these paragraphs, the first of which is devoted to /YQTL/ in temporal and conditional clauses, the second to /QTLa/ in conditional clauses, T. comments in an introductory remark to the problems inherent in using temporal categories to describe the use of the two basic forms in identical constructions. Strangely enough, however, he does not exploit these examples to illustrate how aspect functions nor, from a broader perspective, has he allowed these usages to influence his thinking on presenting the aspectual categories in temporal terms. Even the most basic presentation, viz., the titles of the sections, are revelatory of T.’s unease and of the seriousness of the problem: §76.324 is a sub-section of §76.32, which is entitled “PKL für Sachverhalte der Gegenwart,” whereas §76.533 is a sub-section of §76.53, which is entitled “SKf [T.’s abbreviation for /QTLa/ of fientive verbs] für perfektive Sachverhalte der Gegenwart,” that is, with a temporal definition of the first, an aspectual definition of the second. The use of both /QTLa/ and /YQTL/ in both the protasis and the apodosis of conditional sentences is one of the hallmarks of both Ugaritic and Biblical Hebrew. What appears to speakers of

¹²²⁹On these problems, see remark above to p. 442 (§73.223.5), etc.
¹²³¹”Die Sammlung erhebt keinen Anspruch auf Vollständigkeit. Sie enthält überwiegend morphologisch eindeutige Belege” (p. 684 [§76.24]).
temporal languages to be a free interchange of forms must be understood linguistically in aspectual terms, viz., both the protasis and the apodosis may be expressed as either complete or incomplete by the speaker according to his/her view of the situation. This usage is probably one case where aspect reflects realis/irrealis, with the perfective expression used to categorize a condition or an outcome as more real/certain than one expressed by an imperfective. In any case, the expression of conditions and outcomes has nothing to do with time per se, but with the aspect which the speaker accords to each. This is clear from Biblical Hebrew, where many more examples are attested, but is demonstrated empirically for Ugaritic by the hippiatric texts, where /YQTL/ and /QTLa/ forms appear in identical slots in different versions of an otherwise identical sentence.1233

— p. 686 (§76.324), p. 687 (§76.332). In the first section, T. cites yšû in RS 24.247+: 45' and 51' (KTU 1.103) to illustrate the use of the /YQTLu/ form in conditional clauses, in the second yšpū to illustrate the same form in the apodosis. The first is under the heading “Gegenwart,” the other under the heading “Zukunft,” a good example of T.’s categorization of forms and usages in temporal terms. The text in question is one line of a collection of omens, each of which is expressed as an (unmarked)1234 conditional sentence. Line 51' reads: […]bh b ph yšû ḫbn yspū ḫwt, which may be rendered “[And if] its [--]B protrudes from its mouth, the enemy will devour the land.”1235 The apodosis event is logically posterior (= future) to the protasis event, but the protasis event is not ‘present’ to the utterance of the sentence but posterior to it since it is expressed in the utterance as a hypothetical event. In aspectual terms, both events are viewed as incomplete.

— p. 686 (§76.324), p. 799 (§83.231), p. 806 (§85.1). On p. 686, T. indicates hm in RS 24.266:28 (KTU 1.119) as entirely reconstructed, whereas on p. 799 he transliterates “hîm?” and qualifies this as a “[n]eu[l]e[l]sung.” On p. 805, he simply says that “hm” is to be read in place of “[a]l” in CAT. Both signs of this word are in fact partially preserved but only the upper left corner of the first sign is extant and it is, therefore, uncertain whether the conditional particle here was hm or im.1236

— p. 686 (§76.324), p. 688 (§76.332). It is not clear to me why T. includes hm ymt, “Falls er stirbt” (RIH 78/12:19-20 [CAT 2.82:18-29]) in this list of /YQTLu/ forms in conditional

1233Pardee, Les textes hippiatriques (1985)17-18. In §76.533, T. comments on this ‘interchangeability’ but does not explicitly exploit the usage to illustrate how aspect functions in Ugaritic prose, contenting himself with the remark that both the /YQTL/ and /QTLa/ forms may be rendered as presents: “Die SKf findet auch als ‘Tempus’ von Konditionalssätzen Verwendung und ist dabei im Dt. in der Regel präsentisch wiederzugeben.” Such a remark might be considered appropriate in a teaching grammar intended for German (or English) speakers, but one expects more of a reference grammar which in so many respects is based on sound linguistic principles and so often gives no quarter in combating more traditional approaches. 1234The sections of this omen text where the beginning of the line is preserved show that no conditional particle was expressed except, plausibly, in the first line of the text, where it must be restored (Pardee, AFO 33 [1986] 124, 126; idem, Les textes rituels [2000] 546, 549; idem, Ritual and Cult [2002] 139 with note 4, p. 147).


clauses, for it does not bear an orthographic marker of its form and he himself appends a question mark to the parsing as /YQTLu/. He remarks that the form is “möglw. potentialer Sinn,” which is not quite clear either, since conditionals normally have a ‘potential’ component to them. Surely he does not mean thereby to say that the form might by a jussive occurring in a marked conditional phrase (the context precludes any volitional nuance). Nor can it be a /YQTLØ/ perfective, for the text is a letter and the /YQTLØ/ perfective is not used in prose. On p. 688, ilhmn, ‘I will fight’, in line 21 is cited as an example of /YQTLu/ in the protasis of a conditional sentence, again with a question mark attached to the parsing as a long form. Here the question of an alternative analysis is more open because the form in theory could be /YQTLa/, viz., an indirect volitive expressing resolve on the part of the speaker, and all the options for parsing the appended {-n} must be considered (Energic I, Energic II [both in terms of Arabic grammar], /YQTLu + -na/, and /YQTLa + na/).

— p. 686 (§76.324). ytn in RS 15.082:6 (KTU 4.168) is not only unmarked for mood but also for aspect (the root is YT ˙N) and its inclusion in this paragraph on /YQTLu/ forms in temporal/conditional clauses is therefore dubious (see above, remark to p. 512 [§74.222.2], etc.).

— p. 689 (§76.342). After citing passages where actions extended over two days or more are expressed with /YQTLu/-forms, T. comes to RS 2.[024] i 21-24 (KTU 1.22) where a /YQTLØ/ form is followed by two /YQTLu/ forms. He explains the latter as expressing the plurality (viz., iteration) inherent in actions extending over time, whereas the former “könnte stilistische Gründe haben.” This sort of explanation is the weak link in the hypothesis that the scribes knew the poetic dialect well, for ‘stylistic’ is not a linguistic explanation but an admission that we do not know why the scribe would have chosen knowingly to use different forms to describe a sequence of actions that are otherwise situationally identical.

— p. 689 (§76.342), p. 693 (§76.345). In the first section cited, T. explains the /YQTLu/ forms ttlkn // tdn in RS 2.002:67-68 (KTU 1.23) as expressing iteration (the ‘plural’ nature of the act), in the second as providing the “Inzidenzbasis” for the following act. In this second section, he refers back to the former, saying that “Sie bezeichnen zugleich pluralische S[ach]v[erhalt]e,” but the former explanation must be preferred over the latter because the existence of the second category is inadequately substantiated (see remark below to p. 693).

— p. 690 (§76.342). T. here cites two passages from RS 24.258 (KTU 1.114) in both of which /YQTLu/ forms are used and he contrasts one of these (tlhmn ilm w tštn, ‘the gods eat and drink’) with a passage in the Baal Cycle (RS 2.[008]+ iii 40'-41' [KTU 1.4]) where the same verbs with the same subject appear in /YQTLØ/-forms. This may serve as an example of the necessity to come to terms not only with the different verbal systems of prose and poetry but with the fact that not all poems show the same distributions of verbal forms. Neither here nor anywhere else does T. attempt to view the verbal system of RS 24.258 as a whole. As I have remarked above (remarks to p. 513 [§74.222.2], etc., with note 961, to p. 656 [§75.531e], etc., and to p. 662 [§75.532]), there are no certain cases of /YQTLØ/ forms

in this text and it appears, therefore, to reflect a poetic dialect—if one may infer a system from so brief a text—different from that of the major mythological cycles.

— pp. 690-91 (§76.342). An important problem of aspectual sequencing is encountered in the parallel passages describing self-mutilation as a sign of mourning: in RS 2.[022]+ vi 18-19 (KTU 1.5) one encounters ydy // yhdy, ‘he scratches incisions // he cuts’, when ‘Ilu is the actor; the corresponding forms when ‘Anatu is the actress are td // thdy (RS 2.[009]+ i 2-3 [KTU 1.6]). T. describes td as “obenbar eine PK K,” thdy as “erwartungsgemäß eine PKL,” but offers no explanation for the sequence perfective-imperfective in a single verse.

— p. 691 (§76.342). Apparently inspired by cases of this type, T. remarks that in the sequence td // yml° (RS 2.[014]+ ii 25 [KTU 1.3]) the first verb may either be /YQTLu/ like the second or /YQTLØ/, “morphologisch variiert.” He offers no aspectual explanation for the morphological variation.

— pp. 691-92 (§76.343b). In this paragraph, which is simply entitled “PKL asyndetisch nach PK K,” it would have been useful to remark explicitly on the different function of the perfectives in the latter category, the first five examples, as compared with that of the one example of a jussive.

— p. 692 (§76.344). For another example of explaining variant texts by stylistic considerations, see remarks above to pp. 620-21 (§75.232) and to p. 658 (§75.531f), etc., on the parallel to {tb°} appearing as both {tgly} and {tgl}: T.’s solution here is that the use of the imperfective to express “Ausschilderung bereits genannter Themen” was “nicht obligatorisch,” but he offers no explanation for the sequence tgl ... tb° (perfective ... imperfective) in a single verse.

— p. 693 (§76.345). This section is entitled “PKL im Inzidenzschema” and attempts to show that /YQTLu/ forms may provide the “Inzidenzbasis” for following perfective forms, which constitute the “Inzidenzakt.” Only two passages are cited, however, and one of these two does not meet the structural criteria as T. defines them. In RS 2.002: 67-68 (RS 2.002) the /YQTLu/ forms are followed by what is plausibly a narrative infinitive (ngß hm) rather than a /QTLa/ perfective; T. himself recognizes that explaining tltkn // tsdn in this passage as expressive of iteration is valid (see remark above to p. 689 [§76.342]). The latter explanation is also sufficient to explain ykly in RS 3.367 iv 27’ (KTU 1.2) (as well, perhaps, as the two preceding forms of which the aspect is not orthographically marked): the verbs in question describe the acts by which Ba‘lu defeats Yammu and the imperfective apparently

1238 In my study of the texts from this archive that manifest mythological motifs (Les textes paramythologiques [1988] 26, 205-6), I pointed out the higher incidence of participles and of /YQTLu/ forms in at least two of these texts, this one and RS 24.244 (KTU 1.100).

1239 T. recognizes the validity of this anaysis explicitly on p. 483 (§73.513.5b) and on p. 484 (§73.513.6); on p. 468 (§73.333.1) he presented the parsing as a /QTLa/ perfective as a valid alternative, but the structure QTL + independent pronoun visible here is characteristic of the use of the narrative infinitive (absolute) in later Northwest Semitic, in particular in the Phoenician inscription from Karatepe where it functions as the main narrative verbal structure, and may be considered the better parsing in this context. The parsing of ngß as an infinitive is also indicated in the vocalized text in Bordreuil and Pardee, Manuel (2004) II 34. On the inadmissibility of assigning aspectual function to the verbal nouns and adjectives, see above, remark to pp. 682-718 (§76).
visible in ykly (as well as that of the previous two verbs if they are correctly identified as /YQTLu/ forms) may be explained as expressive of inception of action or of iteration. Indeed, the first two forms may have been /YQTLØ/ perfectives (/yaqṭ ba‘lu wa yaṣīt yamma/) and only the third one of the ‘long’ forms (either /yakalliyu/ or /yakalliya tāpīṭa nahara/), here expressing purpose.1240

— p. 693 (§76.346a). One worries whenever one encounters the word ‘obviously’ in an explanation, all the more so when the word appears in the title to an entire section: “PKL in Temporalsätzen, offenbar mit Vorzeitigkeitsnuance.” In this case, though the category may be linguistically viable, none of the examples is convincing. T. begins with three cases of what he takes as unmarked temporal clauses (w yšū, yšū, ‘he raises’, and OBJECT + ymgy, ‘he arrives at’). As regards the first two, he does not remark that /YQTL/ forms of Nš’, however they be explained, are very frequently /YQTLu/; since these two examples occur in clauses unmarked for temporality, the specific categorization of these two, and these two only, as owing their form to the expression of a temporal clause must be doubted.1241

On the other hand, it may be necessary to admit that Nš’ commonly expresses “Vorzeitigkeit” because of its common appearance in idioms where one lifts X before doing Y—one would have wished to see a general statement from T. on the morpho-semantics of Nš’ in addition to its treatment in various of the categories of this chapter. The third example of a /YQTLu/ form in what T. takes as an unmarked temporal clause comes from RS 24.258 (KTU 1.114),1242 at text which, as I have remarked above (e.g., on p. 662 [§75.532]), shows no certain examples of the /YQTLØ/ perfective; the example is for that reason doubly irrelevant, viz., ymgy is not in a temporal clause and there would be no reason to expect a /YQTLØ/ form in this text.1243 Three examples of marked temporal clauses are cited in a second section of this paragraph, one from another ‘para-mythological’ text which, though its form of expression is prosaic, resembles others of the ‘para-mythological’ texts in its extensive use of the /YQTLu/ form (RS 24.272 [KTU 1.124]);1244 a /YQTLØ/ form is, therefore, no more expected here than in RS 24.258. The other two examples come from inscriptions on liver models, where the temporal clause in each case modifies a nominal main clause, e.g., RS 24.312 (KTU 1.141) ‘(This consultation of a liver is) for ‘Agiptarru when he is to obtain a servant-boy from an Alashian’ (l ágštpt tr k yqny ġzr 1d āltyy ). T. translates, “als er daran war, einen Knaben … zu kaufen,” but there is no “als er daran war” in the text, and yqny expresses, in aspectual terms, simply the incompleteness of the purpose

1240 On the problems of parsing the three verbs in this verse, see above, remark to p. 669 (§75.537d).
1241 The two examples cited are yšū ‘nḥ … w y‘n, ‘He lifts his eyes and sees’, in RS 3.362+ ii 13‘-16’ (KTU 1.10) and yšū yr šmnh, ‘He raises (the staff), casts it into the sky’, in RS 2.002:37-38 (KTU 1.23).
1242 ‘trt w ‘nt ymgy ‘trt t dB nšb lh w ‘nt ktp, ‘He goes up to ‘Aṭṭaritu and ‘Anatu; ‘Anatu prepares him a nšb-cut of meat, ‘Aṭṭaritu a shoulder-cut’ (ll. 9-11).
1243 In RS 2.002:38 (KTU 1.23), yšū is followed by a III-y form written without {-y} and it is not certain that y‘n in RS 3.362+ ii 14‘, 15‘ (KTU 1.10) is not a /YQTLu/ form that shows monophthongization of the historical final triphong. In the case of RS 24.258, however, one would not expect ymgy to be anything but a /YQTLu/ form.
of the oracular consultation at the time of the consultation. The clause does not, therefore, represent “Vorzeitigkeit” to an event expressed in the sentence itself (‘when X has done Y, then he does Z’), as is purported to be the case in the preceding examples, but expresses imperfectively the reason for the consultation: if there is any “Vorzeitigkeit,” it is in the nominal clause that refers to the consultation since it preceded in real time the procuring of the servant.

— p. 694 (§76.347). I am far more dubious about the linguistic viability of this section than about the preceding one. It is entitled “PKL in komplexen, durch apnk oder ähr eingeführten Syntagmen.” The only linguistic argument offered for these adverbs being followed by imperfective forms is that the imperfective “scheint vom vorausgehenden Syntagma logisch abhängig zu sein (vgl. §76.343),” but here, instead of the imperfective form expressing a sequence to another verbal expression, the imperfective would depend entirely on the simple adverb. T. himself cites at the end of the paragraph a set of exceptions (viz., ähr followed by what may be perfective forms—though none is certain) and a set of possible exceptions (viz., apnk followed by orthographically ambiguous forms). All of the examples of sentences introduced by apnk may be explained either by the morpho-semantic nature of the verb (two examples of NŠ—see preceding remark) or by seeing iteration in the verbal expression {SLY, ‘to pray’, and ŠŠQY, ‘to cause to drink’). Only one example of ähr is provided, against four possible exceptions in the following “Anm[erkung],” hardly a statistically convincing presentation. The one example is furnished by the twofold occurrence of ymyg in RS 2.[003]+ iv 34 and 47 (KTU 1.14) for which I have tentatively proposed a narratological explanation above (remark to p. 660 [§75.532], etc.). With the possible exception of this last example (where the adverbial expression is in fact complex, consisting of ähr, špšm, ‘at sundown’, and b + ordinal number, ‘on the nth day’), the entire section appears ad hoc in nature and fails to convince.

— p. 694 (§76.347), p. 796 (§83.211), p. 771 (§82.310). On p. 694, T. says that ähr, ‘after’ (in its function as a conjunction), can “offenbar” be followed by /QTLa/ forms and offers as proof ähr mgy ktr w ĥss, ‘after MGY Kōtaru-wa-Hasisu’, in RS 2.[008]+ v 44 [KTU 1.4]), where the orthography does not allow a certain parsing of mgy as a finite form or as a verbal noun. In the other two sections cited he admits freely that ähr could be functioning prepositionally (in which case the verbal form is that of the verbal noun); indeed, on p. 771, he cites a passage where gender incongruence shows clearly that ähr is there functioning as a preposition followed by a verbal noun ([ähr mgyl dt ilm, ‘after the arrival of the gods’ [RS 3.343+ ii 11’, KTU 1.15]). No cases are cited where gender congruence between subject and verb following ähr proves that particle to be functioning as a conjunction with a /QTLa/ form.

— p. 694 (§76.347). What does “Offen ist der Tempusgebrauch …” mean in a presentation of the verbal system as aspectual? What T. appears in general to be trying to do in this chapter is to determine why a given aspect is used to express a given temporal situation. From that perspective, there is no such thing as “Tempusgebrauch,” only aspectual usage representing real-world temporal situations.

Though w y‘ny does indeed appear “am Textanfang” in RS 24.293:1 (KTU 1.133), I do not see the relevance of the observation since this text is, in one sense or another, only an extract from a longer text.\footnote{For a discussion, with bibliography, see Pardee, Les textes para-mythologiques (1988), ch. 4.}

— p. 695 (§76.411). T. is correct in asserting that there is no difference in “aspektuell-temporale Funktion” between /YQTLØ/ and /waYQTLØ/ forms in Ugaritic, but who would expect there to be any? Though he never says so, the statement appears to have been made with reference to the Hebrew ‘wāw-consecutive’ forms and it is almost as if he were denying the existence in Ugaritic of what is known in traditional Hebrew grammar as “wāw-conversive,” i.e., the view that the wāw “converted” the imperfect /yiqtol/ into a past-tense form. If this is the point of reference, the implied comparison is meaningless, for in the Hebrew syntagmeme wāw + /YQTL/, the verbal form is the old /YQTLØ/ which, with very few exceptions, occurs in Hebrew prose only when preceded by wāw and which has nothing to do with proto-Hebrew /YQTLu/. If one includes both prose and poetry in the purview, and if one admits that some /YQTL/ forms in Hebrew poetry descend from /YQTLØ/,\footnote{As T. does on p. 697 (§76.412).} there is no difference between /YQTLØ/ and /waYQTLØ/ forms in Hebrew any more than in Ugaritic (viz., the verbal system of Hebrew poetry is no easier to describe than is that of Ugaritic poetry and /YQTLØ/ was not used in prose in either language). The principal differences between the two languages as regards verbal forms preceded by w are: (1) /waYQTLØ/ did not become a common expression of perfectivity in Ugaritic prose as it did in Hebrew prose, indeed it is extremely rare in Ugaritic prose (for the possible example of {w ymg[-]} in RS 24.272:10 [KTU 1.124], see remark above to p. 660 [§75.532], etc.) and (2) /waYQTLu/ appears to have been used commonly in Ugaritic prose to express simple imperfectivity whereas in Hebrew prose it is used almost exclusively to express purpose and result clauses.

— p. 696 (§76.411). As nearly as I can determine without re-reading the entire section on verbs up to this point, T. first introduces the concept of narrative ‘foregrounding’ here, where he claims that /YQTLØ/ is used in poetry for “Erzählvordergrund” whereas /QTLa/ functioned as a preterit (above, p. 682, he defined ‘preterit’ as expressing past tense). Below, in §76.524 (pp. 705-12), the foregrounding function of /YQTLØ/ is argued in detail but the description of /QTLa/ as a preterit is dropped (the operative phrase on p. 706 is “dient … zur Darstellung isolierter Sachverhalte der Vergangenheit”). The many cases of /YQTLØ/ and /QTLa/ forms used in parallel in a single verse or in adjacent verses must, however, make one dubious about the two forms having opposite narratival functions. T. refers to this use of /QTLa/ as an “erzähltechnische Variante” to /YQTLØ/, which hardly seems a satisfactory solution: if forms are marked for foregrounding and backgrounding, the poet would be creating narratological chaos by mixing them in a single utterance. T. also exaggerates when he says that a narrative section introduced by /YQTLØ/ “in der Regel” is followed by a series of /YQTLØ/ forms (p. 698 [§76.421b])—I have no quarrel with the examples that he cites, but there are many exceptions, e.g., col. I of the Kirta text (RS...
2,003+ \([KTU\ 1.14]\)). T.’s stance appears to take a measure of validation from the fact that many narrative junctures (e.g., change of speaker) are expressed by /YQTL/ forms. That observation, however, raises the further problem of the mixing of /YQTL\Ø/ and /YQTLu/ forms at narrative junctures (striking examples may be found in what T. referred to in §76.348 [p. 695] as “PKL in der Redeeinleitung”—balanced out by §76.423 [p. 699] entitled “PKKi in der Redeeinleitung”). It is nonetheless a fact that, in the major mythological texts, only rarely does one find /QTL/ forms at narrative junctures and, when one does encounter such forms, there are often reasons to believe that the form in question is the ‘narrative infinitive’ rather than the /QTLa/ perfective. What do /YQTL\Ø/ and /YQTLu/, diametrically opposed in aspectual expression, have in common that make them proper for expressing narrative juncture, a feature that /QTLa/ does not share? The obvious answer is: aspect. If the /QTLa/ form was not marked for aspect in proto-West Semitic, as I have suggested (see above note 1222), it may be expected to have been less strongly marked for aspect in the archaic dialect of poetry than /YQTL\Ø/ and /YQTLu/ and hence less appropriate for the expression of foregrounding.\(^{1248}\) This explanation also fits the Hebrew data remarkably well, for /qātal/ is not used in Biblical Hebrew prose for expressing the backbone of a narrative\(^{1249}\) (that function being reserved for /wayQTL/), though /wQTL/ is used commonly for expressing the main line of an imperfective discourse—surely an inner-Hebrew development explained by grammaticalization of the proto-West Semitic non-aspectual /QTLa/).\(^{1250}\) It thus appears that, at least for major narrative junctures, T.’s description is in part valid (he does not incorporate into the foregrounding hypothesis the frequent /YQTLu/ forms at narrative junctures and one would not want to follow him down the path of tensedness to explain the /QTLa/ forms). Given the variety of usage of /QTLa/ in poetry, it appears dubious that it was “marked” for backgrounding (see remark below to p. 708 [§76.524.3c])\(^{1251}\). At the very least, it must be said that the interplay of /YQTL/ and

\(^{1248}\)In his introduction to §76 (p. 682), T. asserts that in many languages, including Ugaritic, the perfective is less strongly marked than the imperfective. Whether one can spot a difference in markedness between the /YQTLu/ (imperfective) and /YQTL\Ø/ (perfective) in poetry is debatable, but, as we have just seen, there are good historical and descriptive reasons for holding that, in poetry, /QTLa/ was less strongly marked for perfectivity than was either of the other forms for their respective aspect. Another perspective: in the case of Biblical Hebrew the ‘perfect’ (←/QTLa/) is marked for perfectivity and the /YQTL/ forms for non-perfectivity + whatever modal nuance they carry (see B. K. Waltke and M. O'Connor, An Introduction to Biblical Hebrew Syntax [Winona Lake: Eisenbrauns, 1990] 347-48; cf. ch. 31, pp. 496-518, entitled “Prefix (Non-Perfective) Conjugation”).


\(^{1250}\)The definitions just given avoid the word “foregrounding” because J.-M. Heimerdinger has shown that, if ‘foregrounded’ material is defined as that which is essential for understanding the discourse, then such material is commonly expressed by w-X qātal forms in perfective discourse (Topic, Focus and Foreground in Ancient Hebrew Narratives [JSOTSS 295; Sheffield: Sheffield Academic Press, 1999]); the same is demonstrably true of w-X yiqtºl forms in imperfective discourse.

\(^{1251}\)He does not use the technical term “marked” frequently; indeed in the introductory paragraph to the use of /QTLa/ to express the “Darstellung vergangener Sachverhalte in der Poesie” (pp. 705-6), he includes backgrounding as only one of three principal functions and does not assert that the form is “marked” for
/QTLa/ forms within narrative units in Ugaritic poetry is not easily explained by a simple foregrounding/backgrounding dichotomy. Grammars prefer to present data piecemeal; it would have been nice to find as an appendix to §76 a detailed analysis of a long narrative passage in which T.’s views on all forms would have been clearly expounded. In sum, it appears that /YQTLØ/ and /YQTLu/ were the primary forms used at narrative junctures, that the infinitive could also so be used but much more rarely, and that /QTLa/ could be used in almost free variation with /YQTLØ/ within narrative units as expressions of perfectivity. Adequate explanations for the interplay between forms expressing perfectivity and imperfectivity at narrative junctures (viz., /YQTLØ/ and /YQTLu/) and within narrative units (viz., these two plus /QTLa/) are more difficult to attain (see above, remarks to pp. 682-718 [§76] and to pp. 684-701 [§76.3-4]).

— p. 697 (§76.421a), p. 712 (§76.524.6b), p. 747 (§81.24b). RS 3.343+ vi 2 (KTU 1.15) is cited with the emendation {<t>l”m} in spite of the facts that (a) the emendation is not noted above in the section on textual errors consisting of omitted signs (pp. 60-61 [§21.354.1]) and (b) T. himself once proposed that the {t} is actually visible on the tablet.1252 On the problem that the assumed emendation poses, see remark below to p. 712 (§76.524.6b).

— pp. 700-1 (§76.427), p. 702 (§76.521). None of T.’s “Mögliche Belege der PKKi in Prosatexten” (§76.427) is convincing. He argues from the single writing {tq††} in RS 1.002:31 (KTU 1.40) that all similar forms are /YQTLØ/ but (a) drawing such a conclusion on the basis of a single writing is methodologically dubious and (b) the text is not in straightforward prose and may be imitating poetic style (see above, remark to pp. 444-45 [§73.233.41-42], etc.). He takes ymג in RS 24.272:10 (KTU 1.124) as /YQTLØ/ but the analysis is uncertain, perhaps epigraphically unnecessary, perhaps even orthographically unnecessary (the questions being whether the form was {ymג} or {ymג[y]} and whether the former could be contracted /YQTLu/); moreover, it is possible that the morpho-syntactic unit to be considered here is {w ymג[…]} (see remark above to p. 660 [§75.532], etc.). Two cases of /YQTL/ forms of L≥K, “to send”, in letters are cited (RS 16.264:4 tliк [KTU 2.26] and RS 34.124:10 tliкн [KTU 2.72]) but with no conviction for, as T. himself recognizes, the expression may each time be imperfective (the -n on the second form marks it as imperfective, for it is 2 f.s.; the first form is 2 m.s. and hence not open to marking by a morpheme including a consonant, but there is to my mind no reason to doubt that that form is

backgrounding. §76.524.3d (p. 708) is devoted to “SKf zur Darstellung des Erzählhintergrund,” but it contains only five examples (not all of which are convincing—see remark below) with a sixth indicated as possible. Note that Heimerdinger’s research cited in the previous note was devoted primarily to foregrounding and that he does not deal to any serious extent with backgrounding structures (as his observations seriously weaken the facile definition of backgrounding as always being expressed by non-‘waw-consecutive’ forms, his reticence on the definition of backgrounding is frustrating). I have devised the following description, which surely needs further refining: “If one accepts Heimerdinger’s view that not all wayyiqtøl forms express foregrounding, then foregrounding is occurring all around these forms while backgrounding may be expressed by w±-X qøtal forms, by non-verbal phrases, and by entire clauses that have this function, particularly circumstantial clauses” (“The Biblical Hebrew Verbal System in a Nutshell,” manuscript in preparation).

1252AfO 42-43 (1995-96) 269: read {’Id\ dl’lh\lm} for {’dm . <t>l’lh\lm} in CAT.
also /YQTLu/. His last case is ylmdnn in RS 92.2016:42' (RSO XIV 53) but T. has already analyzed it as imperfective (see remark above to p. 223 [§41.221.52c], etc.) and finds that solution preferable here also. His formulation that the /YQTLØ/ perfective “begegnet hier … fast nie” (p. 702) though meritorious for its reserve nevertheless overstates the case. Once again, in §76.427 T. has devoted more space to a (highly dubious) grammatical category than it merits—the only form in this list that requires attention as /YQYLO/ is the one cited from RS 24.272:10 and it may have been better discussed in terms of the morpho-syntactic unit /w YQYLØ/.

— pp. 700-1 (§76.427a). In an “Anm[erkung]” to this section of the presentation of possible cases of /YQTLØ/ in prose, T. opines that all the /YQTL/ verbal forms in RS 24.244:61-69 (KTU 1.100) are /YQTLØ/ (“PKKi”). There is one form in this list that appears rather clearly to be /YQTLu/: tlû in l. 68 (see above, remark to p. 656 [§75.531e]), though T. shows his characteristic ambiguity toward it, parsing it once as /YQTLØ/ (p. 617 [§75.222]) once, with a question mark, as /YQTLu/ (p. 660 [§75.532]). ydy in line 64 is formally ambiguous since it is a I-y root. But, observing that there is not a single certain case anywhere in this text of a /YQTLØ/ form and taking this text in the context of the others from this archive, in particular RS 24.258 (see remark above to p. 690 [§76.342]), it may not be judged likely that the ambiguous forms were /YQTLØ/. (T.’s argument that the forms in ll. 65-66 that show the ending {-nh} were /YQTLØ/ may not be accorded any particular weight, for it is tied in directly with his view of the form and function of energics—see remarks above to pp. 497-506 [§73.6].)

— p. 702 (§76.51). In his introduction to the /QTLa/ form, T. correctly observes that it is “von Hause aus nominaler Natur und deshalb tempusneutral,” but he then goes on to opine that “Ihre Funktionen sind als perfektiv zu betrachten.” As observed above (remark to p. 696 [§76.411]), I believe that a good case can be made for the perfective function of /QTLa/ being secondary, one that it took on when /YQTLØ/ fell from usage as a perfective form in the spoken language. Thus its marking as a perfective may be expected to be weaker in poetry, where the /YQTLØ/ perfective is still alive, than in prose, where the entire burden of expressing perfectivity has fallen on /QTLa/.

— p. 702 (§76.521.1). An example from RS 15.125 is T.’s first of the “Belege [of /QTLa/ forms] aus Briefen,” but, in spite of its inclusion in section 2 of KTU (text 19), that text does not belong to the epistolary corpus (it is a legal text).

— p. 702 (§76.521.1). T. translates ḥbtm in RS 15.098:8 (KTU 2.17:1) as “Ḫubṭu-Söldner.” Though this text does not state what type of service these ḥbtm were expected to provide, there is no reason to infer from the writing with {b} that a special category is designated, for the writing could represent either the plural of ḥuptu (/ḥubṭima/ with the original root preserved because of the vowel between the /b/ and the /t/) or the category of worker designated by what may be the G-active participle (cf. ḥbt ạḥd in opposition to b’lm, [textile] workers’, in RS 18.050:7-8 [KTU 4.360]).1253

— p. 702 (§76.521.1), pp. 870-71 (§93.421). In these two sections, T. explicitly parses ṣl in RS 19.011:6 (KTU 2.61) as a /QTLa/ form, translates by “plünderte,” but neither vocalizes

1253See remarks above to p. 110 (§32.144.12b), etc., to p. 137 (§33.112.31), and to p. 226 (§41.222.4a).
nor proposes an etymology. Because the cognates are clearly geminate (Hebrew and Aramaic šl, Akkadian šalālu), one is surprised at the absence of an entry for a root šl in the appropriate section on geminite roots (p. 676 [§75.64]). Others have analyzed šl as a narrative infinitive because of the fact that it is followed immediately by the 3 m.s. independent pronoun (w šl hw qrt, ‘and he plundered the town’). If, however, sb in RS 2, [008] vi 34 (KTU 1.4) is taken as proof that fientive geminate verbs in Ugaritic did not show the /qallā/ pattern of proto-Hebrew, then the only indicator that šl is infintival would be the fact that it is followed immediately by the independent pronoun. T. does not accept that this fact is sufficient basis for the analysis as an infinitive and I tend to share his dubiety on this point. On the other hand, the postulate that fientive geminate /QTLa/ forms were of the form /qallā/ is based on very ambiguous data (see remarks above to p. 672 [§75.61b] and p. 676 [§75.64], etc.), and it cannot be absolutely ruled out that the Ugaritic system was similar to the Hebrew one. In that case the analysis of šl as an infinitive of a geminate root would fit the pattern of qn in RS 92.2014:7 (RSO XIV 52) (see remark above to pp. 577-79 [§74.511a, b], etc.).

— p. 703 (§76.521.1). In an “Anm[erkung],” T. suggests that the verb lhmt, ‘I have fought’, in RIH 78/12:9 (CAT 2.82) may designate an act begun in the past but that extends to the present. Because the document is a letter, this interpretation would have to be nuanced to reflect the fact that the writer of a Ugaritic letter expresses tense by aspect from the recipient’s perspective not his/her own. Once that is recognized, however, it does indeed appear more than likely that the author was referring to an immediate situation, not to an historical one.

— p. 703 (§76.521.2). RS 16.401:3’-4’ (KTU 2.32) is a strange choice for an example of /QTLa/ in an interrogative sentence, not because it is not that but because T. places the interrogative particle and the verb in a restored context that is far from sure. The text reads {lm . likt l […]l-ly}, which T. restores as lm likt [‘m]ny and translates “Warum hast du (einen Boten) [zu m]ir geschickt?” in spite of the fact that the [n] is uncertain and that there is no reason to believe that the lacuna was only wide enough for two signs. This latter fact is clear both from the presentation in CAT and in my transcription of the text made available to T. It is thus possible that likt, rather than being used in a so-called ‘pregnant’ construction with no expressed object, here had a noun as its direct object. RS 19.029:13 (KTU 2.63),


1255He does not explicitly reject that analysis, but he parses the two tokens of /QTL + hw/ in RS 19.011:5-7 (KTU 2.61) and the three tokens of the same sequence in RS 34.124:27-32 (KTU 2.72), the clearest examples of this structure in prose, as containing /QTLa/ forms (on the explicit parsing of ybl and yšq in RS 34.124:27 and 31 as /QTLa/ forms, see pp. 636-37 [§75.514]; on the sequence /QTL hw/ in these two passages, which we must understand as being taken as /QTLa hw/ because of the parsings just cited, see pp. 870-71 [§93.421]).


which is cited as a point of comparison, does not, according to my collation, even belong here, for the reading $\{l\}m$ of $KTU$/CAT is not to be retained (I read $\{l\}l$).

— pp. 704-5 (§§76.522-3). Though a good case can be made for including a category known as ‘Briefperfekt’ in a grammar of this sort, because it involves a perspective on the epistolary exchange that is different from ours (viz., the writer expresses himself from the reader’s perspective rather than from his own as he writes$^{1258}$), the case for the “Urkundenperfekt” is more difficult. The contracts cited reflect situations where the parties are in a single place at a single time and the document at hand represents the writing down of the legal act. The use of the $/QTLa/$ reflects, therefore, the simple perfective or, perhaps, the performative nuance thereof (‘has given’ or ‘hereby gives’, in English parlance). Is it the formula $I \text{ yn \ hnd}$, ‘on this day’, which corresponds to $ištu \ ümi \ annî$, ‘from this day’, in Akkadian, that has created for T. a problem of temporality?

— p. 704 (§76.522). Since $yblt$, ‘I bring’, in RS 2.[008]+ v 27 ($KTU$ 1.4) is part of a speech addressed by a personage who is in the presence of her interlocutor, it cannot by definition be an example of the “Briefperfekt.” Nor is it at all likely that it is a performative, T.’s alternative analysis. It is a simple perfective expressing a complete act, literally ‘I have brought (the proof being that I am standing before you)’. The translation “I bring”$^{1259}$ is nothing but an accommodation to English usage.

— p. 706 (§76.524.1). In a “S[prach]v[ergleichung],” T. contrasts the use of $/YQTL/$ and $/QTLa/$ in Ugaritic poetry with $/wawayqtºl/$ and $/qªtal/$ in Biblical-Hebrew narrative prose. Though his remarks are certainly correct, of more interest would have been a comparison with verbal usage in Biblical-Hebrew poetry, where problems similar to those of Ugaritic poetry are encountered. Such a comparison would have stressed the virtual absence of narrative poetry in Biblical Hebrew and could have brought to the fore the problems of comparing narrative poetry with narrative prose (see above, remark to pp. 684-701 [§§76.3-4], in particular note 1226).

— p. 708 (§76.524.3c). T. cites ‘rb … tb‘ in 2.[004] ii 26’ and 39’ ($KTU$ 1.17) as his sole example of $/QTLa/$ forms beginning and ending a poetic unit. The unit does not begin with line 26, however, but with the preceding bicolon (ll. 24-25),$^{1260}$ which fits the rule that major poetic units usually begin with a $/YQTL/$ form (see remark above to p. 696 [§76.411]). T. explains these forms as “marking” a backgrounding unit embedded within the larger foregrounding unit, which does appear plausible, though the variety of usage of $/QTLa/$ in poetry would seem to require that the notion of “marking” for backgrounding be dropped, viz., the forms would, properly speaking, be unmarked for foregrounding rather than marked for backgrounding (to the extent that these terms correspond to the usage of the principal verb forms—see remark above to p. 696 [§76.411], in particular note 1250).


T. claims that there is a long backgrounding unit in RS 2.[014]+ i (KTU 1.3) beginning with sid in l. 3 and extending through qm in line 18. It would stand in contrast with Ba’lu’s acts, which would be expressed in the foreground by /YQTL/ verbs. He may well be right, but he explains neither why in the three three-verb units that set off sub-sections within this unit each contains two /YQTL/ forms nor why the bicolon that follows this long unit also contains a single /YQTL/ form in spite of the fact that Ba’lu is the object, rather than the subject, of the verb.

Here T. parses ‘ny- in RS 03.325+ v 13, [16, 19], 22 (KTU 1.16) as certain examples of /QTLa/ expressing backgrounding in spite of the fact that above, p. 666 (§75.535a), the parsing as participles was indicated as equally certain.

One wonders what T.’s definition of backgrounding is when he categorizes the /QTLa/ forms in RS 3.322+ iv 46-51 (KTU 1.19) as fitting this category when the subject is identical here and in the preceding section and when this section describes a trip taken by the principal protagonist that brings her into contact with her adversary (first she dresses [/YQTL/], then she travels [/QTL/]). It should further be remarked that there are significant restorations in the text presented by T. (that of KTU/CAT) and the overall structure of the unit is thus not totally clear. Finally, if the last verb of the sequence is correctly restored as [(y)bl], which appears very likely, the root is I-y and there is thus no way of being certain whether it is /QTLa/ or /YQTL/.

Chair-gods and throne-goddesses and jar-gods and barrel-goddesses (RS 2.[008]+ vi 40-54 [KTU 1.4]) are otherwise unknown in Ugaritic and reading the text in that manner is not required,1261 though T. indicates no alternative.

Though he devotes an entire section (§76.524.41) to the “SKF in der Ausführung von imperativisch formulierten Anweisungen”1262 in which he provides quasi-phonetic reasons for the sequence (viz., like the imperative, /QTLa/ has no performative whereas the jussive and the /YQTLØ/ perfective are identical in form), he effectively dispenses of the sequence as revealing any kind of a rule governing the use of the /QTLa/ form by citing many examples of /YQTLØ/ that express the response to a command expressed as an imperative (§76.524.42). Indeed, there are so many examples of the latter that one wonders if the grammarian should take seriously the idea of an imperative –/QTLa/ command-response sequence as representative of anything but the free variation of /QTLa/ and /YQTL/ within poetic units. There is only one fairly long passage where the sequence operates cleanly, RS 2.[003]+ ii 12 - 26 and iii 55 – iv 8 (KTU 1.14), a total of thirteen lines with nine verbal forms that all correspond to imperatives in ii 12-26. This section is followed by a longer one which shows more mixing of volitive forms that tend to mirror each other in the command and response sections (e.g., yi|p in ii 30 and iv 11 or hil|k in ii 40 and iv 19). There is a set of four such mirroring forms in RS 2.[008]+ iv 4'-12' (KTU 1.4). In other texts, imperatives may be followed by either /QTLa/ or /YQTLØ/ forms with no clear structure.

---

1262What has been known in English as the use of the imperative taken up by the perfect in a “command-response” sequence: T. L. Fenton, “Command and Fulfillment in Ugaritic—‘TQTL : YQTL’ and ‘QTL : QTL’,” JSS 14 (1969) 34-38
visible.\textsuperscript{1263} It appears legitimate to say that the poet(s) who created RS 2.[003]\textsuperscript{+} purposely devised a structure based on a series of mirroring forms that would express morpho- semantically the precise carrying out of the commands but that this was an organizing feature characteristic of virtually this text alone (it is debatable whether the four-verb sequence of RS 2.[008]\textsuperscript{+} is long enough to qualify).

— p. 711 (§76.524.5a). Because of the general absence of /YQTLØ/ forms in RS 24.258 (KTU 1.114)—see above, remark to p. 656 (§75.531e), etc.—and the prevalence of participial forms in the ‘para-mythological’ texts (see above, remark to p. 636 [§75.514]), the distant parallelism of yg\textsuperscript{+}r and g\textsuperscript{+}r in lines 11 and 14 may represent morphological parallelism of /YQTLu/ and the participle,\textsuperscript{1264} rather than of /YQTLØ/ and /QTLa/, as T. holds here, with no reference to the other analysis.

— p. 712 (§76.524.5b). If ymnн in RS 2.002:37 (KTU 1.23) is denominative from ymn, ‘right hand’, a notion that T. refuses even to entertain (see above, remark to p. 578 [§74.511b], etc.), then the parallelism there may be /QTLa// /QTLa/,\textsuperscript{1265} rather than /QTLa// /YQTLØ/, as T. holds here.

— p. 712 (§76.524.5b). By the same considerations evoked in the second previous remark, mгy // y\textsuperscript{+}stqł in RS 24.244:67-68 (KTU 1.100) may be participle // /YQTLu/,\textsuperscript{1266} rather than /QTLa// /YQTLØ/, as T. holds here, with no reference to the other analysis. The same remark holds for hlk // y\textsuperscript{+}stqł in and for ng\textsuperscript{+}snn // y\textsuperscript{+}stqł in RS 24.258:17, 19-20 (KTU 1.114).\textsuperscript{1267} If the four examples of /QTLa// /YQTLØ/ concerning which doubt has been expressed in this and the preceding note are removed from consideration, there remain only two in the entire poetic corpus (RS 2.[022]\textsuperscript{+} v 19-21 škb // t\textsuperscript{+}š\textsuperscript{+}ly [KTU 1.5] and RS 3.348 ii 3 ās\textsuperscript{+}r // īs\textsuperscript{+}md [KTU 1.20]), which requires one to ask whether the first of these two forms may not also be a participle or, perhaps more likely in these texts, both may be infinitives—more likely, because, in an “Anm[erkung]” to this section, T. cites some examples of what must, because of the absence of gender marking, be infinitives in parallel with /YQTL/ forms (e.g., t\textsuperscript{+}t\textsuperscript{+} // t\textsuperscript{+}r, where the goddess ‘Anatu is the subject, in RS 2.[014]\textsuperscript{+} ii 20-21 [KTU 1.3]).

— p. 712 (§76.524.6b). In his treatment of the “Austauschbarkeit” of /QTLa/ and /YQTLØ/ in the “Mahlformel,” T. assumes that the verb meaning ‘to eat’ shows the same variation as the verb meaning ‘to drink’ (\textsuperscript{2}st\textsuperscript{+}y and t\textsuperscript{+}st\textsuperscript{+}y) in two texts. In one of these (RS 3.343\textsuperscript{+} vi 02 [KTU 1.151]), however, the presence of the preformative {-t} is at best dubious (see remark above to p. 697 [§76.421a], etc.) while in the other (RS 02.[008]\textsuperscript{+} iii 40' [KTU 1.4]) the entire first part of the line is destroyed and with it the beginning of whatever form of LHМ may have been present. Coupled with this problem is the fact that the preceding particle is

\textsuperscript{1263}Fenton’s claim to a second series in the Kirta text (RS 2.[003]\textsuperscript{+} iii 4-10 and iv 47-v 3 [KTU 1.14]) is not compelling for some of the forms do not stand up to the analysis as sequences consisting of imperatives taken up by /QTLa/-forms.


\textsuperscript{1265}Bordreuil and Pardee, Manuel (2004) II 33, 35.


\textsuperscript{1267}Ibid., 21. On ng\textsuperscript{+}snn, see above, third remark to p. 223 (§41.221.52c), etc.
'd(m) in those cases where the beginning of the formula is extant (twice clearly ‘d lhm: RS 02.[008]+ vi 55 [KTU 1.4] and RS 2.[022]+ iv 12 [KTU 1.5]), and T.’s interpretation as an adverb meaning “lange” may not be considered certain (see above, remark to p. 252 [§51.41a], etc.).

— pp. 712-13 (§76.525). This paragraph is devoted to examples of /QTLa/ “zum Ausdruck der Vorzeitigkeit in der Vergangenheit” and the term “Plusquamperfekt” is used in the introductory paragraph, placed in quotation marks, as part of the explanation for the usage. Whatever the value of such translational categories may be in a reference grammar of a language with an aspectual verbal system, some of the examples are highly dubious even as such, e.g. RS 22.225:1-3 (KTU 1.96), RS 24.244:70-71 (KTU 1.100), and RS 24.258:1 (KTU 1.114). In these three cases, not only is the translational category dubious, but the morphological one as well, since all the forms identified as /QTLa/ may in fact be participles (see remark above to p. 636 [§75.514] and cross-references there).

— p. 714 (§76.531). The analyses of rgmt in RS 16.402:25 (KTU 2.33) as a 1 c.s. /QTLa/ functioning as a performative or as an ‘epistolary perfect’ are not the only ones possible: it may be 2 f.s. and addressed by the writer of the letter directly to the person who transmitted to him the bad news to which reference is made, that is, the writer of the letter to which he is responding. In this analysis, the function would be simple perfective and correspond to the use of the present perfect in English: ‘you have declared’.1269

— p. 715 (§76.532). “Gnomisch” appears a strange term by which to describe habitual actions expressed as perfects, viz., « X has (always) done Y », as opposed to acts having some connection with gnosis. Even more remarkably, /QTLa/ forms of the verb YD‘, ‘to know’, are classified under “SKf zum Ausdruck anderer perfektiver Sachverhalte der Gegenwart” below on the same page (§76.534).

— pp. 716-17 (§76.541a-c). Of the six examples of the /QTLa/ said to express a future perfect (“SKf für perfektive Sachverhalte der Zukunft”) in main clauses, only one is completely convincing: l yr, ‘you must descend’, in RS 2.[022]+ i 6 [KTU 1.5].1270 The one example said to follow the particle k is yld, from the root YLD, but the writing of that form reveals nothing about whether the form is /QTLa/ or /YQTL/ and the context leaves open the question of real-time tense (on this form in RS 2.[004] ii 14’ [KTU 1.17] see above, remark to p. 512 [§74.222.2], etc.). Three of the other four examples1271 contain the syntagmeme w + /QTLa/, where the historical form is, I believe, unmarked for aspect and secondarily imperfective, not perfective. T. maintains that the proto-Semitic /QTL(a)/ form is

1268See my recent English translations in Ritual and Cult (2002).
1271T. includes in this listing the phrase w ikm kn from RS 1.026+:10’ (KTU 2.7), where the verb is probably in fact perfective. The phrase may not mean “und irgendwie wird/möge es geschehen” (cf. p. 905 [§97.42b] “Und wie auch immer es sein wird …’’); it may be a complete interrogative sentence, ‘And how is he?’ (the referent would be the king, who was named in the previous phrase).
perfective,1272 but one may argue from its basic stativity that it was unmarked for aspect and only developed secondarily into the West-Semitic perfective (see above note 1222). These forms in Ugaritic prose may reflect the syntagmeme w + archaic stative (= Ø-aspect) that came to express imperfectivity in Biblical Hebrew—rather than a simple perfective with future application.

— p. 718 (§76.55). T. cites ytn in RS 15.082:6 (KTU 4.168) as an example of stative /QTLa/; this is followed by “ähnl. 4.182:61f.63f.” The passages are indeed similar, but RS 15.106:61-64 (KTU 4.182) contains neither the root YT ˙N nor any /QTLa/ forms. (On T.’s extreme view of the similarity of these passages, see above, remark to p. 512 [§74.222.2], etc.)

— p. 718 (§76.6). On T.’s classification of the active participle as “imperfektiv ausgerichtet” and the infinitive as “demgegenüber offenbar perfektiv ausgerichtet,” see above, remarks to pp. 471-77 (§73.4-73.427) and to pp. 682-718 (§76).

— pp. 719-36 (§77). In this chapter on the “Modalsystem,” T. all too frequently confuses marking/function and translation value. This is perhaps best illustrated by the contrast between his view of nominal sentences, including infinitives and stative /QTLa/ forms (but not participles), which are “von Hause aus modusindifferent” and hence “In den überwiegenden Fällen sind sie indikativisch gemeint” (p. 719 [§77.22]) and finite verbal forms, any of which may be either volitive or indicative. This contrast is visible also in §77.3 (p. 720), where the fientive /QTLa/ is listed among forms that “können volitivische Funktionen besitzen,” whereas nominal clauses are “modusindifferent” but “können … zuweilen volitivische Nuancen zum Ausdruck bringen.” Would it not be more correct to say that /QTLa/, for example, like the nominal sentence to which it is historically related, is unmarked for mood and that its appearance in sequences expressing volitivity (there is no case of a /QTLa/ form used alone to express volition) constitutes a particular use of the perfective aspect? A similar situation, as is clear from languages better attested than Ugaritic, e.g., Biblical Hebrew, certainly obtained in the case of the indicative imperfective: /YQTLu/ forms were used to express a wish on the part of the speaker that was somehow different from expression of a wish by an imperative, a jussive, or a cohortative. (On this basis, one must doubt T.’s explicit exclusion of the /YQTLu/ form from his group of forms that “können volitivische Funktionen besitzen” [p. 720, §77.3]—though /YQTLu/ is not marked for volitivity, wishes could in all probability be expressed in that form in Ugaritic just as in Hebrew.) The moods should be presented as marked categories and similar uses of forms unmarked for mood should be presented as adjuncts to these marked categories; furthermore, an attempt should be made to determine why unmarked or indicative forms were used in volitive contexts. Several of T.’s examples of /QTLa/ forms and of nominal sentences that are presented here as volitional in mood come from the “command” section of the Kirta story, where there are also some /YQTLu/ forms. More interesting than simply listing such forms under the heading “mit volitivischer Nuance” would have been an attempt to explain with why such forms, either unmarked for mood or marked for indicativity, were mixed in with marked volitive forms. Strictly speaking, it is not the unmarked or indicative

form that has a volitive “nuance,” but the context that requires us to translate along such lines.

— p. 720 (§77.311), p. 721 (§77.32). It would have been nice to have some proof of the assertions that the imperative is less polite (“höflich”) than the jussive and that it is for that reason used particularly when addressing equals or subordinates. To my knowledge, this distribution is not visible in Biblical Hebrew, with its more extensive corpus, nor in the much smaller corpus of ancient Hebrew inscriptions, and for that reason more than simple assertions would have been welcome here.

— pp. 721-25 (§77.32). Though T. admits the existence of the first-person jussive (see above remark to p. 429 [§73.143], etc.), he here presents the jussive (/YQTLØ/) as practically limited to the second and third persons, assuming apparently that the first-person volitive was expressed primarily by the /YQTLa/ form. This seems to be a case of assuming that the Ugaritic volitive system was essentially that of the first-millennium Canaanite languages, where the first person jussive had disappeared (as in Hebrew and Aramaic) and the /YQTLa/ form was practically limited to the first person (as in Hebrew; it had entirely disappeared in Aramaic). He himself admits the existence of one first-person jussive (⁄āḥḏ /‘aḥḏi/, ‘I would see’, in RS 3.322+ iii 4, 19, 33 [KTU 1.19])1273 and there is one other virtually certain form of which he does not admit the classification as a jussive (⁄ašši /‘ašši’/, ‘I would cause to go forth’, in RS 3.367 iv 2’ (KTU 1.2): on pp. 589 (§74.622.3) and 623 (§75.237d) he does not translate this form, the stated reason being “Kontext unklar.” The form is located, however, within a sequence of first-person forms, all of which may plausibly be analyzed as 1 c.s. jussives.1274 Given the very small number of criteria for vocalizing Ugaritic texts, the existence of these two clear forms must be taken as indications that the first-person jussive form was still a living form in poetic diction.

— p. 721 (§77.322a). T. usually makes a point of establishing morphological categories by orthographically distinct forms, but such is not the case here. He cites six examples of 3d-person jussive forms (/YQTLØ/) from poetic texts only one of which is clearly determined by the orthography: {tši} represents /tši’/. In the other five cases, therefore, there is no objective way of determining whether the event in question was expressed as a wish or as a vision of the future. For example, in RS 2.[008]+ iv 62’ (KTU 1.4) ybn, ‘BE BUILT’, may be /YQTLØ/, /YQTLa/, or /YQTLu/; there can be no doubt that ‘Ilu is decreeing the construction of a palace for Ba‘lu, but precisely how he expressed the decree is uncertain; though the writing with {-y} makes the analysis as /YQTLØ/ the preferred one, triphthong contraction does not correspond to orthography as regularly as one would wish and that analysis of {ybn} cannot, therefore, be considered certain. Judging from Biblical Hebrew morpho-syntax, at least one, perhaps two, of these examples could be /YQTLu/ or perhaps /YQTLa/. ykn in RS 2.[004] i 25’ and 42’ (KTU 1.17) follows the conjunction w and may function as a purpose/result clause. The verb after the conjunction in such cases is

marked in Biblical Hebrew as /YQTLu/ or, if 1st person, it may be /YQTLa/.1275 Another such case may occur in col. ii, line 13, of this same text, where w tnḥ follows 1 c.s. energetic forms.

— p. 722 (§77.322b). In this paragraph, six passages are cited from poetry which are said to contain 2d-person jussive forms; only one of these forms is orthographically distinctive (|tšt| 2 f.s.; the indicative form would be {tstå} while another depends on a “n[eue] l[esung]” (what has previously been read as {w dū} in RS 3.322+ iii 14 and 28 [KTU 1.19] is here read as {w tdū}1276). T.’s first example in the list is particularly unconvincing, for it depicts Kōtharu-wa-Ḥasısu as predicting Ba’lu’s victory over Yammu (RS 3.367 iv 9’ [KTU 1.2]), and one might expect the certainty of victory to be expressed by the indicative1277 rather than allowing whatever element of doubt is contained in a volitive. Another pair of examples depends entirely on T.’s view of the energetic: {tbrkn … tmrmn} in RS 2.[004] i 23’, 24’ (KTU 1.17) are enerics to which the /h/ of the pronominal suffix has assimilated (‘may you bless him’), and the appropriateness of including these forms among the jussives depends entirely on whether the energetic forms are independent moods or consist of /YQTLØ/, /YQTLu/, or /YQTLa/ + energetic ending (see remarks above to pp. 497-506 [§73.6]). This same stricture applies to {tškn}, cited among the examples from prose (RS 18.148:3, 5 [KTU 2.47]). At this juncture, it appears more plausible to say that the basic verbal form here was /YQTL-a/-, whether the form be analyzed as consisting of the productive volitive + energetic ending or as one of two independent energetic moods. Finally, it is entirely inappropriate to cite among prose passages RS 1.018:6-7 (KTU 2.4) as providing an example of the jussive because the verbal form is half restored ({|tṭ[ṭb]}) and because the set of restorations to which that one belongs is without parallel (see remark above to p. 594 [§74.622.3], etc.). It is even less proper to cite this form as, by implication, certain—certain by implication because four explicitly “unsichere” examples are cited at the end of this paragraph. Three of these “unsichere” forms, by the way, go beyond simple uncertainty to the point of not belonging in a serious grammar: on RS 1.021:12-13 (KTU 2.6:13-14), see the remark just cited; the context of {tšt} in RS 1.026+:7 (KTU 2.7) is, as T. acknowledges, broken and there is simply no way of knowing what the mood of the verb may have been; {tṭtb ly} probably does not occur in RS 17.327:7 (KTU 2.35—T. cites the reading from CAT while remarking that Bordreuil/Pardée disallow it).

— pp. 723-24 (§77.324a-c). None of the examples claimed to be of jussives following imperatives is orthographically marked. Some of the cited forms are enerics (on which problem see preceding remark). Especially when the two forms are addressed to the same person, it is not at all certain that the second form is marked as a jussive, rather than as an

1275T. cites this passage below, p. 728 (§77.38) and p. 913 (§97.10.2b), where he holds that the purpose/result clause is expressed by /YQTLØ/, but the only orthographically marked form is /YQTLa/ (see below, my remark to §77.38).


indicative expressing the certainty of the outcome, e.g., \( tb' \ldots tbn \), ‘go ... BUILD’: the second form could be either /tabni/, ‘you must build’, or /tabnû/, ‘you will certainly build’.

— p. 724 (§77.325). T. lists \( al \) in RS 16.265:14 (\textit{KTU} 5.9) among examples of the particle \( \{ al \} \) that is used as a negative volitive particle, rather than in the section dealing with \( \{ al \} \) used as an emphatic particle (cf. pp. 805-7 [§85.1]), probably because this would be the only attestation of the latter usage in prose. In this analysis, he follows a long line of tradition.\(^{1278}\) It appears far more likely, however, that the scribe of this practice letter is showing off his erudition by producing as many forms of the verb YTN as possible, and all are positively polarized because the final clause says ‘give me a cup of wine that I might drink it!’\(^{1279}\) It appears highly unlikely that he would have inserted the negative command, ‘don’t give’, in this list of injunctions. Another example of this scribe’s imitation of poetic speech is cited in the remark to p. 777 (§82.414).

— pp. 726-27 (§77.34). A general remark on T.’s examples of what is known traditionally as a ‘precative perfect’ (viz. /QTLa/ used as a volitive): in addition to the possibility that some of these may be participles functioning as attributive or predicate adjectives rather than volitives (see following remarks), T. makes no effort to prove that the few provable examples of /QTL/ volitves are not infinitive used in place of a marked volitive.\(^{1280}\) When these two possibilities are considered, one must conclude there may not be a single case of /QTLa/ used as a volitive in Ugaritic.

— p. 726 (§77.34a). T. cites only one example of /QTLa/ as a volitive “in absoluter Verwendung” (viz., without an accompanying marked volitive form) and that example is far from certain: \( âhd \) in RS 2.[008]+ v 56 (\textit{KTU} 1.4) may be a participle: ‘/’alpa šiddi ’âhida bêta/, ‘(… build) a house covering a thousand šiddu-measures (of land)’.

— p. 726 (§77.34b). Though the possibility certainly exists that \( ngb \) in RS 2.[003]+ ii 32, 33 (\textit{KTU} 1.14) is /QTLa/ or infinitive with volitive value from context, for the simple reason that \( hlk \) certainly has that value in lines 39 and 41, the analysis as a G-stem passive participle is equally plausible: ‘let the army, having been provisioned, go forth’. Also to be considered is the analysis of \( ãly \) in RS 3.322+ iii 54 (\textit{KTU} 1.19) as a G-participle: ‘May your root not send up sprouts from the earth, (your) head having come down into the hand of him who would pull you up’. The poet may purposely have used two participles in the same verse in order to amplify the contrast between the actors. According to this analysis, \( šršt b ârš àl yp’ // riš ãly bd ns’k \) would be vocalized /šuršuka bi ’arsi ’al yipa’ // ra’šuka ġâliyu bîdê nāš’ika/.

\(^{1278}\)Virolleaud, \textit{PRU} II (1957) 40: “Et que tu (le) donnes ou que tu ne (le) donnes pas ...”; Gordon, \textit{UT} (1965) §13.79 (p. 127): “if thou wouldst give, then give; and if thou wouldst not give, then don’t give”; Del Olmo Lete and Sanmartin list this text among their examples of the “fonctor neg.” and translate “no des” (%\textit{Diccionario} I [1996] 22).


\(^{1280}\)He actually includes here a paragraph entitled “Verbalsubstantive mit volativischer Nuance” (§77.36), but it is only four lines long, states that all possible examples are disputed, and refers the reader back to §73.532 (pp. 492-93), which is entitled “Imperativischer Gebrauch” and deals only with possible examples of verbal nouns used to replace marked imperative forms, viz., there is no discussion of verbal nouns replacing jussives.
— p. 726 (§77.34c). T.'s preferred analysis of yblk, ‘they will bear (it to) you’, in RS 2\[008\] v 17 (KTU 1.4) is as a /QTLa/ form because if it were /YQTLu/ it would be in parallel with tblk, with t-preformative. The alternative that he considers, viz., that the poet purposely used a 3 m.pl. form with y-preformative in parallel with a 3 m.pl. form with t-preformative, is a nonetheless plausible one in this text from the Ba‘lu cycle, the one group of texts where the appearance of 3 m.pl. forms with y-preformative is relatively clear.

— p. 727 (§77.35). It is not necessary to take rm in RS 3.343+ iii [2'], 13' (KTU 1.15) as volitive in nature.1281 The reference need not be to some future uplifting of Kirta in the council of Ditānu. The latter may, from the viewpoint of this mythological text, be chairing the council in which Kirta holds an important place. His presence among the ‘shades’ of the dead may not be forecasting his death; one may see him as belonging to the epoch from which these revered shades hailed.

— p. 727 (§77.35). T. correctly rejects the attempt by Cunchillos to interpret šlm in the epistolary formula ‘m X mnm šlm as a volitive, as is proven, if need be, by the fact that the Akkadian equivalent is a nominal phrase, e.g., {it-ti LUGAL KUR-u-ga-ri-it (8) ù it-ti-ki mi-nu-me-e (9) šul-ma-nu [é-ma te-er-ri]}, ‘With the king of Ugarit and with yourself, whatever well-being (there may be), send (me) a message (on that topic)’ (RS 16.111:7-9 [PRU III, p. 13-14]). Since the Akkadian formula is only attested in the west, however, it must be judged unlikely that the Ugaritic “auf einer akk. Vorlage beruht.” Because the formula is clearly not of Mesopotamian origin, is somewhat awkward in Ugaritic, and is only attested within the Hittite sphere of influence, its origin may even have been Hittite.1282

— p. 727 (§77.37). Because “Nominalsätze sind von Hause aus tempus- and modusneutral,” one may doubt that they “können aber auch volitivischen Charakter bestizen,” which is not to deny that nominal phrases are used in volitive contexts, just as /YQTLu/ indicative forms are used in such contexts— it is just that one is required to deduce from the context that the act so expressed is included in the list of desired events. In T.'s first example, the phrase ‘mk šb’t ţlmk is translated “Bei dir seien deine sieben Burschen” because the nominal phrase is preceded by the imperative qh, ‘take’ (RS 2\[022\]+ v 6, 8-11 [KTU 1.5]) and there can be no exception taken to the translation. But seeing this as a category of volitivity is an entirely different question, as the prepositional phrases may be interpreted as depending directly on qh1283 or be translated literally into English, i.e., with no explicit marking of volitivity, and yet retain their comprehensibility.1284

— p. 728 (§77.37), p. 858 (§92.42c and §92.43b). The context permits taking šlm in RS 2.002:7 and 26 (KTU 1.23) as the D-stem imperative, addressed to the gracious gods who are the subject of this text,1285 rather than as a noun used volitively. Though šlm in RS 34.126:31 (KTU 1.161) is, on the other hand, nominal, neither does it occur in a direct

1282See remark above to p. 244 (§45.122b), etc., and Pardee, Fronzaroli (2003) 466 n. 66.
volitive. T. follows some authors in taking the two occurrences of that word in line 31 as having an identical function (“Heil! Heil (sei dem König) ‘Ammura[pi] …!”), but, as the editors of this text have held from the beginning, the first šlm is to be construed with the preceding phrase: ‘You are to present (a) bird(s) as an offering of well-being: well-being for ≤Ammurªpi≥ …’.1286 Finally, it is not clear why T. cites only lines 28’-30’ and 33’ of RS 24.271 (KTU 1.123) as further examples of šlm meaning “Heil!” when šlm appears also in lines 1-3 of the same text. In all these occurrences also, however, the word may be parsed as verbal rather than as nominal.1287
— p. 728 (§77.37), p. 859 (§92.52). Even if the reading of the last word be accepted,1288 āt āḥ w ān ā[tk] in RS 3.340 i 24 (KTU 1.18) need not be taken as a volitive nominal sentence.1289
— p. 728 (§77.37). There is no reason to believe that the Ugaritians translated their epistolary formulae from Akkadian (“vgl. die akk. Wendung lù šulmu ana …, die als Briefgrußformel im Ug. sonst verbal … wiedergegeben wird”). When each of the formulae is examined in detail, it becomes clear that the formulae are not for the most part Mesopotamian in origin; if the Ugaritic form is in any given case calqued on a formula known from Akkadian it is because the ultimate origin of the formula was neither Ugaritic nor Akkadian (cf. the possible example of the formula mnm šlm ‘m X rgm TTB ‘my, discussed in the remarks to p. 244 [§45.122b], etc.; p. 246 [§45.23a], etc.; p. 727 [§77.35]); p. 856 [§92.238b]; p. 904 [§97.42]).
— p. 728 (§77.37). A particularly striking case of a nominal sentence gaining force from translation as an expression of indicativity (i.e. certainty) rather than as a wish is provided by RS 24.252:23’-27’ (KTU 1.108): not ‘may your strength be X’, but ‘your strength will be X’.1290
— pp. 728-29 (§77.38), p. 913 (§97.10.2b). Biblical Hebrew cannot, of course, dictate the analysis of Ugaritic forms, but it does seem reasonable to expect that the basis should be indicated for the assertion that /YQTL/ forms in purpose clauses are /YQTLØ/ volitives (see above, remark to p. 721 [§77.322a]). In Biblical Hebrew, a jussive form (i.e., /YQTLØ/) in a sequence of verbal forms has the force of a simple jussive, whereas an indicative form

---

1288 Cf. Wyatt, Religious Texts (1998) 279 n. 132, who claims that the last visible sign begins with a vertical wedge, not a horizontal, but does not propose a reading/restoration. My collation of the tablet in July of 2004 led me to believe that the traditional reading here is plausible. I observe that the editor’s copy might lead one to think of a vertical wedge here (Virolleaud, Danel [1936], pl. X: reproduced in Herdner, CTA [1963], fig. 57), but what one finds on the tablet better fits the reading as a horizontal wedge.
1290 Pardee, Les textes para-mythologiques (1988) 82; idem, Ritual and Cult (2002) 195. For this fundamental view of the force of the nominal sentence it is immaterial whether the last sentence of the text begin in line 23’, as I believe, or in the following line, as T. translates it (“Deine Kraft … seien inmitten von Ugarit”).
after the conjunction (i.e., wά + /YQTLu/) in such a sequence functions as a purpose or result clause, whether the previous verb be volitive or indicative. Though he does not indicate his reason for the parsing of wykn in RS 2.004 i 25’ and 42’ (KTU 1.17) as /YQTLØ/, one may deduce that it is because /YQTLa/ is attested as the second verb in such a sequence when the verb is in the 1st person (w úbâ in RS 24.244:72 [KTU 1.100]) for /YQTLa/ is, of course, for T. only an expanded form of the /YQTLØ/. But, if the latter theory not be tenable (see remark above to p. 429 [§73.143], etc.), and if the semantics of the /YQTLa/ form not be so closely tied to those of /YQTLØ/ as T. believes, then there may be room for positing that purpose/result clauses were expressed in Ugaritic, as they were in Hebrew, by /YQTLu/ and /YQTLa/ verbal forms.

— p. 732 (§77.412.2a). Above, p. 500 (§73.611.2d), T. identified tmtn in RS 3.325+ i 18, ii 40 (KTU 1.16) as /YQTLu/ + Energic I, here that analysis is indicated first but T. then suggests in parentheses that the basic verbal form may be /YQTLØ/. On p. 500, he translated “mußt du sterben”; here the alternative translation is “Sollst auch du … sterben?” I can think of no case in Biblical Hebrew, a language in which the distinction between jussive and indicative forms is more extensively visible, of a jussive used to express constraint from without in an interrogative sentence, either marked as interrogative or interpreted as such from context. T.’s alternative analysis would appear, then, perhaps to be too closely tied in with the equation in his mind between the West-Semitic jussive and German sollen. As regards T.’s view of the energetic system, see remarks above to pp. 497-506 (§73.6), etc.; it may be noted here that, in a structure such as this one, the question of whether the form be indicative + energetic or an energetic form as constituting an independent mood is of no real importance for interpretation, for the function of the form appears to be only to express a degree of emphasis (‘will/must you indeed die’?). If, however, the energetic forms did represent independent moods, the question regarding the indicativity or the volitivity of the basic verbal form would become moot: the mood would be neither emphatic indicative nor emphatic volitive, but energetic.

— pp. 734-35 (§77.5). It must be judged doubtful that two pages of a reference work such as this should be devoted to “Die modalen Nuancen ‘müssen’, ‘dürfen’ und ‘können’ ” when, as T. makes clear in the introductory paragraph, there is no marking in Ugaritic for such “nuances.” One can see a place for such a translational category in a teaching grammar intended for German speakers, but not in a reference grammar that is addressed to the international community of scholars. Moreover, some of the examples are open to question on a purely translational level. One wonders why, for example, T. included RS 11.772+:25’ (KTU 3.1) here, when he himself translates by “er … zu bringen hat” rather than by one of the verbs included in the title of the paragraph and when the simple future works perfectly well in French, for example. Certainly in French and in English, the future indicative gets the idea of obligation across in various contexts, such as contracts or when deities are

1291 This becomes even clearer below, pp. 734-35, in the section devoted to “Die modalen Nuancen ‘müssen’, ‘dürfen’ und ‘können’ ” (§77.51) where it is stated that /YQTLu/ “für die modale Nuance ‘müssen’ steht” (p. 735); one notes the absence of sollen anywhere in this section.

speaking, with a different force but one that may not be considered of less force than would have been present in a marked volitive (on the strength of indicativity, cf. above, e.g., remark to p. 722 [$77.322b$]). This is illustrated here by contrasting T.’s rendering of *is*‘ in RS [Varia 14]:10 (*KTU* 3.9) as “so verpflichte ich mich … zu zahlen” with the simple English “I shall pay,”$^{1293}$ which, in the contractual context, expresses the legal obligation with no ambiguity.

— p. 735 ($§77.51b$). The problem of whether *t*-preformative forms in the ritual texts are 2 m.s. (as I have taken many of them) or 3 m.pl. (as T. holds for many examples) is not solved by citing forms that may be parsed as passives and forms of which the subject is explicitly indicated in the sentence itself. The problem arises when neither of these solutions is possible, and some passages of the latter type preclude the analysis as 3 m.pl. (see remarks above to p. 211 [$§41.12$], etc., and to pp. 505-6 [$§73.634a$]).

— p. 735 ($§77.53$). Restricting *l tś‘n* in RS 19.066:14 (*KTU* 3.8) to the meaning “nicht zahlen können” is legally incorrect: the persons in question must undergo the stated penalty if for any reason they do not pay the stipulated amount of money. ‘Not being able’ to do so is only one possible situation. Translate: “If they do not pay the 1000 (shekels of silver), they will be sold into Egypt.”

— pp. 737-38 ($§81.11a-d$), p. 751 ($§81.4e$). In the first sections cited here, T. presents the case for *hn*, as well as the expanded forms *hn*, *hnny*, and *hndt*, functioning as local adverbs meaning ‘here’ or ‘hither’. The entire category may, however, be reduced to a single example and, in that example, the local adverb is in all probability to be distinguished from the presentative particle by its vocalism. In RS 16.402:31, *hn* may indeed be a local adverb (see above, remark to p. 229 [$§42.0$], etc.), but it is to be identified with Arabic *huna*, not with Hebrew *hinne*.$^{1294}$ Thus the Ugaritic local adverb would have been /huna/ and only a cousin of the presentative particle, whereas the latter would have been /han(na)/. Other than in this passage, *hn* functions uniquely as a presentative particle in Ugaritic, and the same is true of the expanded forms *hnn* and *hnny*; *hndt* is only attested to date as a demonstrative pronoun (see above, remark to p. 230 [$§42.3$], etc.). Epistolary usage shows that, if one of the presentative particles has taken on a local nuance, it is *hlny*, not *hnny*. This is demonstrated by the fact that the two are interchangeable in the double formula of well-being$^{1295}$ but not at the beginning of the body of the letter, where only *hlny* is found (see above, remark to p. 197 [$§33.322.42a$], etc.), apparently with the purpose of stressing that the events to be recounted occurred where the writer is located.$^{1296}$ I would hold, therefore,


$^{1294}$The origins of the Hebrew particles based on H + N remain to be worked out in detail: judging from the comparative evidence, there should have been in proto-Hebrew a deictic particle /han-/ , a local particle /hun-/ , and a conditional particle /hin-/ . The first two coalesced as /hinn-/ , except in the definite article where the /a/ is retained, while the third shows up as ‘im, already known from Ugaritic as a by-form of /himma/ (← hin + ma/), hence ‘/imma/.


$^{1296}$There is not a single case of reference by *hlny* to a situation occurring in the sphere of the addressee, not even {hlny . āḥl[y][…]} (RS 19.181A [*KTU* 2.67]), where the word, ‘my brother’, in all likelihood refers to
that T. has reversed the proper interpretation of the particles when he translates $hln$ $hn$ `mn $\tilde{slm}$ as “Siehe, hier bei mir ist Wohlbefinden.” It should rather be ‘Here, behold\textsuperscript{1297} with me it is well\textsuperscript{1298}’.

— p. 738 (§81.11c), p. 743 (§81.21a). T. is correct, as we have just seen, in observing that $hlny$ and $hlny$ are interchangeable in the double formula of well-being, but he gives the wrong impression when he says that $ht$ fills that slot in RS 17.139:6 (KLU 2.34), for there $ht$ introduces the main clause of the formula by which one refers to a previous letter by a casus pendens then responds to something, usually a request or a reproach, that was stated in that letter.\textsuperscript{1299} That main clause happens in RS 17.139 to be the double formula of well-being because the preceding letter had asked him how he was: the reference to the previous correspondence is \{l$ht$ . $\tilde{slm}$ . k . lik\textsuperscript{1} (6) $\tilde{umy}$ . ‘my\}, ‘Concerning the fact that my mother sent me a letter asking how I was’, lit. ‘The tablet of well-being, (concerning the fact) that my mother sent (it) to me \ldots’, and the response is \{ht . ‘mny (7) kll . $\tilde{slm}$\}, ‘here with me everything is fine’. Given that the formula of well-being does not need to be introduced by a presentative particle,\textsuperscript{1300} $ht$ is not, formulaically speaking, playing that role here; rather it is a presentative particle introducing the apodosis. That particle is not, therefore, yet attested as one of those by which the double formula of well-being is introduced.

— pp. 741-42 (§81.13h). It is a strange grammatical procedure to create a section for an unattested grammatical entity because it might have existed. This section proposes that $yd$, which is attested only as a preposition meaning ‘with, along with’, “könnte aber auch als Adverb \ldots gebraucht worden sein.” No texts are cited, of course, because the usage is unattested.

— pp. 742-43 (§81.21a). T. classifies the particle $ht$ as a temporal adverb in poetry, as a presentative particle in prose. This distribution must be judged dubious. Only one text can be cited in which the temporal function is clear and there $ht$ is preceded by the preposition l (l $ht$ w ‘lmn, ‘for now and evermore’, RS 2.322 iv 5-6 [KLU 1.19]). One could argue that the preposition was needed to give the particle a specifically adverbial notion (in spite of the parallel construction l ‘nt, where ‘$nt$ is certainly a temporal adverb). In one other case, $ht$ appears after a sentence-initial verb but before the subject ($\tilde{ish}$ $ht$ $\tilde{atrt}$, ‘let ’$\tilde{A}$tratu herself rejoice’ [RS 2.[009]+ i 39, KLU 1.6]), but T. does not here make a case for post-positioning

\textsuperscript{1297}The translation with “behold” is purely conventional as modern American English no longer has a broadly utilized presentative particle; it corresponds to the still living voici in French. For the interpretation of hl- and hn- just outlined, see Bordreuil and Pardee, RSO XIV (2001) 373-74; Pardee, Context III (2002) 112 n. 193; idem, Fronzaroli (2003) 451; Bordreuil and Pardee, Manuel (2004) I 73, II 160.

\textsuperscript{1298}There are good reasons for believing that $\tilde{slm}$ in this formula is verbal, rather than nominal: see remarks to p. 246 (§45.23a), p. 856 (§92.238a), and Pardee, Fronzaroli (2003) 453-54.

\textsuperscript{1299}For the analysis of the epistolary formula and of the particular manifestation of the formula in RS 17.139, see idem, BiOr 34 (1977) 8.

as a criterion for disqualifying particles as presentative in function. If this particle is derived from *hn*, as T. holds and as does indeed appear likely, and if *hanna* is not a local adverb but functions only as a presentative (see above, remarks to p. 230 [§42.3], etc., p. 231 [§42.5], etc., and to pp. 737-38 [§81.11a-d]), then the basic function of *ht* is probably presentative as well. Since the function is clearly presentative in prose, T.’s view of the function in poetry would mean that the function of the particle had evolved from temporal in the archaic form of the language preserved in poetry to one more or less identical to that of *hn* in prose, hardly a likely scenario. T. translates by “jetzt” and by “nun” (sometimes more freely); “nun” is certainly the better of the two for, like ‘now’ in English, that word can function either temporally or as an interjection focusing attention on the following word or phrase. In any case, it is just as inaccurate to build a grammatical category on these translations as it is to classify Hebrew w^ bracelets attâh as functioning temporally on the basis of the common English translation ‘now’ (as is commonly done, though not by T.). In all examples but the one cited here above, *ht* is better translated by something corresponding to the conventional ‘behind’ than by an adverb which primarily expresses temporality. A typical example is the tricolon in RS 3.367 iv 8'-9' (KTU 2 iv) *ht* ibk b’lm // *ht* ibk tmhš // *ht* īsmt šrtk. T. translates *ht* each time by “jetzt,” whereas one finds ‘behind’ in some English translations, ‘now’ in most. Finally, it is not clear why the putative function as a temporal adverb and that as a “Präsentationspartikel” are both presented in the same section when there is a section below on “Demonstrative Adverbien” in which *hn* as a presentative particle is treated; thus the treatment of *hn* is divided into two sections according to its two putative functions whereas that of *ht* is in a single section.

— p. 743 (§81.21c). It is highly unlikely that there was no vowel between the second and third consonants of the particle ‘nt, ‘now’, a possibility that T. leaves open in his vocalization “‘an(V)ta.” If there had been no vowel there in proto-Ugaritic, the /n/ would have assimilated to the following /t/; if that vowel had reduced by syncope in Ugaritic, a murmured vowel would have remained.

---

1301 That such a criterion would be invalid is proven by RS 96.2039, where the sequence *w* *ht* *hn* appears three times and *hn* cannot mean ‘here’ because in one case the position of the referent is explicitly said to be with the writer, i.e., ‘there’. Hence both *ht* and *hn* are functioning as presentative particles and one of the two is perforce not phrase-initial.

1302 My dictionary identifies *jetzt* only as an adverb, *nun* as either an adverb or a particle, the latter translated by the English rhetorical ‘now’ or left untranslated.


— p. 744 (§81.22b). T. is correct in reading {±pn} in RS 2,[004] i 1' (KTU 1.17), rather than {±ph}, which was indicated as a certain reading in KTU and retained in CAT. (The reading of {n} rather than {h} may be judged certain.) On the other hand, he does not justify his decision to emend {±pn} to {±p<h>n} other than by the assertion that ±phn is used in a « stereotyped » way in this text. If the particle does indeed consist of an expanded form of ±p, I see no reason to deny the existence of an adverb consisting of ±p + -n (T. admits only the existence of a conjunction so spelled). T.’s proposals to vocalize this and the following particle also lack coherence: this one is vocalized “appV-hinna” while ±pnk is vocalized “ ’appūnaka.” The first element of ±phn is identified specifically with what T. takes to be a conjunction ±p (on this question, see below, remark to p. 791 [§83.131], etc.), whereas this explicit identification is not stated in the case of ±pnk, which is compared with Akkadian appūn(a)ma). I would remark that the doubling of the /p/ is dubious in West Semitic (see remark just cited), there is no obvious basis for the final /ā/ (above, hn is vocalized as “ha/innV”), and the vowels of ±pnk may not have been identical in West Semitic and in Akkadian. The origin of the /ū/ particularly obscure: /’apunaka/ I could understand because the linking vowel between /’ap-/ and /-na/ is unknown 1305, but what would be the origin of the length in /ū/? If one takes the Akkadian form as evidence for the vocalization of the Ugaritic forms and reconstructs the latter on West-Semitic principles, the three forms just discussed may have been /’apuhanna/ 1306, /’apuna/, and /’apunaka/.

— p. 744 (§81.22e). T.’s confident derivation of idk, ‘thereupon’, from a noun /’iď-/,”Zeitpunkt, Mal,” plus the relative/determinative pronoun (which he here vocalizes as /dā/ with a question mark) plus the afformative particle “-k” is certainly plausible but not the only possibility. Hebrew ‘āz shows that the corresponding Ugaritic particle i may consist of the relative/determinative with prosthetic ‘alif and the Ugaritic particle i dk may be simply this particle expanded with -k. The Hebrew form also shows, by the absence of a final vowel, that this particle stems from a more basic particle with a final short vowel, rather than from the relative/determinative particle itself of which the vowel was long. The Ugaritic form may thus have been /’iďaka/ rather than /’iďdāk/.

— p. 745 (§81.22e). There is no basis whatever for the restoration of i dk in RS 18,[528]:5' (KTU 2.60), where the reading is {iďl}[…]). It would be illegitimate to criticize too harshly T.’s obervation that the literary genre of the text is uncertain, for the fragment is very small, but the editors’ classification, with question mark, as a letter1307 must be judged the most

1305I have in the past indicated /a/ as the linking vowel (e.g., Les textes rituels [2000] 877, vocalization of ±phn in RIH 78/20:8), on the assumption that adverbs appear more frequently with a marking like that of accusatives than with nominative marking, but the Akkadian form may be considered a basis for vocalizing with /u/.

1306One may posit the existence of a demonstrative particle /ha/ in proto-Ugaritic that was expanded in various ways (see above, remark to p. 229 [§42.0], etc., where the possibility is considered that the proto-definite article in Ugaritic that is written {h-} may have been this most basic particle). One would expect the simplest expansion with /-na/ to have given /hana/, but the corresponding particle in Hebrew, /hinneath/, is often taken as a basis for vocalizing with doubled /n/, viz. /ha + n(a) + na/. The vocalization of this particle with all qualifying parentheses in place would thus be /’apuhan(n)(a)/.

1307Dietrich and Loretz, Die Elfenbeininschriften (1976) 34.
likely for all of the words attested are found elsewhere in letters and the ductus belongs to the general type attested in many other administrative/epistolary texts from this large lot of tablets and fragments. One form, ‘my, ‘with/to me’, shows without doubt that the text is recording direct discourse and a second either does the same or else is in a form only attested in epistolary texts (by may either be the preposition + 1 c.s. pronominial suffix or else the expanded form of the preposition — on which see above, remark to pp. 37-38 [§21.322.5], etc.). Because idk is found only in the mythological texts and apparently once in a divinatory text (RS 18.041:21 [KTU 1.86]), because the divinatory text is not expressed in direct discourse, because RS 18.[528] does contain direct discourse, and because most of the Ugaritic texts from this lot of texts are administrative in nature, the only reasonable conclusion is that this text belongs to the latter broad genre and is more specifically epistolary. Hence several readings/restorations and analyses are possible: read simply the adverb id (attested in the letter RIH 78/12:3 [CAT 2.82]), conceivably an expansion thereof (though no such form is yet attested in prose), or consider such restorations as id≤, ‘I will know’ (presently not attested in letters), or the multiplicative morpheme id (that the latter could be separated from the number noun is proven by {®l® . •d} in RIH 78/14:12 [CAT 1.163:5]).

— p. 750-51 (§81.4b-e). T. does not discuss the second element of the particles {hln} and {hlny}, in spite of the fact that the vocalization of the longer form appears to be provided by a polyglot vocabulary entry: {al-li-ni-ya}. It would have been of interest to have T.’s views on the origin of the /i/ of the affirmative element /li/. If one accepts the syllabic writing as evidence that the /l/ of hlny was indeed geminated, it appears likely that, instead of witnessing to an independent deictic particle /hall-/ or /hal- + -l/-, the first two syllables represent the accretion of /han-/ and /-l/-, as T. proposes for the basic particle hl. On the other hand, there are no data from Ugarit for the vocalization of this basic form hl and its expanded form hlk, and the possibility must be kept open that hl was simply /hal(i)/ (viz., a variant of the particle that in Arabic became the definite article /al/) and that hlk was a direct expansion of it, vocalized /halika/. On the semantics of hlny, see above, remarks to p. 197 (§33.322.42a), etc., and to pp. 737-38 (§81.11a-d).

— p. 750-51 (§81.4b-e). In spite of having above described hlm as a “Nebenform” of hl, “nun, siehe” (p. 332 [§54.423c]), there is no reference to a form hlm in these sections on the deictic adverb hl and its expanded forms.

— pp. 750-51 (§81.4b-e). One may doubt that hlh in RS 2.002:32, 33 (KTU 1.23) consists of the deictic adverb hl and the suffixal form of the 3 f.s. pronoun, for the contextual meaning of hlh is, as T. translates, “die eine … die andere.” It appears more likely that the reference to the two distinct entities is by the repetition of the deictic particle alone (as in Hebrew kōh wā kōh, lit., ‘thus and thus’, contextually ‘in one manner, then in another’, ‘this way and that’).

1308RS 20.426B:5’ (Nougayrol, Ugaritica V [1968], text 138). On the interpretation of this entry, which the editor did not translate, see Huehnergard, Ugaritic Vocabulary (1987) 32-33, 121. Because only the Ugaritic column is preserved of this small fragment, Huehnergard’s analysis may not be considered certain, though its plausibility is indubitable.

1309On the nature and vocalization of the element /ni/, see below, remark to pp. 823-25 (§89.1).
If such be the case here, *hlh* would be another expanded form of *hl*. This is, however, one of those cases where different analyses would probably give the same vocalized form, viz., */halliha/ (on T.’s unlikely vocalization of the 3 f.s. pronominal suffix as */-hã/, see above, remark to p. 214 [§41.21], etc.).

— p. 751 (§81.4d). *hlm lnm* in RS 92.005:9 (probably to be restored in line 29 as well) (RSO XIV 49) does not mean “Siehe, hier,” but ‘Hier, siehe’. See remarks above to p. 197 (§33.322.42a), etc., and pp. 737-38 (§81.11a-d), etc.

— pp. 752-53 (§81.61c). In terms of historical phonology, it makes no more sense to say that Ugaritic *ân*, ‘where?’, derives from “’ayyânV” than to say that Hebrew *ân* derives from Hebrew *ayin*. These are simply variant forms that arose either by mutation or by particle accretion (i.e., the most basic particle may have been */á/ rather than */’ay/). If T. wishes by citing Hebrew *ân* as an etymological element to imply that the Hebrew development was identical to the one that produced the Ugaritic form, which he vocalizes as */’ân-/*, the comparison must be rejected, for the process that produced Ugaritic */’ân-/* would have produced */’ân/ in Hebrew.

— p. 753 (§81.63). For a criticism of T.’s derivation of the interrogative adverb */iy*, ‘where?’, from a base form */’ayy-/*, see above, remark to p. 171 (§33.213.1b), etc. The same basic proposal is made here for */ik/iky*, ‘how (is it that)?’, though it is tempered somewhat, that is, the form is derived directly from “’ay(yV)-kã,” which is said to be etymologically derived from a basic particle */’ayy-/*. The Ugaritic form, as well as Hebrew *’êk*, must, of course, be derived, from the form without parentheses, viz., */’ayk-/. The Hebrew and Syriac forms (Hebrew *’êk* and *’êkāb*, Syriac *’aykan* and *’aykanna*) show, by the way, that the Ugaritic particle probably consisted of */’ay/+ the expanding particle */-ka/*, that is, the second element was not the adverb *kã*, as T. holds.

— p. 754 (§81.64a). With no hesitation, T. indicates the final vowel of *lm*, ‘why?’, as */á/, then cites three forms from other Semitic languages which illustrate that the vowel must have been short, viz., Hebrew *lam(m)âb*, Syriac *’mã*, and Arabic *lima* (the */á/ in Hebrew is certainly secondary, i.e., represent the lengthening of */a/*, while the length of the vowel in Syriac may also be secondary, as is often the case in word-final position in Aramaic). As for the first vowel, T. indicates that it may have been */i/ or */a/*, but does not indicate the possibility of */ê/ (see above, remark to p. 52 [§21.341.21c], etc.).

— p. 754 (§82.11), p. 758 (§82.12). In line with observations made above (remark to p. 52 [§21.341.21c], etc.), {by} and {ly} may not be described simply as “orthogr. Variente” of {b} and {l}. It is just as plausible, if not more so, that the longer forms consist of the basic prepositions + the enclitic particle {y}. It is also highly unlikely, because it goes against the evidence of the other West-Semitic languages, that *b* was realized with a long vowel (“*[bi]*”). The case of *l*, however, is very different because it appears to have developed a secondary form */lê/ (← */lay/), more akin to Hebrew *’êlê* (← */’i/ + */ay/), than to Arabic *li/la*.

— pp. 755-81 (§82). T.’s presentation of the Ugaritic prepositions is certainly well-founded theoretically—how could I say otherwise when he accepts the basic thesis of my dissertation, namely that the prepositions in Ugaritic are principally marked for position

---

rather than for directionality? On the other hand, T. has not always distinguished as well as he might have done between prepositions in verbal clauses, where any possible directionality may have been a function of the verb/preposition combination, and nominal clauses, where the function of the preposition may be expected to have hewed more closely to its ‘original’ meaning. The principal criticism that I would make of this section, however, concerns the organization of each paragraph: the data for any given prepositional lexeme may be organized into both grammatical and lexical categories without proper distinction between the two. For example, is presented in fifteen sections set off by bullets, seven of which are grammatical in nature (from “possessivisch” to “ablativeisch”) while the other eight are defined by glosses (from “bei” to “gegen”); the two forms of categorization are interspersed according to no clear principle (i.e., “in (großer Zahl),” “wegen,” and “während” all follow “distributiv” but do not appear to be subsets thereof). Such a presentation must be judged faulty because it is confusing. Moreover the use of many detailed German glosses as an organizing principle in so serious a grammar must be judged unwise—the place for such categorizations is the dictionary, of which the intention is to offer glosses of words from one language in another, not a reference grammar. (I do not mean to imply that grammatical categories do not have broad lexical values in any given language that are more or less easily extrapolated to another language; it is the mixing of the two categories and the micro-organization of a grammar by translation value to which I am objecting.)

— p. 756 (§82.11). T. does not mention the possibility that trq + b + yd in RS 3.367 iv 13' and 20' (KTU 1.2) and yrq in lines 15' and 23' of the same text may express ‘whirling in the hand’ rather than “Springe aus der Hand.”

— p. 757 (§82.11), p. 792 (§83.141.1a). T. takes RS 24.266:12-13 (KTU 1.119) as expressing a sacrificial act in which a bull would be offered either as a ūrm-sacrifice or as a šnpt-sacrifice. An alternative division of the text is possible, however, which does away with this optional functional categorization of a single specific sacrifice, something for which I know no precise parallel in these texts. If the phrase ū ūrm ū šnpt is attached to the following series of sacrifices and if ū is identified as the coordinating conjunction (on which see below, remark to pp. 782-91 [§83.1-132]), then a double functional categorization is applied to a series of sacrifices—something for which good parallels also do not exist but which is easier to understand because of the multiple sacrifices in the following list.

— p. 758 (§82.12). One may doubt that the semantics of in the epistolary formula NGR + l šlm, attested now in RS 92.2005:7-8 (RSO XIV 49) but common in the Akkadian letters from Ras Shamra as NŠR + ana šulmān, is correctly described as an “Angabe eines Ortes” and that it is to be translated “bei, neben, in,” here “euch beide in Wohlgergeh bewahren.” The agreement in the two languages on using a preposition that commonly expresses a

---

1311 UF 8 (1976) 286-91.
notion of ‘to’ or ‘toward’ led the editors of the text to see the l here as marking a result of the divine protection: “Que les dieux en vue de (votre) bien-être vous protègent.”

One encounters here another striking use of “[n[eue] L[esung].” The reading in question is \( l \) in RS 1.009:2 (in place of \( l \) in CAT 1.46), first proposed in print in 1995. The authors of CAT have muddied the waters here by proposing in a note that the correct reading might be either \([\text{sp}]n\) or \([\text{nt}]\),1316 then asserting in a corrigendum that they really meant the latter proposal to be \([\text{nt}]\).1317

— p. 760 (§82.12). T. does not explain what would be the point of gathering dew ‘from’ barley rather than ‘for’ it (HSP \( l \) in RS 3.322+ ii 2, 6 [KTU 1.19]).1318

— p. 760 (§82.12). T. neither informs the reader that that was not the interpretation of the editors of the text1319 nor does he even mention the possibility of another interpretation.

— p. 762 (§82.13). Does German not have terms for distinguishing an equational metaphor from an explicit simile? T. remarks with regard to RS 2.[022]+ i 14-16 (KTU 1.5) and RS 24.293:24 (KTU 1.133) that “Vergleichsgrößen können auch ohne \( k(m) \) genannt werden” with no comment on the rhetorical form. In these parallel texts, the phrase npš npš lb•m thwt takes on the form of the equational metaphor: ‘My throat (is) the throat of the lion in the wasteland’.

— p. 762 (§82.13), p. 802 (§83.24c), p. 904 (§97.41). In a remark attached to the end of the section on the preposition \( k \), T. asserts that the particle \( k \) in RS 2.[009]+ ii 6, 7, 28, 29 (KTU 1.6) is to be analyzed as a conjunction, not as a preposition. §83.24c is devoted entirely to this passage which is said to contain the only case of the conjunction \( k \) “mit modaler Funktion,” but that example is said to be uncertain and the section concludes with the assertion that “die k-Lexeme sind hier jedoch eher als Präpp. zu deuten” (similar bipolar presentation on p. 904). This same conclusion was already assumed on p. 748 (§81.3c), where the particle was translated as a preposition. The distinction is only made in German

1315Pardee, BSOAS 58, p. 229. The reading was indicated in the transcriptions of texts collated by P. Bordreuil and myself that was made available both to the authors of CAT prior to its publication and to T. prior to the publication of this grammar.
1316CAT, p. 83, n. 1.
by adding “(ist)” at the end of the clause according to the analysis as a conjunction. In English the different analyses would be expressed more explicitly: ‘As (is) the heart of a cow for her calf …’ vs. ‘Like the heart of a cow for her calf …’. The absence of other cases of such a use of the conjunction in Ugaritic and perhaps elsewhere (T. cites no comparative evidence and I can think of none) makes the prepositional analysis preferable. That being the case, the category should have been eliminated with, perhaps, a note in the section on the preposition mentioning the other analysis as an outside possibility—not the contrary, as T. has chosen in fact to do.

— p. 764 (§82.31). T. rightly observes that the verb TṭB is normally followed by the preposition l in the epistolary return-of-news formula and cites two cases of TṭB ‘m (RS 15.008:19 [KTU 2.16] and RS 92.2005:13 [RSO XIV 49]) To these may be added RS 94.2479:7-10 {tnny (8) ‘m . ádty . mnm <šlm> (9) w . rgm . tṭb (10) ‘m . ‘bdh}, ‘There with my lady, whatever <is fine>, may she return word (of that) to her servant’.1321 Since in the other two examples the phrase was TṭB ‘my, ‘return (word) to me’, without an explicit nominal object, this third example is valuable in that it shows that ‘m was used with nouns as well in the absolute formulation, just as is the case with l.

— p. 764 (§82.31). It is a dangerous procedure to reason from the clear case of MḪš ‘m meaning ‘to fight against’ in RS 2.[009]+ vi 24-25 (KTU 1.6) that the preposition ‘m means “gegen” elsewhere, in particular in col. I, lines 51 and 52, of this same text, where the difficulties are legion and T.’s argument that the passage must refer to combat is not altogether convincing.1322 The presentation here appears to be based a bit too much on the German translational equivalent (contrast English ‘to fight with’, which is normally the equivalent of ‘to fight against’ but can mean ‘to fight alongside’, as in ‘X fought with Y against Z’).

— p. 767 (§82.34). The preposition ‘l does not function ablatively in RS 3.325+ vi 47 (KTU 1.16): the prepositional phrase complements tšm, ‘those who prey’,1323 not tdy, ‘you drive out’. Thus the only example of ‘l fitting such a category disappears.

— p. 770 (§82.38). Again in the interest of banning 2d person forms from the ritual texts,1324 T. prefers emendation or aberration: tnrr in RS 24.266:9 (KTU 1.119) should mean ‘oven’ (i.e., /tan(n)ûru/) rather than a verb meaning ‘to produce light’.1325

— p. 772 (§82.311), cf. p. 333 (§54.423d). T. classifies bl in RIH 78/20:7 (CAT 1.169) as a certain example of the preposition meaning ‘without’ and remarks that the terms hls and šml

---


in the same phrase are unclear, but leaves unmentioned that not everyone accepts that bl is a preposition in this text.\textsuperscript{1326} In the other mythological text cited (RS 3.322+ i 44-45 [KTU 1.19]), the thrice repeated bl is better identified as a noun or as the adverbial negative particle derived therefrom,\textsuperscript{1327} each time the subject of a nominal clause, ‘(there is) absence of dew … ’.\textsuperscript{1328} T. cites one other case of what he takes to be the preposition bl (RS 16.395:11 [KTU 4.243]), then remarks that all other occurrences of bl are to be classified as the negative particle; he singles out RS 2.[003]+ ii 37, 38 (KTU 1.14) but does not give his reasons here for that interpretation and translates the passage below with a preposition, “ohne Zahl,” but gives the literal meaning of bl spr as “der Nicht-Aufzählung” (p. 817 [§87.31b]). The interpretation of bl in RS 22.225:4-5 (KTU 1.96) as a noun appears likely (so T. p. 780 [§84.424]) because it is there preceded by the preposition l and compound prepositions are relatively rare in Ugaritic.\textsuperscript{1329} Similarly, bl in the phrase d bl spr, ‘without number’, in RS 2.[003]+ ii 37 might be more plausibly taken as the noun (‘of absence of number’) or the preposition (‘who were without number’) because it is introduced by the particle d and not followed by a preposition which would have explicitly marked the phrase as genitival (‘of which there was no number to them’ = ‘they had no number’). T. treats the phrase bl spr as the negative particle negativizing a noun (cf. phrases of the type lº≥ dªbªr in Biblical Hebrew), but the presence of the relative particle makes that interpretation dubious (in Biblical Hebrew, simple adverbial phrases with lº≥ are not introduced by the relative pronoun).

\vspace{-1cm}


\textsuperscript{1327}T.’s alternate interpretation is as a negative particle (pp. 817-18 [§87.32a]).

\textsuperscript{1328}Cf. Pardee, \textit{Context I} (1997) 351 (interpreted as a description of the drought described in the earlier narrative); Parker \textit{apud} Parker, ed., \textit{Ugaritic Narrative Poetry} (1997) 69 (the translation appears to reflect this same interpretation); Wyatt, \textit{Religious Texts} (1998) 296 (interpreted as a curse formula). The interpretation of this passage hinges on the interpretation of yšly in line 39: Pardee and Parker seek to avoid a negative polarity, translating respectively by ‘to utter a spell’ and ‘to abjure’, while Wyatt believes it means ‘to curse’ (cf. Pardee, note 95; Wyatt, p. 295, note 201); T. appears to accept the former view since he translates yšly by “anriefen” (p. 554 [§74.412.27]) and by “beschöwen” (p. 669 [§75.537d], p. 694 [§76.347]).

\textsuperscript{1329}Del Olmo Lete and Sanmartín, \textit{Diccionario I} (1996) 180, classify bl in this text as a preposition but cite the text without l!
lX bd Y in the same entry, e.g., RS 18.024:2-5, 6-7 (KTU 4.337). The fact that this text is dealing with movable goods while T. was referring to fields is insufficient basis for holding that bd and l were interchangeable when the entity in question was real estate. On the other hand, until the Ugaritic economy becomes known in more detail, it is difficult to say exactly what the difference was. In theory, when the entity in question is fields, bd should denote some kind of superintendence, l some form of proprietorship, but the precise form that each of these legal statuses took is presently unknown.

— p. 774 (§82.411). T. does not point out that his interpretation of RS 18.031:19 (KTU 2.38) as “sein (sc. des Schiffes) ganzes Getreide” reflects a n[eue] L[esung]: he transcribes kl drh whereas KTU/CAT show drhm. It is the latter reading that is correct.

— p. 775 (§82.411). The preposition in the phrase ykl bd in RS 19.015:1 (KTU 1.91) does not mark the agent of a passive verb, for the agent of the passive verb is not marked in the ancient Semitic languages and because it is preposterous to believe that a thousand liters of wine would have been used up by the person in question (perhaps {r[b khnm]}, ‘the chief priest’). Whether the verb be passive or simply stative, the preposition marks the oversight by the official in question, responsible for storing the wine and distributing it for actual consumption at the various feasts named in the first part of this text.

— p. 777 (§82.414), cf. p. 766 (§82.33). T. takes ‘d in the phrase b ‘d ‘lm in the practice letter RS 16.265:6 (KTU 5.9) as a noun that is to be vocalized ‘ād- and translated as “Dauer”; it is said to be cognate with Hebrew ‘ōd, ‘yet’, which also occurs following the preposition b. The problem with this analysis, which goes back to the editor of the text and has been adopted by a few scholars, is that, in Hebrew at least—the point of comparison generally cited!—b‘ōd means ‘while yet’, as in, e.g., Prov. 31:15 wattaqom b‘ōwd laylā, ‘she arises while it is yet night’. The epistolary formula cannot, however, mean ‘while it is yet eternity’, as is shown by simple logic and by the use of adi darīti, ‘to eternity’, in corresponding Akkadian formulae. The Ugaritic phrase must consist, therefore, either of a compound preposition (b + ‘d, ‘in + until/during’) or, as I consider more likely, of the preposition b + the common noun cognate with Hebrew ‘ad, ‘perpetuity’. This noun is

---

1331On ownership of fields by members of the royal personnel, see Pardee, Semitica 49 (1999) 19-64.
1332See the new photograph and copy in Bordreuil and Pardee, Manuel (2004), text 26 in the Choix de textes.
1334My preference in Les textes rituels (2000) 491, 494-96; see remark here above to p. 512 (§74.222.2), etc..
1335Ibid., p. 497.
1336Virolleaud, PRU II (1957) 40; Dahood, Psalms III (1970) 282 (who proposes that several Hebrew texts be revocalized in favor of ‘ōd); Ahl, Epistolary Texts (1973) 427; for Gordon, UT (1965), §10.8 (p. 96), §19.1813 (p. 453), ‘d here could be either the adverb/noun ‘ōd or the noun ‘ad, ‘perpetuity’.
1337Ahl, ibid., p. 139.
1338J. Aistleitner, Wörterbuch der ugaritischen Sprache (ed. O. Eissfeldt; Berichte über die Verhandlungen der sächsischen Akademie der Wissenschaften zu Leipzig; Philologisch-historische Klasse, Band 106, Heft
attested in poetry, but only here in prose, and its use in this artificial letter appears to constitute another element of the high-flown style that the scribe is imitating (see remark above to p. 724 [§77.325]).

— pp. 777-78 (§82.421), p. 792 (§83.141.1b), pp. 792-93 (§83.141.2a). T. glosses the compound preposition \(lp\) as “gemäß dem Wortlaut von; auf Geheiß; gemäß; nach Art von,” then uses the latter in his translation of RS 1.002:19'-21' (KTU 1.40) where the idiom is HT⁻, ‘to sin’, + \(\dot{u} \dot{l} p\) and of lines 28'-32' of the same text, where the idiom is \(\dot{s}n \ ypkm\), ‘your “beauty” changes’, + \(\dot{u} lp\). He comments that the prepositional phrase may also be interpreted as “gegenüber” but provides no indication of what he believes the socio-religious function of the text would be if the Ugaritians are in it described as sinning or of seeing their well-being disintegrate ‘in the manner of’ various ethnic groups with whom they had regular contact; nor does he explain how ‘against’ would work with the second idiom. For a global interpretation based on the an interpretation of \(lp\) as meaning ‘according to the declaration of’, which T. only includes among his glosses, not as a real interpretational possibility, see my studies of the text.¹³³⁹

— p. 778 (§82.422). By the principle of ‘a single swallow does not a summer make’, the single occurrence of the noun \(pnm\) spelled with \(\{w\}\) in the pronominal state (\(pnwh\) in RS 2,[014]* i 6 [KTU 1.3]) is insufficient to prove that this noun (and the related verb meaning ‘to turn’) were derived from a III-\(w\) root, as T. assumes here and above, p. 204 (§33.443). As for the suffixal form of the noun written with two \(\{n\}\)s (\{\(pnnh\}\) in RS 2,[014]* iv 40 [KTU 1.3] and in RS 3.362+ ii 17* [KTU 1.10]), T. offers no explanation for his vocalization “\(pani-nnVhu\).” The first and perhaps most obvious remark: as has been observed many times above, the vowel /\(\i\)/ would not have retained its length in a closed syllable and the actual Ugaritic form would perforce have been /\(pannnVhu/\). But even more important: what is the reason for the gemination of the /\(n\)/? This looks for all the world like a suffix on a verb (i.e., /-hu/-ha/ attached to the energic form ending in a vowel, e.g., /-annahu/), not the suffix appropriate for nouns (where the suffix follows immediately on the case vowel). More likely than the hypothesis according to which suffixes appropriate for verbs were occasionally extrapolated to nouns is the idea that compound prepositions, just like simple prepositions, showed forms expanded with \{-\(n\}\}.¹³⁴⁰ Above, pp. 221 and 223 (§41.221.51, §41.221.61), T. refers to \{\(nh\}\) on \(pnnh\) as the “sogenannte \(n\)-Variante des P[ronominal]s[uffix]” whereas here he refers to “\(-n\) erweiterte Formen” of the prepositions; he provides here a cross-reference to §41.221.51, .61, but the examples cited on p. 781 are only of primary prepositions (\(b, l,\) and \(\dot{m}\)). It appears necessary to adopt the view that the \{\(n\}\} is an expanding element on the preposition, not a part of an expanded pronominal suffix;

---


¹³⁴⁰ The signs \(\{bdnhm\}\) in RS 18.031:18 (KTU 2.38) were first explained by Hoftijzer as representing the compound preposition \(bd\) (\(\dot{\sim} bi + yadi/\)) plus the expanding element \(-n\) plus the 3 m.pl. pronominal suffix \((UF 11 [1979] 387 with note 37).
l pnnh shows that compound prepositions had forms with expanding -n just like primary prepositions. The phrase l pnnh is, therefore, to be vocalized with the -n expanding particle, and there is no reason to expect that -n to have been geminated. The expected vocalization is, therefore, something like /lê panînahu/.

— pp. 782-91 (§83.1-132). T. makes no place in this section on coordinating conjunctions for ū, to be vocalized /’ū/ and distinguished from the disjunctive conjunction, also written {ū}, but vocalized /’ô/ ← /*ôw/.1341 For specific comments on instances of /’ū/, see remark above to p. 757 (§81.11), etc., and below to p. 792 (§83.141.1a). The origin of the conjunction is obscure because one cannot cite for it rules of the relatively simple type that account for Akkadian u or Hebrew/Aramaic {w} pronounced as /û/. Nevertheless, I am not convinced by the proposal of del Olmo Lete and Sanmartín to see it as a semantic variant of disjunctive /’ô/ which would have had a conjunctive function.1342

— p. 782 (§83.1). One should perhaps not lodge too vigorous a complaint against T.’s formulation when he says that the coordinating conjunctions “dienen zum Ausdruck” of various “Beziehungen” (he names copulative, adversative, disjunctive, and causal)—though “Ausdruck” is not the best term since the conjunction does not ‘express’ the ‘relationship’, it only links two phrases that happen to have this or that relationship. In a remark to p. 786 (§83.113d), however, the terminology is even less felicitous: “Zu einer vergleichbaren Funktion [the reference is to the adversative relationship] der Konj. ū in den Amarnabriefen ….” The particle itself has neither different vocalizations according to its different translation values nor different functions defined syntactically (as, for example, with certain particles that introduce morphologically distinct phrases, such as particles that function both as prepositions, introducing a noun, and as conjunctions, introducing a finite verb); its single function is explicitly to mark a link between various morpho-syntactic units (words, phrases, clauses, and on up the ladder to long literary units). These units will have different logical relationships that require different translations of the conjunction in languages where such relationships are expressed explicitly at the lexical level. At the most basic level, first-year students of Biblical Hebrew, for example, have to learn that the conjunction w may be translated either as ‘and’ or as ‘but’ (among others) but that these are not different manifestations of w, only a reflection of the fact that Biblical Hebrew does not have a commonly used adversative particle; the various translations reflect, not different meanings or even different functions of a single w, but the logical relationships between clauses as expressed in a given modern language. It does not appear to be too much to ask to find this basic fact of grammar/lexicography reflected in a reference grammar such as this.

— p. 782 (§83.11). T. cites the Hebrew forms of the conjunction cognate to Ugaritic w as “we” and “wa”; to these should be added wā (as in tôhūw wā bōhūw), not an unimportant

1341 For the instances of coordinating /’ū/ in the ritual texts, see Pardee, Les textes rituels (2000) 1103 and commentaries to the texts cited there.

alloform since it provides a datum for the proto-form being /wa/. As earlier evidence, it would have been appropriate also to cite Eblaite wa.1343 — p. 784 (§83.112a). Whether the w of the phrase w hl, ‘and (X-category) is pure, i.e., cultically unrestricted’, common in the ritual texts, belongs properly to the category of ‘pleonastic’ wāw depends on whether hl is verbal or not. If both this phrase and the preceding are nominal phrases, ‘(there is) setting of the sun and the king (is) pure (/wa ḫallu/) or ‘and (there is) purity of the king’ (/wa ḫullu/),1344 then the two clauses are on the same syntactic level and the use of the w is not peculiar.1345 If, on the other hand, hl is verbal (/wa ḫalla/), as is certainly plausible,1346 then the ‘wāw-consecutive’ structure following a nominal clause fits the category under discussion in this section, i.e., the translation is ‘(At) the setting of the sun, the king becomes pure’.

— p. 784 (§83.112c). T.’s classification of km ḡlmm w ‘rbn (RS 2.[014]+ iii 8'-9' [KTU 1.3]) as containing an example of ‘pleonastic wāw’ is certainly valid, but doubts arise regarding his interpretation, “Wie (zwei) Pagen * * tretet fürwahr ein!” (the two asterisks stand for the untranslated w). The persons addressed are two pages; what would be the force of the command to enter ‘like two pages’? It appears better to take km as an adverb meaning ‘so, at that moment, then’ or the like.1347 It is perhaps distinct from ḫāma, ‘thus’, consisting simply of the preposition plus enclitic -m (/kama/).

— p. 785 (§83.112e). T.’s doubts at taking the w of ‘bdk Ḥn w d ‘lmk as ‘pleonastic’ appear well founded, for the second clause appears to be simply an expansion of the predicate of the first, lit. ‘I am your servant, (I am) the one of your eternity’. His first classification, as ‘explicative wāw’ (p. 783 [§83.111d]) is, therefore, preferable.


1345 In his definition of the category, T. refers to only one specific syntactic incongruity, the so-called ‘hanging clause’ (“pendierendes Satzglied”); his two over-arching categories are “ein topikalisiertes … Satzglied” and “ein nachgestelltes Satzglied”; in none of these is the w to be translated in German or “in anderen indoeuropäischen Sprachen.” What characterizes all of these examples of “w zur Verbindung von Satzteilen,” however, is that the morpho-syntax of the clause or phrase introduced by w is different from that of the preceding clause or phrase. Most examples consist of the well-known structure consisting of a prepositional phrase + w + verb, e.g., ‘in his lifting his eyes and he sees’ = ‘when he lifts his eyes, he sees’. But other categories are represented, e.g., a direct object followed by w plus a prepositional phrase: št ālp qdmnh // nriâ w tk phn, ‘he puts a bovid before him, a fatling in his presence’, lit., ‘he puts a bovid before him, (he puts) a fatling and (it is) in the midst of his face’ (RS 2,[014]+ iv 41-42 [KTU 1.3]).

1346 On p. 675 ([§75.64], T. parses the form as /QTLa/, but he also indicates in parentheses the alternative analysis as an adjective.

zur Kenntnis nehmen." The absence of a word-divider after {b‘ly} and the presence of {y} on a noun that would be in the nominative case (both attested but nevertheless minority usages) indicate that b‘lyska may be a personal name.1348

— pp. 787-88 (§83.115a). In this section entitled “Kopulative Konstruktionen ohne w," one encounters only one putative example of two nomina recta following a nomen rectum: T. interprets um phil philt in RS 24.244:1 (KTU 1.100) as meaning "die Mutter des Hengstes (und) der Stute." He does not mention the fact that the standard interpretation is to take philt as modifying um appositionally, rather than genitivally: ‘The mother of the stallion, the mare …’ (/‘ummu paḥši pahlatu/ rather than /‘ummu paḥši paḥlati/).1349

— p. 788 (§83.12), p. 790 (§83.123b), p. 832 (§89.29). In this section entitled "Kopulative Konstruktionen ohne w," one encounters only one putative example of two nomina recta following a nomen rectum: T. interprets um phil philt in RS 24.244:1 (KTU 1.100) as meaning "die Mutter des Hengstes (und) der Stute." He does not mention the fact that the standard interpretation is to take philt as modifying um appositionally, rather than genitivally: ‘The mother of the stallion, the mare …’ (/‘ummu paḥši pahlatu/ rather than /‘ummu paḥši paḥlati/).1349

— p. 791 (§83.131), pp. 807-8 (§85.2). Not a single example cited on p. 791 requires the analysis of áp as a coordinating conjunction, in spite of T.’s claim that he is citing only examples in which áp “eindeutig kopulativ gebraucht wird.” Once one is willing to admit that the particle can function as an adverb (which is what T. essentially does in §85.2, where usages of áp as an “Affirmationspartikel” are proposed),1350 all examples may be identified as an adverb which, because of its etymology (the conjunction pa + prosthetic ‘alif), marks a logical relationship between what precedes and what follows. For example, in RS 15.098:8-10 (KTU 2.17:1-3) {l yblt . ḫbtm (9) áp ksphm (10) l yblt}, which T. translates “Du hast keine Ḥubtu-Truppen gebracht. Und auch ihren Sold hast du nicht gebracht,” may just as well be translated ‘You haven’t brought the Ḥubtu-men; what’s more, as regards their money, you haven’t brought that either.’ T., following most commentators, takes áp in RS 2.[024] i 12 (KTU 1.22) {ṭŋt . ḥlm . áp ṣín} as coordinating the two elements of a compound object phrase: “Er schlachtete sowohl Rinder als auch Kleinvieh.” The following division of the

1348So, most recently, van Soldt, UF 33 (2001) 581 n. 22.


1350Like T., Gordon ascribes both functions to the particle (UT [1965] 104 [§11.10] 106 [§12.1]), as do del Olmo Lete and Sammartín (Diccionario I [1996] 42-43). Both Segert, Basic Grammar (1984) 80 (§57.22), 134 (§83.3), and Rainey, Or 56 (1987) 401, parse áp as a conjunction in RS 18.075:21 (KTU 2.41:20), which is certainly incorrect (the phrase is w áp anik, ‘and I, for my part …’).
passage, however, provides good semantic and quantitative parallelism (only the syllable count is indicated).1351

<table>
<thead>
<tr>
<th>Syllable Count</th>
<th>Verb Form</th>
<th>Translation</th>
</tr>
</thead>
<tbody>
<tr>
<td>7</td>
<td>/tablet a `alapîma/</td>
<td>verb + A</td>
</tr>
<tr>
<td>7</td>
<td>/apa ša`na šaqîla/</td>
<td>B + verb</td>
</tr>
<tr>
<td>10</td>
<td>/tôrîma wa mari<code>i </code>êlîma/</td>
<td>A' + B'</td>
</tr>
<tr>
<td>9</td>
<td>/agalîma dîta šanati/</td>
<td>A''</td>
</tr>
<tr>
<td>9</td>
<td>/immiri qumši lali`ima/</td>
<td>B''</td>
</tr>
</tbody>
</table>

If this analysis be accepted, it eliminates the only example of the use of wa as a copula linking noun phrases that is cited by any of the authorities that I have consulted. The frequency with which ap follows the coordinating conjunction w and with which it precedes an independent pronoun (e.g., w ap anîk, ‘and I for my part’) in both Ugaritic1353 and Hebrew makes the identification as a coordinating conjunction dubious.1354 Finally, in both paragraphs cited, T. indicates the vocalization /apV/,1355 for which I can see no basis. T. does not propose a specific derivation, but I see no reason to doubt the etymology indicated here above, i.e., prosthetic /a/ attached to the conjunction p, analogical to id, ‘then’ (/a/ + the relative/determinative pronoun /dl/), or to âl, ‘not’ (/a/ + the negative particle /l/). That derivation indicates the vocalization /apa/—at least until explicit data confirm or disprove it. — pp. 792-93 (§83.141). T. classifies the two examples of íy (RS 1.013+;13 [KTU 2.3] and RS 88.2159:21 [RSO XIV 51]) under the only word he recognizes behind the writing {ü}, viz., the disjunctive conjunction. In both passages where the particle occurs, however, the context is too damaged to permit distinguishing between coordinating /ü/ (on which see above, remark to pp. 782-91) and disjunctive /êü/.

— p. 792 (§83.141.1a). T. includes the phrase `mîtmr mlk … ù nqmd mlk (RS 34.126:11-12, 25-26) under disjunctive ù but translates it “auch(?)”, i.e., conjunctivally.1356 Since the function of the particle in these passages is clearly not disjunctive (the reference is to ‘Amîttamru and Niqmaddu’, not to ‘Amîttamru or Niqmaddu’), it is better to admit the existence of a coordinating conjunction of which the vocalization may be /ü/.1357

---

1351 On the place of the various parallelistic devices in Ugaritic and Hebrew poetry as I understand them, see Pardee, Ugaritic and Hebrew Poetic Parallelism. A Trial Cut (‘nt I and Proverbs 2) (SVT 39; 1988).
1352 This formulaic list is found also in RS 2.[008]+ vi 40-43 (KTU 1.4) and, largely restored, in RS 3.361 iv 30-32 (KTU 1.1).
1353 T. includes w ap in RS 88.2159:9 (RSO XIV 51) among his “eindeutig” examples of ap functioning as a conjunction, but no others; the context of w ap in this text is badly damaged, but the adverb is certainly not followed by an independent pronoun (the signs are [ût[…]}).
1354 In Hebrew, the status of the particle is clouded by its association with ki to form a compound conjunction `ap ki, which may or may not represent an original conjunctival function for ap.
1355 Rainey, Or 56 (1987) 401, gives /apV/, with no explanation.
1356 His alternative analysis, as the interjection ù, will not stand up for two reasons: (1) such a particle is as yet not certainly attested in Ugaritic (see remark below to p. 805 [§84.22]) and (2) such a translation of RS 34.126 (p. 805: “Gerufen ist — ach! — der König Niqmaddu”) does not fit the rhetoric of RS 34.126, in particular ll. 25-26, where ù nqmd appears at the end of a long list.
It is debatable whether half a page should have been devoted to the possibility that ūr functioned in Ugaritic as a conjunction when no certain example exists whereas certain examples of the functions as an adverb and as a preposition do exist. Cross-references to the sections dealing with the latter categories would have been sufficient. (The ambiguity exists, of course, only because our texts are unvocalized: the Ugaritians would have distinguished clearly between, say, an adverb followed by 3 m.s. /QTLa/, ‘thereafter he arrived’, and a preposition followed by the infinitive of the verb, ‘after the arrival of…’.)

— p. 796 (§83.211). T.’s argument that id must be a subordinating conjunction because it may appear at the beginning of a text is insufficient to prove the point because the particle is attested to date primarily in ritual texts. In this genre, the text borne by a given tablet is not always independent of other ritual events, i.e., the text on a given tablet may begin in medias res. The fact that k is the common conjunction for introducing subordinate temporal clauses may be adduced as a (relatively weak) argument for classifying id as an adverb. But, until a text is discovered in which one or the other of these analyses is clear, the question must remain open.1358

— p. 797 (§83.213). T. devotes half a page to the proposition that hlm is a subordinating conjunction, then a long remark showing that such is not necessarily the case. In fact there is no reason, etymological or contextual, for believing that this particle ever functioned as a subordinating conjunction. hlm is simply an expansion of hl and, unless clear evidence of conjunctival usage exists, there is no reason to see it as anything but an adverbial particle of presentation, perhaps with a temporal connotation along the lines of ‘then, next’. One may note here that hlm is followed directly by the verb in only one text (RS 24.244:6 et passim [KTU 1.100]), the only one of his list to which he attaches the question mark. This word order is not, of course, required of conjunctival usage, but it does occur frequently. Presentative particles, on the other hand, often precede a topicalized noun in the phrase, often the subject.

— p. 798 (§83.221). I see no reason to set up a separate grammatical category for ālr in the phrase {fāldm (34) ālr . it . bqt (35) w . imson . ly}, ‘A man, wherever he might be, seek out and dispatch to me’ (RS 18.038:33-35 [KTU 2.39]). T. does so and entitles it “Lokale Konjunktionen” but the section contains this example only and, furthermore, it contains the correct analysis of the form as a common noun (“Subst. afr ‘Ort, Stelle’ in einem adverbialen Kasus”). The correct grammatical analysis was argued in detail by Rainey as early as 1966.1359 One cannot posit the passage from one grammatical category to another on the basis of a single example.

— p. 800 (§83.232d). T. follows KTU/CAT in reading {im} as the last signs of the line in RS 1.024 B 3 (KTU 4.17:3). Traces of another sign appear after these two, however, and then the tablet is broken off. Though the traces are not clear, the sign in question is certainly

1358 See my more detailed discussion, with bibliography on the two analyses, in Les textes rituels (2000) 482-83.
1359 In Hebrew in the journal Lešonenu 30, pp. 250-72; see the English version in UF 3 (1971) 151-72, esp. 160-62.
not the word-divider, and this text does not, therefore, contain the conjunction *im*. Read perhaps {imr'l}, viz., the same word as appears in line 1.

— p. 800 (§83.24). As remarked above (remark to p. 52 [§21.341.21c], etc.), {ky} may not be just an “orthogr. Variante” of the conjunction *k*, but a phonetic one, i.e., the regular form expanded by enclitic -y, hence /kīya/.

— p. 802 (§83.24d), p. 822 (§88.22b). The first paragraph is unnecessarily devoted to a single example of the conjunction *k(y)*, said to introduce a “Kausalsatz (‘da, weil’),” in RS 18.038:19 (KTU 2.39). In the passage in question, the second ky does not mark the reason for the previous assertion but the content of the message: *w lḥt ākl ky likt ‘m špš b’lk ky ākl b ḫwtk inn špšn tǔbd […]* ‘Now regarding the fact that you sent a message to the Sun to the effect that there is no food in your land, the Sun is perishing…’.

As the -n of apodosis on špš shows, the protasis of this reference to a preceding letter extends through ākl b ḫwtk inn and, as recent commentators have seen, the passage does not refer to a request for food from the king of Ugarit to the Hittite emperor but to a refusal on the part of the Ugaritic king to furnish food to his sovereign, refusal based on the claim that he has no food to send to Hatti.

— pp. 802-3 (§83.24e). T. accepts my views on the meaning and function of *k(y)* in the epistolary formula of the type *lḥt X k(y) likt*, ‘As concerns the fact that you sent a message regarding X’, lit. ‘the tablet of X, concerning the fact that you sent (it)’. (It was previously thought by many that the particle *k(y)* in this structure was to be analyzed as a relative pronoun rather than as a subordinating conjunction.) But nowhere does he outline how the *k(y)* subordinate clause is linked to the following main clause (that is done by p, ht, and Ø); nor does he deal specifically with the topic of a second subordinate clause inserted between the first *k(y)* clause and the main clause (on his misunderstanding of one of these, see preceding note).

— p. 804 (§84.12). *I* in lines 1, 4, and 19 of the first of the Arslan Tash incantations is not vocative (the reading of line 1 indicated by T. is incorrect). There is, therefore, no attestation of a vocative *I* elsewhere in Northwest Semitic (on the similar use of Arabic /la/li/, see above, remark to p. 315 [§54.214b], etc.).

— p. 805 (§84.22). There is something wrong with a particle that is supposed to be phonetically indistinguishable from the conjunction meaning ‘or’ (both, according to T., were pronounced /š/ ← /’aw/), that is supposed to mean both “ach!” and “wehe!,” and regarding which, in usage, it is possible to say “Es ist schwierig, eine genaue Grenzlinie zwischen der Ausrufpart. *u* und der Konj. *u* ‹oder› … zu ziehen.” There is in fact no convincing example of


1363*BiOr* 34 (1977) 7-8.

1364On these matters, see the chart in *BiOr* 34 (1977) 8.

the emphatic particle (see remark above to p. 196 [§33.322.3c] and the remark in note 741 on ùzb in RS 92.2014:1), and it should be removed from future editions of this grammar. T.’s own ambivalence is revealed by the fact that he translates ù in RS 3.343+ iii 29’ (KTU 1.15) and in RS 3.325+ i 4 (KTU 1.16) by “oder” on p. 793 but here by “Wehe!” As we saw above, remark to p. 792 (§83.141.1a), T. accepts implicitly that the particle /ô/ can also function conjunctively (on this view, proposed by del Olmo Lete and Sanmartín, see remark to pp. 782-91 [§83.1-132]).

— p. 809 (§85.5). As was noted above (remarks to p. 88 [§31.3], etc.), ădn in RS 18.038:6, 8 (KTU 2.39) means ‘father’ not ‘lord’.1366

— p. 809 (§85.5). T. suggests that {ûky} in RS 16.078+:5 (KTU 2.23) may consist of the emphatic particle ūk + enclitic -y. It may also consist of the conjunction ū (see above, remark to pp. 782-91 [§83.1-132]) plus ky; the latter element may consist either of the well-attested conjunction plus enclitic -y (/kìyà/) or of the adverb plus the enclitic particle (/kªya/).

— p. 809 (§85.7). T. remarks concerning the so-called ‘emphatic k’ that “Die Abgrenzung von k mit affirmativer (‘emphatischer’) Bedeutung von der kausal-koordinierenden Konj. k ist—inbesondere in satzeinleitender Position—schwierig.” If one accepts, as does T., that this particle is in fact phonetically identical to the conjunction and if one remarks the many examples of kì in Biblical Hebrew serving to link only very loosely one utterance to another, it appears necessary to accept that the ‘emphatic’ particle was extremely frequent in Biblical Hebrew, both in prose and in poetry but especially in the latter. An alternative view would be that there was no distinct emphatic particle in either language, that the coordinating conjunction could be used to link utterances with a looser form of causality than our modern languages would lead us to expect. This would mean that the clear cases of ‘emphatic’ k in Ugaritic, viz., where it appears before a verb that is dislocated to the right in its clause (i.e., the particle and the verb are not in first position in the clause), do not contain a distinct particle but the coordinating conjunction placed in an abnormal, i.e., emphatic or highly marked, position. It would, if there is anything to this view, be the syntax/word order that creates the emphasis, not the use of a lexically distinct particle. In any case, none of T.’s three examples of ‘emphatic’ k in first position in an utterance is particularly convincing, especially not the one example from prose: k in RS 18.038:7 (KTU 2.39) clearly introduces an explicitation of the previous statement and is translatable in standard American English by ‘for’.1367 As regards the examples from poetry, little confidence is inspired when he describes them here as “relativ sichere Belege” after he has already provided a first classification of one of them as causal (p. 785 [§83.113b] on RS 3.362+ iii 35’).

— p. 810 (§§85.7b), p. 812 (§§85.8b), p. 884 (§94.23). T. proposes somewhat hesitantly that RS 29.093 (CAT 2.70) would contain two examples of ‘emphatic’ k, but neither should have been cited here. I have already dealt with his treatment of line 23 above (remark to p. 448 [§73.243.1], etc.). A different division of the signs in line 22 from T.’s (k l ttn åkl lhm),

1367Ibid.
sollst du ihnen fürwahr(?) zu essen geben") permits a very different interpretation: one may divide \textit{tmt ‘mnk kl t ln äkl lh}m and translate along the lines of ‘there with you is all (one could need): give food to them!’ (or: \textit{kl ttn, ‘is all: you must give’}).\footnote{Ibid., p. 111. T.’s complete translation of lines 20-22 is “Und was deine beiden Diener betrifft, die dort bei dir sind, (so) sollst du ihnen fürwahr(?) zu essen geben,” but there is no relative pronoun before \textit{tmt} and “die” should, therefore, have been in parentheses in the translation. The absence of the relative pronoun makes possible the epigraphic and syntactic divisions that I am proposing, for the entire ‘hanging nominal clause’ may consist of \textit{w tm ‘bdk} in line 20.}

There are, therefore, no clear examples of ‘emphatic’ \textit{k} in prose, whether that entity be described lexically or syntactically.

— p. 810 (§85.7c). With the reading of the particle in RS 22.225:2 (\textit{KTU} 1.96) as \textit{k} rather than \textit{w} (see remark above to p. 597 [§74.624], etc.) and with the analysis of \textit{tp} in the same line as verbal, meaning ‘she/it sees’ (see remark above to p. 199 [§33.323.2b], etc.), this example of ‘emphatic’ \textit{k} disappears. The function of the \textit{k} here is to introduce the object clause expressing what is seen.\footnote{Pardee, \textit{Ritual and Cult} (2002) 162.}

— p. 811 (§85.8aa). Understanding that \textit{ādn} means ‘father’ not ‘lord’ (see remarks above to p. 88 [§31.3], etc., p. 250 [§51.3h], etc., and p. 809 [§85.5]) would have enabled T. to see that the \textit{l} in line 9 could not be emphatic: the line must mean ‘my father never lacked grain’\footnote{Idem, \textit{Context III} (2002) 111.}. It could not mean ‘my father always lacks grain’ because the father of the reigning Hittite monarch is dead.

— p. 816 (§87.2). In his discussion of the etymology of negative \textit{‘āl}, T. does not mention the possibility that the form may have arisen historically by prefixation of ‘/‘ to negative \textit{l}. Similar forms can be cited (for some examples, see remark above to p. 791 [§83.131], etc.) but this one appears particularly appealing because of the basically identical semantics of the two forms, i.e., they are both negative particles of which the distribution reflects mood.

— p. 816 (§87.21), p. 883 (§94.21). In the first section cited, T. translates \textit{w b’ly bt ‘bdh ‘āl yb’r b ydh} in RS 92.2010:21-22 (\textit{RSO} XIV 50) as a simple sentence (“Und mein Herr soll das Haus seines Dieners nicht mit seiner (eigenen) Hand zerstören”) but in the second as cleft (“Und was meinen Herrn betrifft - Das Haus seines Dieners soll er nicht mit seiner (eigenen) Hand zerstören”). I can only hope that in the latter case he was not influenced by the editors’ translation (“Et, mon maître, quant à la maison de (22) son serviteur, qu’il ne (23) la détruis pas (24) de sa main”\footnote{Bordreuil and Pardee, \textit{RSO} XIV (2001) 376.}) which was only an accommodation to the physical order of the sentence components on the tablet, not a statement regarding morpho-syntax. However that may be, neither the fronting of the object phrase nor the writing \{b’ly\} qualify as indicators of a true cleft sentence.

— p. 820 (§88.1c). T.’s argument that \textit{iṭ} in RS 29.093:29 (\textit{CAT} 2.70) may be verbal\footnote{On the ambiguity of the textual data cited by T. in favor of \textit{iṭ} having become a true verb in Ugaritic, see remark above to p. 463 (§73.313), etc.} because of similarities with the construction \textit{mm m ḥṣrt} in RS 18.075:20’-21’ (\textit{KTU} 2.41:19-}
founders on the fact that $\text{hsrt}$ in the second text is probably nominal rather than verbal (see remark above to p. 52 [$\S$21.341.21a], etc.). T.’s position on $\text{hsrt}$ is ambivalent in that he never parses it and treats the syntactic construction in which it is found alongside others where the corresponding element is indubitably nominal (p. 902 [$\S$97.23a]); the most striking example is $\text{mmn irštik}$ in line 17’ of this very text. But his unambiguous statement here on p. 820 can at least be taken at face value as indicating that he takes $\text{hsrt}$ as a verb. The analysis as a noun (‘whatever my lack [may be]’) is based on the nominal structure in line 17’ of this text and on a clear parallel in an Akkadian text from Ras Shamra: RS 17.116:24-25’ ($\text{PRU IV}$, p. 133) reads $\{\text{ma-an-nu-me-e (25’) ši-bu-te-ka a-na maľ-ḫḫi-ia (26’) [ša t]a-šap-pár-ra a-na-di-na-ak-ku}\}$, translated by Nougayrol as “Tout désir [que tu] m’exprimeras, je le satisferai pour toi.”

---

T. goes far beyond any of his predecessors in his treatment of enclitic particles, both in quality and in quantity (numbers of citations and variety of distributions of the particles). Comparing his work with some of the muddled presentations of earlier decades, especially by those whose only real interest was in Biblical Hebrew, is like comparing day with night. My negative reactions here are few. One of the most important is to the fact that T. provides very little space here for the interpretation of various tokens of {-y} attached to particles as the enclitic particle, having preferred almost exclusively the interpretation as a mater lectionis (see above, remark to pp. 37-38 [$\S$21.322.5], etc.).

---

It is difficult to see on what basis -h- and -d- are excluded from this presentation of the enclitic particles. It would appear to be on semantic/functional grounds: one of the functions of -h- is much more clearly definable than are any of the particles listed here, viz. when attached to a substantive as adverbal marking place, direction, or manner, while d also exists as an independent particle, viz., the determinative/relative pronoun. But are these grounds for excluding them when they are formally enclitic, i.e., attached to other nouns or particles and never separated therefrom by the word-dividing small vertical wedge when the particle in question is affixed? (The latter feature is regular among monoconsonantal particles: while such particles that precede the word they modify are often separated therefrom by the word-divider, such is never the case of the particles that follow the word to which they are attached.) In his treatment, T. proceeds from the productive particles, those that could in theory be attached to any other word (-n, -m, and -y), to those which are known only as formative elements of other particles (-k- and -t-). -d-, in its capacity as a particle expander, clearly belongs to the latter category, while the distribution of -h- is more complex, for it appears as the adverbalizing affix -h, as the root particle of other more complex particles (h-, hn, hl, etc.), and as a particle expander itself (mh, mhk, mhkm, etc.).

---

1374 The major exception is K. Aartun’s Die Partikeln des Ugaritischen (AOAT 21; 1974, 1978) to whose views on this matter I have come increasingly closer over the years since they first appeared in print.
In his treatment of the “enklitische Partikel -n” T. considers only cases where [-n] is actually attached to the end of a word; he vocalizes it “-/Vnナル/” and considers it to be etymologically identical to the energetic verbal morpheme. Nowhere does he deal with the problem of the relationship between this particle and the expanding particle /n/ which shows up as an element in several compound particles. One of these particles, hlny, is attested in a polyglot vocabulary spelled {al-li-ni-ya} which T., following Huehnergard,1376 vocalizes /hallinいや/, without discussing the element /nī/. Is the /i/ correct here and does this form mean that the [-n-] always has /i/ as its vowel? It appears necessary to respond negatively to both questions. Whether or not [-n] is the same element as the energetic particle, a hypothesis that does not appear implausible and which T. accepts provisionally (p. 823), there is every reason to believe that it is cognate with the /n/ of nominal nunation, which in Arabic is /-na/ and which was certainly not /-nī/ in proto-Aramaic since the vowel has disappeared in the attested Aramaic dialects. If one be willing to admit that the expanding particle [-n-] and the enclitic particle [-n] are etymologically identical—and I can see no reason not to do so—it appears necessary to conclude that the /i/ of [al-li-ni-ya] is secondary, having arisen under the influence of the following /y/ or of the preceding /li/, or of both. It is, therefore, probably short, though the /y/ may have been geminated: /halliniya/ or /halliniyya/. It is probably best, however, to represent the proto-form of the particle as /-n(a)-/, since the vowel of the deictic and expanding particles does not appear always to have been stable.

Given the many uncertainties of the ‘energetic’ elements of the verbal system (see remarks above to pp. 222-23 [§41.221.52c], etc., and to pp. 497-506 [§73.6], etc.), it appears a bit rash to define enclitic -n as serving “zur Hervorhebung von nicht-verbalen Wortarten.” Not only may the principal energetic morpheme itself be historically identical to ‘enclitic’ -n (as T. himself observes here), but saying that no final -n on any verb form may be enclitic rather than energetic appears to go beyond that which is firmly supported by the data at hand.

In this listing of the enclitic particle -n “nach topikalisiertem (betont vorangestelltem) Satzglied,” T. includes three prepositional forms ending in -n: bn, ln, and ‘mn. This raises the question as to whether these alloforms of the prepositions should be simply listed alongside nouns to which the enclitic morpheme has been added ad hoc. This is particularly true of ‘mn, which is a very frequently attested alloform, especially in prose and especially before the 1 c.s. pronominal suffix. An attempt should be made to distinguish between the productive use of the enclitic particle and frozen forms of other particles which arose at some time in the past by affixation of the etymologically identical particle. What is interesting is that, in the case of prepositions, one encounters what appear to be very rarely attested forms with [-n-] inserted between the lexical stem and a pronominal suffix (on the case of {b dînh˘m} in RS 18.031:18 [KTU 2.38], see remark above to p. 214 [§41.21], etc.)—were these frozen forms that happen to be attested rarely or do they attest to the productivity of the [-n] even between lexical and pronominal morphemes?

There is a particularly egregious bit of circular reasoning here: it is concluded that a Ugaritic noun with enclitic -m probably showed geminated /m/, e.g., /'aršumma/, 'to the earth', because that is the common form in Akkadian; the conclusion is then drawn that nouns in the singular must once ("ursprünglich") have had mimation in the singular, as in Akkadian. (According to the systems as attested, Akkadian shows -m in the singular but not in the plural, while the West-Semitic system is the opposite, e.g., Akk. šarrum/šarrû vs. West-Semitic šarru/šarrûma.)

On p. 841 (§91.24) T. states that the placement of an attributive adjective before the noun it modifies occurs only in poetry, but here he cites the prose text RS 17.031:11 (KTU 4.296), where one finds the phrase ʾāḥd ʾālp, 'one bovid'. This is probably, however, not a simple case of an attributive adjective preceding the noun it modifies but a bit of book-keeping shorthand: the previous paragraph began ʾilt ʾālp, 'three bovids', and, though the beginnings of the rest of the entries in this list are missing, it appears likely that the scribe was simply placing the number word in first position, irrespective of whether it was a noun or an adjective, viz., that the number word was being put conceptually in a separate column in spite of the fact that the text is not arranged in columns. Above, p. 388 (§69.112), two other texts are cited where this placement of ʾāḥd occurs and each time the entry with ʾāḥd is immediately preceded by another entry headed by a cardinal number noun.

On p. 828, the -m} of †hrm in the phrase bht †hrm iqnîm, ‘a mansion of purest lapiz-lazuli’ (RS 2.[008] v 19, 34-35 [KTU 1.4]) is said to be enclitic whereas, on p. 842, the reader is urged to note the gender and number agreement between the adjective †hrm and the noun iqnîm. Whether †hrm be in construct or appositional to iqnîm, I see no reason to doubt that it is, morpho-syntactically, a substantivized adjective rather than a simple attributive adjective. (On p. 841 [§91.24], T. avers that not all examples to be cited in the following paragraphs belong to this latter category, but he does not say why that should be the case; on my principal reason for disagreeing, viz., that the placement of an attributive adjective before the noun it modifies is a late phenomenon in West Semitic, see remark above to p. 419 [§69.43], etc.; the other important reason is that one sometimes finds gender disagreement between the two elements, which is a clear indication that the ‘adjective’ is not modifying the following noun in a simple attributive way.)

T. presents the phrase w n̄lh̄m that appears in administrative texts after w n̄lh, ‘and his heir’, as bearing the 3 m.s. pronominal suffix and enclitic -m (‘and his heir’, viz., that of the preceding heir). He makes no mention of the possibility that -hm might be the 3 m.du. (or pl. in the cases where there are two tokens of w n̄lh̄m one after the other) pronominal suffix, stating that the third-generation heir is not simply the descendent of his father but of his grandfather as well (and, in the case of four generations, of his great-grandfather). In that interpretation, n̄lh̄m would be vocalized either /nahaluhumā/ and mean ‘and their heir (they = dual)’ (i.e., the heir of his father and his grandfather) or /nahaluhumu/ ‘and their heir (they = plural)’ (that is of his father, his grandfather, and his great-grandfather).
— p. 833 (§89.3). T. refers only to Aartun’s1377 and his own1378 treatments of the Ugaritic particles as having given space to enclitic -y. Though certainly not to be classed with these extensive studies, my mention of the phenomenon in a précis of Ugaritic grammar, prepared independently of T.’s study, may, by the nature of the work in which it appeared, have brought the existence of the category to the awareness of a larger audience.1379

— p. 834 (§89.32). T. asserts here that RS 11.772+ (KTU 3.1) “gehört offenbar zur Gattung der Briefe und beruht auf einem akk. Originaltext (RS 17.227+).” In an epigraphic and philological study of the Ugaritic text, I have evaluated the first proposition with an emphatic negative (though RS 11.772+, RS 17.227, and the other Akkadian texts all appear to belong to the same genre, none is epistolary in nature) and the second with a guarded negative (there are too many differences between RS 11.772+ and any of the known Akkadian texts to allow for the former to be a direct translation of one of the latter).1380

— p. 840 (§91.21a). T. does not state why he rejects the editors’ analysis of qdš in the phrase ‘š qdš (RS 92.2014:3 [RSO XIV 52]) as nominal (/‘iši qodshi/, lit. ‘wood of holiness’)1381 in favor of identifying qdš as an adjective—all the more surprising when he makes a point in the introductory paragraph to this section of the fact that the Semitic languages make greater use of the nominal attributive genitive than do the Indo-European languages, where the attributive adjective is more common. Another important consideration is that, with the root QDŠ, the nominal attributive genitive construction is common in Biblical Hebrew, as in the type-expression har qodši, ‘my holy mountain’. Nevertheless, it goes without saying that, in the absence of a vocalized text, it is impossible to know which construction was actually used in any given case. (It is also impossible to know whether ‘š might in fact be a plural construct, reflecting the use of more than one piece of wood in this rite; if such were the case, the construction would certainly be nominal.) But, when in doubt, might it not be best to choose the more commonly attested construction?

— p. 840 (§91.21b). It is strange to find ālp mri listed as one of only two examples of participles used as attributive adjectives when it is not at all certain that mri is a participle. The doubt is not just mine, for in various sections above, T. considers that this form may be a simple adjective, a substantive, or a passive participle (p. 261 [§51.43c], p. 294 [§53.312b], p. 303 [§54.111], p. 476 [§73.426]). Because the comparative evidence speaks for the adjective mri having the form /qatîl/ (as T. classifies it on p. 261), while what little internal evidence there is speaks for the G-passive participle being /qatûl/ in Ugaritic (see remark above to p. 473 [§73.422], etc.), one must conclude that it is unlikely that the phrase ālp mri contains a token of the passive participle that was productive in Ugaritic.

1381 The formal editio princeps of this text was in RSO XIV (2002), but the manuscript was made available to T. in advance.
— p. 841 (§91.23), p. 885 (§95.14), p. 901 (§97.122). T. does not state why he rejects the editors’ analysis of ghrt in RS 92.2014:11 (RSO XIV 52) as verbal and as syntactically independent of the previous unit—except to the extent that the subject of ghrt was stated in the previous unit.1382 T. takes ghrt as an adjective immediately dependent on the preceding phrase (hwt bn nšm, ‘the word of anyone’). The preceding phrase, however, is the last unit of a longer poetic verse, while ghrt begins a new verse; for that reason, the editors saw a syntactic break between the two verses. On pp. 885 and 901, T. takes ghrt as agreeing with hwt, which is described as the “Leitwort im (vorausgehenden) Hauptsatz,” but on both pages takes the following nouns (phm w šptm) as the real subject of the nominal sentence, translating “deren Münner und deren Lippen laut/leer(?) sind.” He does not explain why he chooses to reject the editors’ overall interpretation of the passage according to which ghrt begins a new sentence and the following nouns are in the adverbial accusative: ‘it (the word) resounds in their mouth and on their lips’.1383

— pp. 844-45 (§91.314.1). In an “Anm[erkung]” to the use of the construct chain to express a notion of superlativity, T. cites a series of texts where X + •l is supposed to mean “göttlich, majestätsch, überraschend schön.” One may observe, however, that all of the examples that even approach being qualifiable as clear1384 are from a single passage, RS 2.[008]+ col. 1 (KTU 1.4), e.g., kt il, in line 30, is translated “ein herrlicher Sockel.” An argument can be made, however, that the various items in this list are in the long run intended for ‘Ilu himself and that the construct formulation expresses attribution, just as in the preceding formulations of the type mgn rbt årt ym, ‘a gift of (= for) Lady ≥A®iratu of the Sea’ (line 21).1385 Maximalist lists of the so-called superlative use of various divine names and titles in the Hebrew Bible have also been proposed and contain many examples that are equally dubious. The parade example, ṛwâh ʿelōhîȳm in Gen. 1:2, sometimes translated ‘a mighty wind’, is among the most dubious: how can one believe that a Hebrew speaker on hearing that formula just after hearing that ʿelōhîȳm was creator of heavens and earth would not have understood that the same ʿelōhîȳm was the one who had sent the wind? This does not mean that the wind was not ‘mighty’ but that by, restricting the English translation to that word, the notion of divine origin in the Hebrew expression is left out entirely. Perhaps the modern reader, just like the ancient audience, may be expected to understand that a wind from the creator would by the nature of its origin be a mighty one.

— p. 845 (§91.314.1). T. cites the phrase mlk mlkm as occurring in lines 9 and 10 of RS 34.356 (CAT 2.76) and in lines 1 and 9 of CAT 9.530. The latter “text” is, however, in fact identical to the former (see above, remark to p. 10 [§18.6]). Whatever may be the reason for the creation of the new text CAT 9.530, the reading of the phrase mlk mlkm is equally

1383Ibid.
1384For several examples representing two basic phrases that do not require the interpretation as superlatives, see above, remark to p. 245 (§45.21d), etc.
plausible in lines 1, 9, and 10 of RS 34.356. According to my collation, the exact readings are, respectively: {mlk . m[īl][km]}, {mlk . mlk[m]}, and {mlk [.] lm[lk[m]].

— p. 846 (§91.314.2). The reading {gzrm . ūg1 . ūl} in RS 2.002:14 (KTU 1.23) is much clearer than the transcription {gzrm [[tb]]g . ūl} in CAT has led T. to believe (he here qualifies the reading gzrm g ūl, ‘lads with nice voices’, as uncertain). The {m} of gzrm is not in fact written over “an anticipated ūl” and there is a word-divider between this word and the following g. The reading may be judged certain from context though the bottom of the wedge is damaged with the result that, epigraphically speaking, it could theoretically be {g} or {z}. The syntactic construction is certainly different, therefore, from gzr ūl ql, lit. ‘the lad (who is) nice of voice’, in RS 2.[014]+ i 20 (KTU 1.3).  

— p. 846 (§91.315.2). T. opines that the substantives šmāl and ymn, ‘left/right (hand)’, may function as adjectives in Ugaritic because their syntax is not that of a nomen rectum. Proof for the latter grammatical observation appears clearly in RS 24.247:+:11 (KTU 1.103) one finds [b] pīth šmā[l] (the pronominal suffix would appear on the final word of the phrase if it consisted of a construct chain). His alternative solution, however, viz., that these nouns function as adverbial accusatives, conforms better to known Northwest-Semitic usage (‘in its temple on the right’) and indeed is required in RS 24.247+ since neither šmāl nor ymn ever shows gender agreement with the noun it is modifying.1387

— p. 848 (§91.321c). T. accepts the reading in KTU/CAT of {ṭāt lūl}, ‘ewes for’, in RS 24.255:17 (KTU 1.111:18). Neither of the last two signs are to be found on the tablet, however, and the writing of the first two is not unproblematic.1388

— p. 848 (§91.321c). Bordreuil, in his preliminary publication of RIH 83/22 (CAT 4.779),1389 took l ḫṭb in l. 12 as the negative particle (“n’a pas été compté”); T. here translates it as a purpose clause (“zur (Begleichung der) Rechnung”), with no mention of the editor’s views. Moreover, T. does not translate the purpose clause in context, and I do not see how his analysis could be made to fit in with what precedes.

— p. 849 (§91.321f). T. offers here a reading of RS 17.434+:4 that does not correspond to the transcription in CAT 2.36: in CAT one finds {d . ḫwtk . [w] . dt [.] ‘mlk} whereas T.’s version is (he includes no word-dividers and indicates signs of which the reading is uncertain as restored) {d ḫwtk [w d] b[t mlk[k]}, “deines Landes und [deines](?) Königshau[ses].” Since T.’s readings correspond to those that my collation of the tablet showed to be most likely,1390 I can only prefer his. I have not in the past restored a pronominal suffix after mlk, but such a restoration is certainly plausible. T.’s remark that his readings are “aus dem Akk. übersetzter Text,” might, however, be misunderstood by some, for he is referring to a

1386 CAT, p. 67, n. 2.
1388 Ibid., p. 619 with note 14, p. 621 with notes 17 and 18, and p. 1282.
1389 CRAI 1984, p. 431; see now Bordreuil and Pardee, Manuel (2004), text 52 in the Choix de textes.
1390 AfO 29-30 (1983-84) 321 (with n. 7), 323, 324. The precise transliteration resulting from several collations is {d . ḫw[t]k . lw[l] . d . bt[.] lm[lk[.]}. 

Archiv für Orientforschung 50 (2003/2004) online version
hypothetical original from which the Ugaritic would have been translated, not to any text that has actually been discovered.

— p. 849 (§91.322). Not everyone agrees with T.’s analysis of yd ʾl, ‘hand/love of ʾIlu’ (RS 2.[008]+ iv 38’ [KTU 1.4]), as an objective genitive (‘love directed towards ʾIlu’).\textsuperscript{1391}

— p. 851 (§91.338). The pronominal suffix on mlàkty, ‘my messenger party’, in RS 16.402:35 (KTU 2.33) is more likely expressing a subjective genitive (‘my messenger party, viz., the one that has brought this tablet to you’) rather than an objective notion (“die an mich gerichtete Gesellschaft”). At this point in the letter, the author is requesting a response to his complaints, not referring to the message to which he is responding.\textsuperscript{1392}

— p. 855 (§92.237b). Whatever the exigencies of translation into modern European languages may be, the poetic verse in RS 2.[008]+ iv 16'-18' (KTU 1.4) does not contain two nominal sentences, as T. proposes here, at best only one. The verse appears to consist of a tricolon with a verb in the first colon which is gapped in the second, perhaps in the third as well:

\begin{quote}
qdš yūḥdm šbʾr \\
āmrʾ k kkb l pmn \\
āʾr btlʾ nt
\end{quote}

Qudšu begins to illumine (the way),
(as does) ʾAmruru, like a star, in front,
(while) behind (is) Girl ʾAnatu
or: behind Girl ʾAnatu(also gives light).

— p. 856 (§92.238b). T. analyzes šlm in both parts of the ‘double formula of well-being’ (see above, remarks to p. 197 [§33.322.42a], etc., to p. 246 [§45.23a], and to pp. 737-38 [§81.11a-d], etc.) as nominal. He cites as his example of the first ʾhlny šlm, ‘Siehe, bei mir (herrscht) in vollkommener Weise Wohlbefindung,’’ but the fact that the adverb md(m), ‘very’, can be inserted between kll and šlm (RS 8.315:11-12 [KTU 2.11], RS 18.038:3-4 [KTU 2.39], RS 20.199:12-13 [KTU 2.68]) and the fact that kll can follow šlm rule out the analysis of šlm as a noun: (1) kll šlm cannot be a construct chain (‘there is entirety of well-being’) when an adverb intervenes or when kll follows šlm; (2) kll is not, at least when an adverb appears between kll and šlm, used adverbially with the noun šlm (the use of a double adverbial expression, ‘there is entirely very much well-being’, is unlikely); (3) it is implausible that kll is a noun when it precedes šlm but an adjective when it follows that word. These variations of the formula show that šlm is best taken as a verb, e.g., ‘all is very well’ (/kalšlu maʾda šalima/) and ‘all is well’ (/kalšlu šalima/ and /šalima kalšlu/). On the other hand, the Akkadian formula corresponding to the second element of this double formula regularly contains a noun (šulmānu) and one does not find similar syntactic features in the corresponding Ugaritic formula as those we have just discussed. These differences lead to the conclusion that šlm in the second part of the double formula of well-being, e.g., ṭmn̄y ʾm ūrny mmn šlm w ṛgm ṭb ly, ‘There with my mother, whatever well-being (there may

\begin{footnotes}
\footnotetext{1392}{Pardee, AfO 31 (1984) 216; idem, Context III (2002) 106.}
\end{footnotes}
be), send word (of that) back to me’ (RS 11.872:11-13 [KTU 2.13]), was nominal in Ugaritic.\footnote{On these various questions regarding the analysis of the elements of this epistolary formula, see Pardee, Fronzaroli (2003) 446-75; specifically on the šlm-formulae, see pp. 452-54. T. holds that the corresponding Akkadian formula, with nouns in both parts, shows that šlm in both parts of the corresponding Ugaritic formula was nominal, but he does not attempt to explain why different nouns were used in Akkadian, šulmu in the first part, šalnānu in the second. This distinction may be explained by taking the use of two nominal formations as an attempt to reflect different Ugaritic forms, verbal then nominal. That is, the scribes would have remarked the use of šalima in the first part of the formula, šalamu or šulmu in the second, and reflected that difference by using different nouns, the first most commonly accompanied by the particle lā which emphasizes the predication. The use of a verb in formulae which resemble the Ugaritic phrases in question is clearly visible in the Amarna letters and hence may reflect Canaanite usage (see Pardee and Whiting, BSOAS 50 [1987] 7) and is attested in epistolary usage in Old Babylonian as well (ibid., p. 20-22).}

— pp. 861-69 (§93.3). Readers unacquainted with modern German scholarship on verbal valence, viz., whether a verb takes zero, one, or two complements, must be aware that T. uses the concept to describe only surface phenomena: a G-stem intransitive verb, a G-stem transitive verb with unexpressed direct object, and a D-stem factitive verb with unexpressed direct object are all univalent; a Š-stem causative verb with only one expressed direct object is bivalent. This approach has its uses but the concentration on surface phenomena sometimes leads to passing over important underlying phenomena (see following remark and remark to p. 864 [§93.33c]).

— p. 863 (§93.33a). Though at one level one cannot fault T. for considering suffixal complements to transitive verbs that function as direct or indirect objects as examples of ‘trivalent’ verbs (identified as transitive verbs that take two nominal or pronominal complements neither of which is introduced by a preposition), for he is describing a single surface phenomenon, there are reasons why it might nevertheless have been worth his time to separate out the two grammatically distinct entities. First, the oldest attested Semitic language, Akkadian, had a set of dative pronominal suffixes distinct from the accusative set; for someone who bases so much of his analysis of Ugaritic on comparative phenomena, T. might have taken the examples of the indirect object expressed by a pronominal suffix (e.g., åtnk, ‘I will give to you’) as a relic of an earlier stage of the language, especially since the certain attestations of this syntagm are found only in poetry. Second, on a purely statistical level, such structures are extremely rare, for indirect objects are normally expressed by prepositional phrases; the rarity of the phenomenon is basis enough for treating it separately. Finally, since in his introduction to this section he speaks of these two categories, he certainly should have presented separately all examples of nominal accusative complements that express an adverbial notion rather than a direct object complement.\footnote{In his remarks to various examples, he sometimes refers to “freie Adverbiale Angabe,” with which I agree fully, but also to the locative case, for which he has made a greater place than is to be found in any previous grammar (on my hesitancy regarding such broad acceptance of this grammatical category, see above, remark to pp. 326-35 [§54.4]).}

Once again, one cannot quarrel with the listing in one place of all accusative complements, but that is not how he introduces the paragraph and, in any case, it is not reprehensible on the part of a
grammarian to describe sub-categories of a single overtly marked grammatical category—T. does it frequently in this grammar (and I have sometimes criticized him for ascribing different “functions” to these sub-categories). Because there are usually clear grounds for distinguishing the two (see further below, remark to p. 864 [§93.33c]), it would have made the structure of the language in this respect much clearer to his readers to have presented the data separately.

— p. 861 (§93.33a). I find it remarkable that a grammarian as alive to enclitic -m as T. would base his analysis of HLM, ‘to strike’, as taking a double accusative in RS 3.367 iv 14'-15', 16'-17' (KTU 1.2) on the form ydm (parallel to ‘nm in lines 22' and 25'). It appears equally plausible to see in each colon a single accusative phrase, i.e., ydm and ‘nm would be in the construct state with enclitic -m (quoted is the first verse of each poetic section):

\[
\begin{align*}
\text{hlm ktp zbl ym} & & /hulum katipa zabûli yammi/ & \text{Strike the shoulder of Prince Sea,} \\
\text{bn ydm [tp]t nhr} & & /bêna yadêma ūpîtî nahari/ & \text{twist the hands of Ruler River.}
\end{align*}
\]

\[
\begin{align*}
\text{\ldots} & & \ldots \\
\text{hlm qdqd zbl ym} & & /hulum qudquda zabûli yammi/ & \text{Strike the pate of Prince Sea,} \\
\text{bn ‘nm tpt nhr} & & /bêna ‘ênêma ūpîtî nahari/ & \text{twist the eyes of Ruler River.}^{1395}
\end{align*}
\]

One could further argue that, even if the construction was a double accusative, the body part was an ‘accusative of respect’, not a direct object (T. is in this paragraph treating the latter topic): ‘Strike Prince Sea on the shoulder, Ruler River between the hands’.\(^{1396}\) The analysis of the structure as containing two accusatives—of either type—would be more plausible if the word designating a body part did not each time precede the name of the deity being struck.

— p. 863 (§93.33b). There is nothing at all “offenbar” about T.’s analysis of yqr dbḥ ilm (RS 3.322+ iv 22-23 [KTU 1.19]) as a double-accusative construction, ‘he causes the sacrifice to reach the gods’. Attributions may be expressed genitivally in all the old Northwest-Semitic languages, and such may be the form of this phrase: ‘He brings the sacrifice(s) of the gods’.\(^{1397}\)

— p. 864 (§93.33c). Though causative-stem verbal forms certainly provide the primary examples of true double accusatives (i.e., X causes Y to perform Z-act on M-entity), one of the accusatives in some of T.’s examples consists of an adverbial rather than a direct object. The primary criterion by which to determine the distinction is the morpho-semantic type of the G-stem, viz., whether it takes a direct-object complement or adverbial complements (adverb, prepositional phrase, or adverbial accusative): in the former case, both complements of the Š-stem may be expected to be direct objects whereas, in the second, one will be an adverbial accusative. Examples: LBŠ is probably stative (cf. /lābēš/ in Biblical Hebrew) and šlbš does not, therefore, mean ‘to cause someone to wear a garment’, but ‘to clothe someone in/with a garment’; ‘RB, ‘to enter’, is a verb of movement and a complement


\(^{1396}\)This analysis appears required for RS 3.340 iv 22’ (KTU 1.18) hlmn ūm qdqd, ‘Strike him twice on the pate’, where the parallel to qdqd is ‘l ṭudm, a prepositional phrase (‘above the ear’).

\(^{1397}\)Pardee, Context I (1997) 354.
stating where the entering takes place is doubtlessly adverbial rather than expressing a second direct object. (For the case of HLK, see remark above to p. 589 [§74.622.3], etc.; for that of QRB in the D-stem, see remark above to p. 551 [§74.412.26], etc.) Though the marking of these two categories may be identical, viz., the accusative case, the morpho-semantics of the basic form and the fact that the adverbial accusative is interchangeable with another form of expressing the adverbial notion require distinct grammatical descriptions of complementation of the derived form as well. The upshot of this discussion and those to which cross-references are provided above is that, of T.'s seven examples of the double-accusative construction, three may be explicitly classified as expressing one direct object and one adverbial accusative (ŠHLK, ‘to cause X to go to Y’, ŠLBŠ, ‘to cause X to be clothed in/with Y’, and Š’RB, ‘to cause X enter into Y’), two are clear examples of the double accusative in the traditional sense of the term and both direct objects are in each case expressed in the text (ŠSPR, ‘to cause X to count Y’1398 and ŠPQ, ‘to cause X to obtain Y’1399), and one is theoretically likely, but uncertain because of the state of the text (šš‘, ‘to cause X to pay Y’—see remark above to p. 598 [§74.624]). I would have to judge that the final example belongs to the first category on the basis of the comparative evidence, but these data are not all that clear and the Ugaritic data are insufficient (I refer to ŠSKN, which may mean ‘to care for X with Y’—see remark above to p. 595 [§74.623], etc.).

— p. 864 (§93.342). In citing the verb-preposition idiom YTN ‘m, ‘to give to’, as appearing in RS 2.[004] vi 18’ (KTU 1.17) and only there, T. notes neither that the reading of the preposition is indicated as uncertain in KTU/CAT nor that Herdner tentatively read a word-divider where the authors of KTU/CAT read {‘} and reconstructed a [l] thereafter. Herdner’s eye and instincts were correct, for the reading does indeed appear to be [l. l]: though both signs are damaged, the preserved traces of writing correspond much better to these signs than to {‘m}. Thus the only case of YTN ‘m in the sense of ‘to give to’1400 disappears.

— p. 865 (§93.343b). Though the problem is less acute in Ugaritic than in Biblical Hebrew, T. might have addressed explicitly the cases of verbs that take both an accusatal complement and a prepositional complement, with little apparent difference in meaning, e.g., BHR + ‘et or b in Biblical Hebrew, both translatable as ‘X chooses Y’. The Ugaritic example that incites this remark is ‘HDb, ‘to seize’, concerning which T. notes “‘Hd wird sonst transitiv konstruiert.” This case is fairly easily explained because one also finds the construction ‘HDb + acc. + b, which means ‘X seizes Y by his/her Z’. The idiom with ‘HDb + b is attested only with b preceding a word that denotes a part of the body, and one may

1398dašprk ‘m b’l šnt (RS 2.[004] vi 28–29’ [KTU 1.17]), ‘I will cause you to count years with Ba’lu’.

1399špq ilm krn ynt (RS 2.[008]+ vi 47, with more examples in the following passage [KTU 1.4]), ‘he provides the gods with rams, with wine’, lit. ‘he causes the gods to obtain rams, wine’ (on the interpretation of the passage as a whole, see remark above to p. 298 [§53.331.2], etc.).

1400For that verb-preposition combination with the direct objet pnm, ‘face’, in the idiom meaning ‘to head off in the direction of X’, see Pardee, UF 7 (1975) 352.
conclude that the expression is elliptical for ‘X seizes (Y by his/her) Z’. If T. accepts this explanation, he might have made that clear; if not, he should have given his reasons.

— pp. 866-68 (§93.351). In this section are listed verbs that are used with Ø complement, an accusative complement, and/or a prepositional complement. I cannot say why so much space was devoted to this topic, for just about any transitive verb in any language that I know can be used ‘absolutely’, i.e., without a stated object, or with a direct-object complement, or with a prepositional phrase expressing an indirect or a quasi-indirect object. E.g., BRK, ‘to pronounce a blessing’, ‘to bless someone’, ‘to bless someone to a deity’; DBH, ‘to sacrifice’, ‘to sacrifice a beast’, ‘to sacrifice a beast to a divinity’, etc. What this section does, then, is provide many details on such distributions of complementation to transitive verbs in Ugaritic.

— p. 867 (§93.352). Because it is a standard rule for understanding prose as well as poetry in Biblical Hebrew that a pronominal direct object may be omitted if the meaning of the phrase is clear from context, one may doubt that such omissions in Ugaritic were a feature primarily of poetry and owing to ellipsis in parallel verse-segments. T. himself cites a case from prose (RS 34.124:31 [CAT 2.72]), and one might expect more prose attestations if there existed a corpus of narrative prose similar in size to that of narrative poetry. The relatively high number of cases in RS 2.002 (KTU 1.23) lead one to speculate, moreover, that this omission was a stylistic feature that would vary in frequency from one poet/narrator to another.

— pp. 874-76 (§§93.423-.424). In these two sections, T. identifies with more or less hesitation some epistolary formulae as translations from Akkadian because they show verb-final word order (e.g., ìlm l ñlm t̀rkm, ‘may the gods guard you for well-being’ [RS 92.2005:7-8 [RSO XIV 49]] or l p‘n X qlt, ‘at the feet of X I fall’). Because there is very little evidence for direct Akkadian influence on the formation of the Ugaritic epistolary formulae, however, and because variations on the prostration formula are well attested in Ugaritic poetry, one must doubt any direct influence from Akkadian in these cases. Inner-Ugaritic topicalization of the word judged to be most important in the sentence is a sufficient basis to explain these highly stilted formulae. Interestingly enough, T. does not identify the much commoner simple formula ìlm t̀rk, ‘may the gods guard you’, as a translation from Akkadian (p. 872 [§93.422.1]), a lack of identification with which I fully agree. But one must ask whether it is legitimate to identify the addition of l ñlm as calqued on the Akkadian (‘calque’ must, in any case, be considered a better term than ‘translation’) simply because it is frequent in Akkadian but attested only this once in Ugaritic.1403 Is the letter of a son writing home to his mother and father the most likely place to find an erudite imitation of Akkadian usage? Finally and most importantly, because the epistolary formulae in Levantine Akkadian tend to differ from Mesopotamian usage, a case could be made for the claim that the Akkadian used in these areas imitated local usage rather than vice versa — though the

1401 Pardee, ibid., pp. 376-77.
1402 On this interpretation, see remark above to p. 758 (§82.12).
exact origin of each formula is debatable (cf., for example, remark above to p. 727 [§77.35]).

— p. 885 (§95.12). Discussing the personal name *il’nt*, ‘my god is ‘Anatu’, under the category of “Genusinkongruenz” must be considered a misunderstanding of the name, which does not mean ‘my goddess is ‘Anatu’, but ‘my god is ‘Anatu’, i.e., since the masculine form is the less marked of the genders, it is the one used to express divinity in general. Naming one’s child *ilt’nt*, ‘my goddess is ‘Anatu’, would leave open the question ‘who, then, is your god?’ whereas *il’nt* precludes such a question.

— p. 887 (§95.231). Neither of T.’s two examples of a singular verb with a dual subject proves the point: *y’n* in the phrase *y’n gpn w úgr* (RS 2.[022]+ i 11 [KTU 1.5]) may in fact be dual (see remark above to p. 438 [§73.223.41.2], etc.) and the reading of the word *y’n* in RS 2.[014]+ iv 5 (KTU 1.3) is probably incorrect (see other remark above to p. 438 [§73.223.41.2], etc.). With regard to the first text, it may furthermore be remarked that double divine names sometimes take singular modifiers, sometimes plural modifiers. Is sufficient complementation of *gpn w úgr* attested to allow certainty that this name is always treated as a dual? In §95.233 (p. 888), T. considers singular agreement to be normal with double divine names. But one must ask why, if Kôtharu-wa-‡as≠su take(s) both forms of agreement, would Gapnu-wa-‘Úgru not qualify for the same status?

— p. 887 (§95.232). All of T.’s examples of a singular verb with multiple subjects, some of which may be morphologically plural, are taken from the hippiatric texts, where he analyzes all the verbs as passive in voice. As we have seen above (remark to p. 511 [§74.222.2], etc.), all these forms may be active, 3d person, singular or plural, with as objects the nouns that T. takes as subjects. Though T. cites an example of absence of number agreement in an Amarna text, it is difficult to admit the existence of the feature in Ugaritic on the basis of such weak evidence.

— p. 889 (§95.44). T. claims that the use of the singular in speaking of several persons each of whom has a single body part (the example is ‘head’) is only sporadically attested and constitutes logical number disagreement (lit. ‘the plowmen lift their head’). Neither here nor above, p. 289 (§53.14), a similar though less theoretically oriented section, is the similar behavior of the dual mentioned. One must object to both facets of the formulation. Though the examples are too few to be certain of the distribution in Ugaritic, it is the rule, rather than the exception, for the singular, and even more so the dual, to be so used in Biblical Hebrew: the plural of ‘hands’ is not used when the reference is to the hands of a group of living individuals. Including the dual in the formulation also shows quite clearly that this form of agreement is in no way illogical. To the contrary, it is just as logical as T.’s preferred logic: since each person of the group has only one head and two hands, it would be illogical to add them all up as though they were discrete entities and express them higgledy-piggledy as plurals. (Note that in Hebrew body parts are expressed as plurals when they are separated from the body and become discrete entities: II Kings 2:6-7). It is a common feature of the old Northwest-Semitic languages, as well as of many modern languages, to use singular/dual formulation of distributives, though the reality of the plurality can also be

expressed. In English we regularly express the plurality (‘they put their hats on their heads’) whereas in French the singular distributive form is common (‘ils mirent leur chapeau sur la tête’, ‘mettez vos mains dans la poche’) though the real plurality may also be expressed, but usually not in the case of a single body part (‘ils mirent leurs chapeaux sur la tête’).

— p. 895 (§96.28). It is highly unlikely that the two entries beginning with k in RS 19.015:10-11 (KTU 1.91) are to be associated under a single cultic heading as T.’s classification as an example of “Asyndese von Nebensätzen” assumes, for the first (k t’rb ‘ttrt šd bt mlk) is attested as a discrete entity in RS 24.643:18-22 (KTU 1.148). It is moreover possible, though less clear, that the second entry (k t’rbn ršpm bt mlk) corresponds more or less directly to another rite which would be only partially preserved on the second of two tablets (RS 24.250+ [KTU 1.106]).

— p. 896 (§96.311). Quite remarkably T. compares the “Koppelung” of two finite verbal forms, where the first has the translation value of an adverb, e.g., ḨWŠ, ‘to do something quickly’, with Akkadian but not with Biblical Hebrew, for Hebrew has a whole series of these verbs. They were classified by Lambdin as “hendiadys,” were appropriately grouped, and their different morpho-syntactic structures clearly laid out. T. might have given heed especially to this latter facet of their usage because he describes the phenomenon uniquely in terms of parataxis, e.g., hš bhtm tbnn, ‘quickly build a palace’. One finds this structure in Hebrew, but others as well, with no apparent fundamental difference in meeting, viz., (1) parataxis of precisely identical forms (e.g., hōw ṭēl qaḥ, ‘please take’, II Kings 5:23, two imperatives); (2) identical forms joined by w (e.g., hōw ṭēl ābārēk, ‘please bless’, II Sam. 7:29, two imperatives joined by w); (3) the verb in question followed by a ‘wāw-consecutive’ structure (e.g., wʾlūw hōw almāw wannēšeb, ‘if only we had been content to stay’, Josh. 7:7); (4) the verb in question followed by l and an infinitive construct (e.g., hōw ʾaltīy ṣdabbēr, ‘I have dared speak’, Gen. 18:28). It is the similarity of meaning across these different syntactic structures that is of interest in terms of T.’s classification of the Ugaritic forms, for it makes clear that the function of this class of verbs is not to be linked with any single syntax. This may be taken as at least an indication that a heading broader than ‘parataxis’ might have been found for the Ugaritic phenomenon. At the very least, an explicit comparison with the Hebrew usage would have been in order.

— p. 896 (§96.312). This paragraph provides an example of T.’s maximalist approach: the correlation of the D-stem of ŠLM and NGR, ‘to keep well’ and ‘to guard’, in the epistolary blessing formulae is hesitantly placed under the following classification: “Zwei Verben bilden bedeutungsmäßig eine Einheit (‘Hendiadyoin’).” T. himself brings forward one argument against that classification, viz., that the two-verb sequence may be expanded to three (he cites the case of ‘ZZ D-stem, ‘to strengthen’; in addition to expansion by this verb, Bordreuil and I have proposed that √TMM D-stem, ‘to keep in perfect condition’ was so used in RS 92.2005:28 [RSO XIV 49]). Two other arguments go unmentioned here, though

1406 Introduction to Biblical Hebrew (1972) 238-40 (§173).
T. is in each case well aware of the phenomena: the order of the two verbs is not fixed (šlm – NGR and NGR – šlm are both well attested) and an element may be inserted between them, namely l šlm, ‘for well-being’. It is hardly plausible that an epistolary formula that shows so much variety is properly described as expressing a semantic “Einheit.” —pp. 904-5 (§97.4). None of T.’s examples of subordinate clauses with a modal function is convincing and the section should be eliminated. §97.41 deals with “Komparativsätze,” for which, in spite of the plural in the title, a single example in two tokens is proposed (RS 2.[009]+ ii 6-9, 28-30 [KTU 1.6]); but T. himself considers the analysis as a subordinate nominal clause unlikely (see remark above to p. 762 [§82.13], etc.). §97.42 is devoted to “Andere Modalsätze,” but neither of the analyses proposed here proves the existence of the grammatical category: §a deals with a formula that appears frequently in letters of which the protasis is not a modal expression (T.’s interpretation is criticized in detail in the following remark); §b deals with a single phrase in a broken context of a letter (RS 1.026+ [KTU 2.7]) that may be an independent interrogative sentence rather than a modal subordinate clause (see above, remark to pp. 716-17 [§76.535a-c], esp. note 1271).

— p. 904 (§97.42). I agree fully with T. that mnm šlm in the epistolary formula that runs mnm šlm ‘m X rgm TTUB ‘my,’ whatever well-being there may be with X, (may X) send back word (of that) to me’, does not constitute an independent sentence, as some have held, but a subordinate clause (see remark above to p. 244 [§45.122b], etc.). I cannot see, however, that it has any modal value. T.’s expanded translation is “Hinsichtlich (der Art und Weise bzw. des Ausmaßes) des Wohlbefindens schicke eine Nachricht zurück.” I would not wish to deny that the writer wishes to know ‘how’ the addressee is, but, at the surface level, he is simply asking that the addressee return word of his/her well-being. This is indicated by two important variations on the formula: (1) when the writer asks for return of news regarding a specific situation, in the sentence mnm rgm d tšm’ tmt w št b spr ‘my,’ Whatever word you hear there, put (it) in a letter to me’ (RS 4.475:16-19 [KTU 2.10]), rgm in the casus pendens is the implied direct object of št in the main clause; (2) in the expanded form of the return-of news-formula found in RS 92.2010:12-20 (RSO XIV 50), the noun rgm is omitted from the main clause and it is the quadruple šlm that is the direct object of the verb: b’ty šlm h1 w šlm nkly w šlm bth w šlm šm’ rgmk n’m át tšb ‘m ‘bdk, ‘As for my lord, his well-being and that of Nikkalaya and the well-being of his household and the well-being of those who listen to your good word may you return to your servant (i.e., please return word of that well-being)’. These variants show that T.’s analysis of the casus pendens in the standard formula as having an adverbial function (‘as regards your well-being’) is correct because the verb of the main clause has another explicit direct object, rgm; but they also make it dubious that the use of mnm in the casus pendens constitutes an expression of modality. In Ugaritic, one did not ask ‘how’ someone was, one asked for his ‘well-being’ or

1408 On this element, see above, remark to p. 758 (§82.12).
1409 See the French rendition in the editio princeps (RSO XIV 50) and my English translation of the full text in Context III (2002) 112-13; see also Bordreuil and Pardee, Manuel (2004), text 30 in the Choix de textes. On the casus pendens structure of this passage and the shift from 3d to 2d person (the latter a common feature of letters), see T., p. 883 (§94.21), p. 890 (§95.43).
for a report thereon. All that *mn̄m* adds to the request is an expression of completeness: ‘report to me whatever forms of well-being characterize your life.’ I am dubious that this constitutes a grammatical expression of modality. To have categorized the Ugaritic return-of-news formula as explicitly modal, T. appears to have permitted himself to be influenced by the psychology reflected in our modern expressions, which consist of a neutral statement that allows explicitly for a positive or a negative response (‘Wie gehts?’, ‘Comment ça va?’, ‘How are you?’), and to have infused *mn̄m* with the same degree of neutrality rather than taking the positively marked  *šlm* at face value.

— p. 906 (§97.71a). With a question mark, T. interprets *āṯlmṯūtk tizz pn̄m* in RS 24.261:9 (*KTU* 1.116) as consisting of a main clause and an asyndetic circumstantial clause: “ein *āṯlm*-Opfer für Šauška, wobei das Gesicht zu verhüllen(?) ist.” But he has failed to notice that, in this bilingual text, the attribution of sacrifices to a divinity is expressed explicitly by the Hurrian dative morpheme attached to the divine name. Above, in line 3, the attribution of the *āṯlm*-sacrifice to  Ṭa‘uṭka, is so expressed, *āṯlmṯūkḍ*. Below, in lines 10-11, there is a mixed Ugaritic-Hurrian sentence similar to the one in lines 8-9 and there the divine name bears the dative morpheme. The absence of -d on *p̄zm* in line 9 means that the *āṯlm*-sacrifice is not explicitly ascribed to her there and that the divine name functions as the subject of the following verb.

Typographical errors:
— p. 69, §21.412g, line 5: “bd.pdr 4.269:7” appears twice.
— p. 125, §32.146.313  *gr*: “4.17” should be ‘4.27’.
— p. 139, §33.112.36Anm., line 4: the last sign of the PN “agṭṭb” according to CAT 4.320:3 is *p*.

---

1410 The limitation of the question to well-being is also characteristic of the later West-Semitic languages: though Š’L (+ l) +  *šlm*-X is the equivalent of our ‘asking how someone is’, on the surface level it consists of ‘asking (regarding) the well-being of someone’. The first explicit expression of uncertainty regarding the quality of the addressee’s well-being of which I am aware in inscriptions is in the Phoenician letter KAI 50 where, in line 2, w  *šlm* ’t must in context be taken as a question and mean ‘Are you in a state of well-being?’ (cf. Pardee, *et al.*, *Handbook* [1982] 167). In the Hebrew Bible, the inquiry after someone’s health may also be expressed interrogatively:  *ḥāšālōm* ’at, ‘Are you in a state of well-being?’

1411 Ll. 8-9:  w l b bt *āṯlmṯūk tizz pn̄m*, ‘And in the house/temple, sacrifice; as for Ṭa‘uṭka, you are to veil her face’; ll. 10-11:  w b bt *āṯlm intṯ tmttn*, ‘And in the house/temple, sacrifice for the gods TAILANN’.

1412 See Laroche, *Ugaritica V* (1968) 500, 502, for the hurritological viewpoint.


1414 “Should be” in this and following entries represents the correct notation according to T.’s conventions which are not necessarily mine; what is found in his text is enclosed in double quotation marks, the correction in single quotation marks.
The line numbers indicated for the ‘polyglot vocabulary’ RS 20.123+ often differ from those of the editio princeps (Nougayrol, Ugaritica V, text 137. pp. 240ff.); the first example I noted was on p. 144, §33.115.42, last line: "II:6' is found in col. II, line 12'.

— p. 171, §33.213.1a, line 4: the entry said to appear in “II:4’ ” is found in line 10'.
— p. 171, §33.213.1a, line 7; p. 301, §53.34, line 10: the entry said to appear in “I:3’ ” is found in line 20'.
— p. 188, §33.311.1a, line 3: the entry said to appear in “III:7’ ” is found in line 32'.
— p. 188, §33.311.2a, line 2: the entry said to appear in “I:3’ ” is found in line 9'.
— p. 251, §51.41a, lines 10-11; p. 278, §52.11, line 4: the entry said to appear in “IVb:18’ is found in line 17; because the lemma for each entry is “ba-a-lu(-ma), examples should have been cited for the writing with [-ma], e.g., col. II, lines 30’ and 33’ of this text.
— p. 251, §51.41a, line 18: the entry said to appear in “II:30’ ” is found in line 26'.
— p. 252, §51.41a, line 2: the entry said to appear in “IVb:15’ is found in line 14.
— p. 252, §51.41a, line 9; p. 303, §54.111, line 2: the entry said to appear in “II:13’ ” is found in line 19'.
— p. 252, §51.41a, line 31: the entry said to appear in “I:3’ ” is found in line 9'.
— p. 254, §51.41c, line 8: the entry said to appear in “I:5’ ” is found in line 11'.
— p. 254, §51.41c, line 14; p. 303, §54.111, line 4: the entry said to appear in “III:4’ ” is found in line 29'.
— p. 254, §51.41c, line 17: the entry said to appear in “I:4’ ” is found in line 10'.
— p. 257, §51.41g, line 2: the entry said to appear in “II:4’ ” is found in line 10'.
— p. 266, §51.45e, line 5: the entry said to appear in “II:15’ ” is found in line 21'.
— p. 266, §51.45e, last line: the entry said to appear in “III:7’ ” is found in line 32'.
— p. 303, §54.111, line 7: the entry said to appear in “II:6’ ” is found in line 12'.
— p. 149, §33.115.9Anm., last line: the cross-reference to “§74.232.2a” should be ‘§74.232.21’ (see p. 521).
— p. 158, §33.141.42, line 5: ditto.
— p. 171, §33.212c, last line: in CAT 2.31, ib’r is found in line 55, not “54.”
— p. 172, §33.212.2, line 5: “1.23:2” should be ‘1.23:6’.
— p. 175, §33.215.21b utb, line 2: “4.337:1” should be ‘4.337:11’.
— p. 188, §33.311.2aAnm., line 1: “RS 20.196” should be ‘RS 20.196A’.
— p. 195. §33.322.1b, line 6: “1.16:I” should be ‘1.16:VI’.
— p. 197, §33.322.42c, line 11: insert double quotation marks after Birt.

The differences cannot reflect another edition of the text, for T.’s numbers sometimes coincide with those of the editio princeps and he sometimes cites the same reference for different entries (see below “I:3’ ” on pp. 171 and 252).

The great number of discrepancies would lead one to believe that T. is using an edition other than the editio princeps, but I know neither of such an edition nor why the numbering of the lines would vary so significantly and in an unpredictable way from the editor’s.
— p. 198, §33.323.1b, line 2; p. 258, §51.42a, line 10: the word pn is not attested in CAT 1.1:II:14.
— p. 200, §33.323.4b, line 6: the syllabic entry ta-bu should be moved up to §a.
— p. 202, §33.421, line 5: “§33.151.1” should be ‘§33.151’ (see p. 162).
— p. 210, §41.112.7, line 5: “2.31:65” should be ‘2.31:63’.
— p. 215, §41.221.11a, last line: “2.71:12” should be ‘2.75:12’.
— p. 216, §41.221.12b, line 1: “§54.133c” should be ‘§54.133.2c’.
— p. 225, §41.222.2b, last line: “3.9:8-7” should be ‘3.9:6-7’.
— p. 255, §51.41c, line 6: “4.15” should be ‘4.14’.
— p. 255, §51.41e, line 8: it is strange to cite hmr as appearing in “4.14:18&” when the word is also found in lines 6 and 12 of the same text.
— p. 256, §51.41e, line 6: no gloss is provided for šb‘.
— p. 257, §51.42a, line 10: “4.272:1” should be ‘4.272:2’.
— p. 265, line 11: the second “51.44m” should be ‘51.42n’.
— p. 268, §51.45k, line 1: “/mappuh/” should be ‘/mappuh/’.
— p. 271, §51.46a, line 9: “9.432:17” should be ‘RS 92.2016:18’ (elsewhere, this text is always cited by its RS-number rather than by the meaningless CAT number—meaningless because §9 was added in CAT to provide a place to list tablets of which the existence was known to the editors but not the text itself).
— p. 275, §51.5a, line 20: “Planzenspezies” should be ‘Pflanzenspezies’.
— pp. 301-2, §53.36: this section has paragraphs “b” through “f,” but no section ‘a’.
— p. 349, §62.182bAnm, line 9: the tablet RS 3.343+ (KTU 1.15) is referred to as a “Stein.”.
— p. 349, §62.192, line 3: “63.18” should be ‘§63.19’.
— p. 379, §65.147c, line 8: “2.40:5-9” should be ‘2.40:5-8’.
— p. 410, §69.223.12b, line 7; p. 417, §69.313.11, line 5: it is strange to cite tt dd š’rm as appearing in “4.14:7” when the phrase is already found in line 1 of the same text.
— p. 413, §69.241, line 6: “4.132:2” should be ‘4.132:3’.
— p. 420, §69.53, line 3: “ksmk” should be ‘kmsk’.
— p. 446, §73.233.9, line 7: insert ‘nicht’ after “auf daß ihr.”
— p. 464, §73.331.1, line 20: it is strange to cite “qra” as appearing in “1.161:5.6.7.11.12” when the first appearance of the form is in line 4.
— p. 467, §73.332.4, line 13: insert ‘5.11:5’ as the text reference for the cited form.
— p. 468, §73.333.1Anm., line 5: contains a cross-reference to this very section.
— p. 475, §73.426, line 9: “1.124.15f” should be ‘1.124:14f’.
— p. 478, §73.431c, line 10: “Pfüger” should be ‘Pflüger’.
— p. 524, §74.232.21 √rrqs, line 3: “1.3:IV:23f” should be ‘1.2:IV:23f’.
— p. 526, §74.232.21 √mnn, line 6 contains a cross-reference to this very section (it should apparently have been to §74.432).
— pp. 537-38, §74.333 √mṣḥ, line 1, √ngḥ, line 1, and √nṭk, line 1: the 3 m.du. ending is each time indicated as /-ũna/.
— p. 547, §74.412.21, four lines from bottom of page: “4.442:2” should be ‘4.422:2’.
— p. 547, §74.412.21Anm., lines 1-2: several words are in Italic script that should be in Roman.
— p. 553, §74.412.27 √kbd, line 7: “1.17:VI:30” should be ‘1.17:V:30’.
— p. 561, §74.414.3 √ğzy, line 1: “*ğazıytumā” should be ‘*gazziytumā’.
— p. 580, §74.512 √rmy, line 1: according to T. reconstruction of the form in line 2 of this paragraph, the vocalization should be ‘râmim’, not “râmim.”
— p. 582, §74.515.1, line 5: “1.12:II:8” should be ‘1.12:II:9’.
— p. 596, §74.624, line 9: “D-PK” should be ‘D-SK’.
— p. 600, §74.626.1, line 3: “Ptz.” should be ‘Inf.’.
— p. 600, §74.626.1, line 5: the Hiphil infinitive absolute in Hebrew is /haqtēl/, not “haqtīl.”
— p. 606, §74.642 √hya, line 5: “/yVšəṭāhwiyu/” should be ‘/tVšəṭāhwiyu/’.
— p. 611, §75.212.11 √py: “3.f.s.” should be ‘3.m.s’ and “1.14:II:3” should be ‘1.14:II:30’.
— p. 613, §75.212.14 √wd: “2.16:19” should be ‘2.26:19’.
— p. 616, §75.222, line 22: “Z. 4” should be ‘Z. 3’.
— p. 617, §75.223 √d’w, line 4: “du” is said to appear in 1.19:III:14 though it was just stated in lines 2-3 of this paragraph that the proper reading there is “tdu”.
— p. 617, §75.224, lines 6, 7, and 13: the repetition of “2.36+” (twice) and “2.4” should be removed.
— p. 621, §75.232, line 3: “[ʾ]l tšî” should be ‘[a]l tšî’.
— p. 621, §75.232 √ml’, line 2: “3.f.sg.” should be ‘3.m.sg.’
— p. 622, §75.235b √ḥ’: the cross-reference to “§73.422.3” should be ‘§73.426’.
— p. 624, §75.32 √hdy, line 2: “/yuhaddiyu/” should be ‘/tuhaddiyu/’.
— p. 633, §75.512 √wd’, line 1: “TV /i/” should be ‘TV /a/’.
— p. 635, §75.512 √ytn, line 2: “the form ytn does not occur in “1.1:II:14.”
— p. 635, §75.512 √ytn, line 7: “the form ttn is entirely reconstructed in 1.18:IV:5.”
— p. 635, §75.513 √wd’, line 3: “/da’/” should be ‘/da’/’.
— p. 639, §75.517 √wld, lines 1-3: “the entire paragraph is repeated.”
— p. 640, §75.518Anm., line 2: “§51.45i” should be ‘§51.45w’.
— p. 643, §75.522 √bw’, line 3: “*tabû” should be ‘*yabû’.
— p. 643, §75.522 √bw’, lines 7-8: “1.15:IV:21” is repeated.
— p. 643, §75.522 √bw’, line 9: “/taba’ā/” should be ‘/taba’ā/’.
— p. 646, §75.522 √šyt, line 8: “the form tšt is entirely reconstructed in 1.101:16.”
— p. 648, §75.524 √nwr, line 2: “3.m.sg.” should be ‘3.m.pl.’.
— p. 649, §75.527a √dwk, line 2: “1.85:2” should be ‘1.85:3’.
— p. 652, §75.527g √twb, line 2: “/ṭatib/” should be ‘/ṭatib/’.
— p. 652, §75.527g √twb, line 3: “/ṭatibî/” should be ‘/ṭatibî/’.
— p. 652, §75.527h √qyl, line 2: “1.23:11” should be ‘1.23:10’.

Archiv für Orientforschung 50 (2003/2004) online version
— p. 652, §75.527i $\sqrt{kwn}$, line 2: “/yVštakînu//yVštakînu//— p. 659, §75.532 $\sqrt{hdy}$, line 3: “/tuhaddiyu/” should be ‘/tuhaddiyu/’.  
— p. 663, §75.533 $\sqrt{mîy}$, line 2: “1.124:15” should be ‘1.124:14’.  
— p. 663, §75.533 $\sqrt{iîy}$, line 2: “/iîy/” should be ‘/iîy/’.  
— p. 668, §75.537d $\sqrt{hy/\,Wy}$, line 6: “a$h$w” should be ‘a$h$w’.  
— p. 670, §75.537g $\sqrt{hy/\,Wy}$, line 2: “/yuštahwiyu/” should be ‘/yVštahwiyu/’.  
— p. 670, §75.537g $\sqrt{hy/\,Wy}$, line 3: “/yVštahwiyu/” should be ‘/tVštahwiyu/’.  
— p. 676, §75.64 $\sqrt{v/\,zz}$, line 2: correct the ordering of “1.6:VI:17(2x).20(2x).18.19.”  
— p. 676, §75.65b $\sqrt{br}$, line 3: “§73.422.3” should be ‘§73.426’.  
— p. 686, §76.323, line 3: insert quotation marks at end of translation.  
— p. 687, §76.331, line 19: “1.17:VI:33” should be ‘1.17:VI:38’  
— p. 691, §76.343a, last line: “§97.11.2” should be ‘§97.11b’.  
— p. 692, §76.343b, line 3: ditto.  
— p. 692, §76.343c, last line: “§97.11.2-3” should be ‘§97.11b-c’.  
— p. 692, §76.344, line 18: “1.2:II:4f” should be ‘1.2:III:4f’.  
— p. 699, §76.423, line 4: “/yVštakînu/” should be ‘/tVštakînu/’.  
— p. 703, §76.512.1, line 6: “/yVštakînu/” should be ‘/tVštakînu/’.  
— p. 709, §76.524.41, line 22: square brackets should be inserted at various points to indicate the damaged nature of the quoted text.  
— p. 711, §76.524.44, line 7: “§73.223.33” should be ‘§73.223.33.2.’  
— p. 712, §76.524.6a, line 8: insert ‘[n.L.]’ after “1.18:I:19f.” ([ttb’] is not in CAT).  
— p. 716, §76.541, line 6: “aber” is repeated.  
— p. 716, §76.541a, line 8: “Verpflichtung” should be ‘Verpflichtung’.  
— p. 723, §77.323, last line; p. 724, §77.324bAmm., line 2: “§97.102b” should be ‘§97.102b’.  
— p. 725, §77.33, line 18: “/šîtā/” should be ‘/ašîtā/’.  
— p. 726, §77.34cAmm., line 1: “1.23:38” should be ‘1.24:38’.  
— p. 740, §81.12e, line 3: “als” is repeated.  
— p. 742, §81.21a, line 2: insert ‘und’ after “Poesie.”  
— p. 759, §82.12, last line: “b’id” should be ‘aṭr’.  
— p. 764, §82.31, line 13: “mlykm” should be ‘mlakm’.  
— p. 765, §82.33: “§82.214” should be ‘83.214’.  
— p. 772, §82.312, line 26: “1.3:IV:41-43” should be ‘1.3:IV:41-42’.  
— p. 785, §83.113a, line 5: “die die” (twice) should be ‘(die) die’ (the relative pronoun is not present in the text being translated.  
— p. 790, §83.122g, line 5: “2.72:22f” should be ‘2.72:20-23’.  
— p. 791, §83.131, line 10: “mk b b” should be ‘mk b ṣb’.  
— p. 791, §83.131, line 10: insert “\” for each line division  
— p. 791, §83.131, line 15: “1.17:VI:30-32” should be ‘1.17:VI:30-33’.  
— p. 807, line 2; p. 816, §87.23, line 2: “1.6:VI:25f” should be ‘1.6:VI:25f’.  
— p. 847, §91.321b, line 3: insert “\” between ḳmṣṭ and pwt.
In sum, while recognizing that T.’s organization of Ugaritic grammar is far and away the best, the most complete, and the most linguistically sophisticated treatment currently available, the number of debatable points as well as a significant number of epigraphic faults and a not insignificant number of grammatical decisions that I would consider faulty or at the very least unwise keep me from being more enthusiastic about it than I am. One important question regarding this very serious work of scholarship is whether its weight will confound the detractors of presenting Ugaritic texts and grammar in vocalized form or whether the many debatable points included herein will create even more dubiety towards the procedure than has been voiced hitherto. The fact that this work is firmly founded in comparative Semitics cannot in and of itself override such doubts, for any given case that is decided entirely or even principally by comparative considerations must by its very nature remain within the domain of speculation until internal data are attested to confirm or deny the hypothesis. From the above remarks, it should be clear that T. often prefers a reconstruction favoring archaic Semitic whereas it appears to me that one based on a triangulation with Hebrew and Arabic (and Phoenician and Aramaic where possible and relevant) is often preferable. As was remarked in the introduction, perhaps what was needed before this study, which is in many respects an historical grammar of Ugaritic, would have been a less speculative descriptive grammar that treated the data as completely as this one. T. could then have devoted his considerable erudition as a comparative Semitist to a historical/comparative grammar addressed primarily to specialists in Ugaritic and in comparative Semitics, one that would have been less likely to be considered canonical and thereby to have an effect upon even the more casual students of Ugaritic. As it is, one can foresee generations of students becoming confused by the many inconsistencies encountered in this grammar and believing, to choose three egregious examples, that Ugaritic was the only West-Semitic language to have three energetic morphemes, that the {-h} adverbial morpheme was a true case marker, or that the D-stem was really /yuqattal-/ in spite of attested /ʔaqattal-/.

DENNIS PARDEE

University of Chicago